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S. I.
U.S. National Museum
S. I.
Baldwin

Nature

A WEEKLY

ILLUSTRATED JOURNAL OF SCIENCE

VOLUME XXVIII

MAY 1883 to OCTOBER 1883

"To the solid ground
Of Nature trusts the mind which builds for aye."—WORDSWORTH

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NATURE

A WEEKLY ILLUSTRATED JOURNAL OF SCIENCE

*"To the solid ground
Of Nature trusts the mind which builds for aye."*—WORDSWORTH

THURSDAY, MAY 3, 1883

LIFE OF SIR WILLIAM ROWAN HAMILTON

Life of Sir William Rowan Hamilton, Andrews Professor of Astronomy in the University of Dublin, and Royal Astronomer of Ireland; including Selections from his Poems, Correspondence, and Miscellaneous Writings. By Robert Perceval Graves, M.A., Sub-Dean of the Chapel Royal, Dublin. Vol. I. pp. 692. (Dublin University Press Series, 1882.)

WE are glad to welcome the appearance of the first volume of this work, which has long been eagerly watched for by those interested in the career of the wonderful genius whose life is here narrated. To many readers this volume will afford material for no little surprise. Sir William Rowan Hamilton is known to fame as a mathematician. He is known by his memoirs on systems of rays; by his discovery of the great dynamical generalisation which is implied in his theory of the characteristic function; by his exquisitely beautiful prediction of the phenomena of conical refraction; and above all by his theory of quaternions—an imposing mass of profound thought which must be ranked with the very greatest mathematical achievements of any age or nation. Yet here we have a very portly volume of almost seven hundred pages, of which only an extremely small fraction is devoted to Hamilton's mathematical work. The progress of his papers on rays is here and there referred to, and there is an interesting historical chapter on conical refraction, but we may turn in vain to the index for a reference to quaternions, and we have only noticed the word occurring once or twice in the entire volume. But the surprise will disappear when the reader begins to make acquaintance with the volume. He will then see that Hamilton's mathematical labours were only one of the forms in which his most extraordinary genius was manifested. He will see that the early years of Hamilton's life afforded such copious materials to a biographer that the present volume only extends to the time when Hamilton had attained the age of twenty-seven, and that the crowning achievement of quaternions by which Hamilton is best known was the fruit of his riper years, and belongs to his subsequent career.

At the Cambridge meeting of the British Association in 1833, Prof. Sedgwick spoke of Hamilton—then twenty-eight years old—as "a man who possessed within himself powers and talents perhaps never before combined within one philosophical character." The volume before us bears testimony which would go a long way towards justifying this eulogium. We think that Sir W. Hamilton has been fortunate in having a biographer so careful in his facts and so skilful in the manipulation of his copious materials as Mr. Graves has proved himself to be. Hamilton had the habit of putting on record very minute circumstances. He preserved copies of a large proportion of the letters and notes written by him, whether important or not; he often recorded the hour at which they were despatched, and the person to whom they were intrusted for the post. The enormous mass thus accumulated during a long and very studious life were left at Hamilton's death in a state of utter confusion, and it has been the laborious duty of his biographer to extract from the mass those materials which were suitable for his purpose. The very extensive correspondence of Hamilton is also a source from which his biographer has obtained much aid. Of his own qualifications for the task the biographer thus modestly expresses himself in the preface:—

"The public has some right to inquire why one who has to confess himself to be no mathematician should have undertaken the present work. To such an inquiry I may reply as follows: that although unconnected with Sir W. R. Hamilton by any tie of kindred, I became his friend in the youth of both of us, and that our friendship continued unbroken till the day of his death; that when he was applied to by the Editor of the *Dublin University Magazine* in 1841 to name a friend who should be requested to supply to that magazine a biographical sketch for insertion in its portrait gallery of distinguished Irishmen, he did me the honour of designating me, and furnished me with the necessary facts; that he afterwards sought my consent to his nomination of me as his literary executor,—a nomination however which, he told me afterwards, he thought right to withhold when he found that the remainder of my life would probably be spent in England, and that I should therefore be unable to fulfil

the duties of the trust without undue inconvenience. Lastly, that after his death I was asked by his sons to undertake the task, and was at the same time informed by several of the most influential of his friends that this selection met their approval, and that they were willing to trust to my judgment the correspondence over which they had control."

William Rowan Hamilton was born in Dominick Street, Dublin, on August 3-4, 1805. His father, Archibald Hamilton, was a solicitor. When the boy was little more than a year old, it would seem that he gave such indications of unusual talent that his parents decided to commit the education of the child to his uncle, the Rev. James Hamilton of Trim, a man of very remarkable talents, who, with his sister, Jane Sydney Hamilton, reared and educated the child. What that childhood was can be best described in the words of the biographer, who says, on pp. 46-47:—

"It will then be noted that, continuing a vigorous child in spirits and playfulness, he was at three years of age a superior reader of English and considerably advanced in arithmetic; at four a good geographer; at five able to read and translate Latin, Greek, and Hebrew, and loving to recite Dryden, Collins, Milton, and Homer; at eight he has added Italian and French, and gives vent to his feelings in extemporised Latin; and before he is ten he is a student of Arabic and Sanscrit. And all this knowledge seems to have been acquired not indeed without diligence, but with perfect ease, and applied, as occasion arose, with practical judgment and tact; and we catch sight of him when only nine swimming with his uncle in the waters of the Boyne. In this accomplishment he afterwards became a proficient."

Again, on p. 51 we have a description of a little manuscript book of 30 pages thus entitled "A Syriac Grammar. In Syriac Letters and Characters; Compiled from that of Buxtorf; Translated into the English Language and Syriac Characters by William Hamilton, Esq., of Dublin and Trim. Begun July 4th, 1817; Finished July 11th, 1817." The conclusion of the book is as follows:—"Thus have I gone through what is necessary to be known for reading and writing Syriac. . . . Soon may be expected an account of their irregular and indeclinable words, &c., with a syntax." The author of this production was still under twelve years old.

A couple of years later (November, 1819) we find Hamilton inditing a letter in Persian to the Persian ambassador, Mirza Abul Hassan Khan, then on a visit in Dublin. Hamilton has left a translation of this production, the following extract from which evinces the Oriental aroma which pervades the whole:—

"As the heart of the worshipper is turned towards the altar of his sacred vision, and as the sunflower to the rays of the sun, so to thy polished radiance turns expanding itself the yet unblossomed rosebud of my mind, desiring warmer climates whose fragrancy and glorious splendour appear to warm and embalm the orbit about thee, the Star of the State, of brilliant lustre."

Hamilton's letter met with a very favourable reception; the secretary had observed no mistakes, and inquired whether he had not copied it from something, and the compliments bestowed on the author were all the more pointed, because "Captain Kian," who had also attempted to write a letter in Persian, was informed that his presence would be dispensed with, as *his* letter was totally unintelligible.

A large fraction of the present volume is filled with the poetical effusions, in which on all occasions Hamilton was prone to indulge. The first traces of these "showers of verse," as Wordsworth afterwards playfully called them, is found in Hamilton's letters to his sisters. The biographer has not, however, deemed it desirable to record any poetical effort prior to his sixteenth year, and the first piece we find is (p. 95) "To the Evening Star," of which the first stanza is—

"How fondly do I hail thee, Star of Eve,
In all thy beauty sinking to the west,
And as if loth our firmament to leave
Slow and majestic sinking to thy rest."

Hamilton lived and thought in an atmosphere of poetry; he wrote poems on all occasions and all sorts of subjects. It was perhaps not unnatural that as a disappointed lover he should bewail his sorrows in verse, that he should write birthday addresses to his sisters, and sonnets on the Beauty of the Dargle, but we also find him addressing an "Ode to the Moon under Total Eclipse," and to use his own words in writing to Wordsworth, "I have always aimed to infuse into my scientific progress something of the spirit of poetry, and felt that such infusion is essential to intellectual perfection." He has, however, indicated very clearly where his real ambition lay, for at the age of twenty, writing to his friend, Miss Lawrence, he says:—

"But while you concur with my own sober judgment in refusing to award me the crown of poetic power you would not I am sure desire to extinguish in me that love of 'sacred song' to which I can with truth lay claim. There is little danger of its ever usurping an undue influence over a mind that has once felt the fascination of science. The pleasure of intense thought is so great, the exercise of mind afforded by mathematical research so delightful, that having once fully known it, it is scarce possible ever to resign it. But it is the very passionateness of my love for science which makes me fear its unlimited indulgence. I would preserve some other taste, some rival principle; I would cherish the fondness for classical and for elegant literature which was early infused into me by the uncle to whom I owe my education, not in the vain hope of eminence, not in the idle affectation of universal genius, but to expand and liberalise my mind, to multiply and vary its resources, to guard not against the name but against the reality of being a mere mathematician."

A year later (1822) we find Hamilton entering upon the path of original mathematical discovery. The title of one of the first of these early papers is "Examples of an Osculating Circle determined without any Consideration repugnant to the utmost rigour of Analysis." With two others, one on "The Osculating Parabola to Curves of Double Curvature," and the other on "The Contacts between Algebraic Curves and Surfaces," Hamilton paid his first visit to Dr. Brinkley, then the Astronomer Royal of Ireland. Dr. Brinkley was impressed by their value and by the genius which at the age of seventeen had produced work of so much originality.

The first year of Hamilton's career in Trinity College, Dublin (1824), justified all the expectations entertained by his friends. In his Freshman year he distanced all his competitors alike in classics and in mathematics, while he was also awarded a Chancellor's prize for his poem on the subject of the Ionian Islands. In the same year we read that he commenced another friendship, which remained unbroken to the end of the long life of the brilliantly gifted Maria Edgeworth, and which brought to Hamilton

many of her delightful notes and letters, and in them cordial sympathy and wise counsel. Of Hamilton Maria Edgeworth writes: "Mr. Butler came with young Mr. Hamilton, an 'admirable Crichton' of eighteen, a real prodigy of talents, who, *Dr. Brinkley says, may be a second Newton.*"

At the age of twenty-one came the turning-point in Hamilton's career—his appointment to be Andrews Professor of Astronomy in the University of Dublin, and Royal Astronomer of Ireland. The vacancy arose from the promotion of Brinkley in 1826 to be the Bishop of Cloyne. The following incident of the occasion is given by his biographer:—

"Candidates for the post came over from England, among them Mr. Airy of Cambridge (already distinguished by his Senior Wranglership and by optical researches), and some who had already gained the rank of Fellow in Hamilton's own college were competitors. It appears that before the end of April he met Airy and other eminent men at the table of Dr. Lloyd, and we remember hearing that, in the scientific discussion to which the meeting gave occasion, he took his part with striking ability, modesty, and firmness, when it became necessary to defend some of his optical results against the objections of Mr. Airy."

Hamilton seems to have felt that it would be presumptuous for an inexperienced undergraduate to put himself forward as a candidate; he therefore retired to the country to carry on quietly his work for the classical medal. It was only a week before the appointment had to be made that he received at Trim, from his tutor, Mr. Boyton, an intimation that the Board were favourably disposed towards him, and urging him to come up at once to take the advice of his friends. That advice concurring with the strong opinion of his zealous friend and tutor, he was unanimously appointed on June 16, 1827.

A few months later Hamilton paid a visit to Keswick, and commenced his memorable friendship with Wordsworth. That the philosopher and the poet were mutually interested is manifest from Hamilton's account written in a letter to his sister Eliza:—

"He (Wordsworth) walked back with our party as far as their lodge, and then, on our bidding Mrs. Harrison good night, I offered to walk back with him while my party proceeded to the hotel. This offer he accepted, and our conversation had become so interesting that when we arrived at his home, a distance of about a mile, he proposed to walk back with me on my way to Ambleside, a proposal which you may be sure I did not reject, so far from it that when he came to turn once more towards his home I also turned once more along with him. It was very late when I reached the hotel after all this walking."

Hamilton quickly followed up his introduction to Wordsworth by sending him an original poem entitled "It haunts me yet." Wordsworth replies:—

"With a safe conscience I can assure you that in my judgment your verses are animated with the poetic spirit, as they are evidently the product of strong feeling. The sixth and seventh stanzas affected me much, even to the dimming of my eyes and faltering of my voice while I was reading them aloud. Having said this I have said enough. Now for the *per contra.* You will not, I am sure, be hurt when I tell you that the workmanship (what else could be expected from so young a writer?) is not what it ought to be. . . ."

"My household desire to be remembered to you in no formal way. Seldom have I parted—never, I was going to say—with one whom after so short an acquaintance I lost sight of with more regret. I trust we shall meet again."

The biographer adds that Wordsworth has said in his hearing that Coleridge and Hamilton were the two most wonderful men, taking all their endowments together, that he had ever met.

At the commencement of his career at the Observatory Hamilton entered with diligence into the practical work of observing, but it would seem that the necessary exposure told injuriously on his health. It does not appear that he made any observations of importance. His tastes pointed strongly in the direction of mathematical research, and the development of his discoveries occupied more and more of his time, until at length, with the full consent of the authorities of the University, Hamilton practically relinquished all observatory work and gave his splendid mathematical genius full scope. Unquestionably this was the best course for the credit of Hamilton himself, best for the credit of his University, and best for the interests of science. Hamilton could never have made even a moderately successful practical astronomer. He tells Dr. Robinson that he disliked observing; he was essentially a man of speculation rather than of action. Like his friend De Morgan, Hamilton was not "a man of brass, a micrometer-monger, a telescope-twiddler, a star-stringer, a planet-poker, or a nebula-nabber"—he had none of the qualifications necessary for a routine of observatory work. His workshop was his study, where he sat immersed in what he calls his "mathematical trances" and elaborated his great discoveries.

The latter half of the volume describes his early life at the Observatory. He was fortunate in obtaining as a pupil Lord Adare, afterwards Earl of Dunraven, between whom and Hamilton a lifelong friendship of the tenderest character arose. Many other friendships are here copiously illustrated by the letters which have been preserved. The letters to and from Sir J. W. Herschel and Sir G. B. Airy relate chiefly to the discussion of Hamilton's labours on the systems of rays and other matters of purely scientific interest; but there are stores of other letters. The voluminous correspondence between Hamilton and Wordsworth will itself possess a wide interest even in circles where Hamilton's more serious labours are unknown. There are letters to and from Coleridge, as well as many others relating to purely literary matters. There is an extensive correspondence with Dr. Robinson, in which the Armagh astronomer gives kindly counsel to his younger brother at Dunsink. There is the correspondence with his friend, Aubrey de Vere. There are the numerous letters to his lady correspondents, to his sisters, to Maria Edgeworth, to Lady Dunraven, to Lady Campbell, and to Miss Lawrence. Then there is the visit of Hamilton to London, chiefly for the purpose of visiting S. T. Coleridge, to whom he had an introduction from Wordsworth; and there are interesting accounts of the visits of Wordsworth to the Observatory at Dunsink, where a shady walk in the garden still bears the poet's name.

A chapter towards the close (p. 623) gives a sketch of the discovery of conical refraction made in the year 1832, while the author was still only twenty-seven. The importance of this discovery was speedily recognised, and the biographer writes: "At the Cambridge meeting of the

British Association, 1833, the attention of the mathematical and physical section was largely given to the subject, and Herschel, Airy, and others spoke warmly in praise of the discovery. In the introductory discourse with which the proceedings of that meeting was opened, Prof. Whewell made it a topic, and expressed himself in the following words: 'In the way of such prophecies few things have been more remarkable than the prediction that under particular circumstances a ray of light must be refracted into a conical pencil, deduced from the theory by Prof. Hamilton and afterwards verified experimentally by Prof. Lloyd.' Previously, in the same year, Prof. Airy had publicly recorded his impression upon the subject as follows: 'Perhaps the most remarkable prediction that has ever been made is that lately made by Prof. Hamilton.'"

The view Hamilton himself took of the discovery of conical refraction was characteristic. "It was," he writes to Coleridge on February 3, 1833, "a subordinate and secondary result when compared with the object I had in view to introduce harmony and unity into the contemplations and reasonings of optics regarded as a branch of pure science."

At the close of this volume we still leave Hamilton quite a young man. The great labour of his life has not yet commenced; its nature has not indeed even dawned upon him. We shall therefore look forward with pleasure to the continuation of the present most interesting work. The development of Hamilton's more mature genius, his correspondence with De Morgan, in itself no inconsiderable mass, and above all the gradual evolution of quaternions, will form most attractive materials for his biographer.

It is by the liberality of the Board of Trinity College, Dublin, that the present instalment of the work has been brought out, and we sincerely trust that the same liberality will be extended to enable the biographer to continue to do real justice to his subject. But besides the present work another debt is due to his memory. Hamilton's earlier papers are very inaccessible: many of them are scattered about in various periodicals, and his two noble treatises on quaternions are out of print. A complete edition of Hamilton's works would be an appropriate sequel to this biography, and they would be not unfitting companions for the works of Lagrange and of Gauss. It is not often that a University has so gifted a son as Hamilton. Let us hope that the University which is proud to claim him will see fit to raise this further monument to his genius.

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications.*

[*The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to insure the appearance even of communications containing interesting and novel facts.*]

Sheet-Lightning

In Nature, vol. xxvii. p. 576, a statement is made that the "opinion so long and generally entertained" that "sheet-lightning and the so-called summer or heat-lightning are nothing else than the reflection of, or the illumination produced by distant electrical discharges, is not supported by observation." This statement surprises me, for I should have said that the opinion once commonly entertained that sheet-lightning is a distinct form of lightning unaccompanied by sound, is now for the most part rejected, the results of observation being distinctly against it. The question is an old one; but as the writer of the above statement only refers to the observations made at Oxford during the twenty-four years ending 1876, I will confine myself in the main to an examination of these. I must premise that I do not assert that lightning never occurs at such an altitude that the thunder accompanying it is not audible. In rare instances in Europe lightning is observed in the zenith, followed after an interval of twenty seconds or more by faint rolling thunder immediately overhead. It is therefore antecedently probable that lightning may occur at too great an elevation for the thunder to be heard at the earth's surface at all; and this is especially likely to happen in some of those thunderstorms within the tropics, the altitude of which is extremely great.

The distance at which the illumination produced by lightning in a dark night can be observed depends upon the altitude and the intensity of the discharge, and further upon the altitude, character, and amount of the clouds. It is possible that the diffused particles of ice (at a much greater altitude than the cirri), which produce the phenomenon called "rayons du crépuscule," are capable in some cases of reflecting the illumination. However this may be, it is certain that the illuminations of an ordinary thunderstorm at midnight, when there is no moonlight, have an average radius of more than forty miles. The distance at which thunder is heard depends on a variety of conditions; but we may safely state that in the open country in calm weather at midnight the sound is rarely heard at a greater radius than fifteen miles. At the Radcliffe Observatory, which is scarcely out of the reach of rumbling sounds produced by the traffic of a town, the average distance at which thunder is distinguished may probably be safely reduced to seven miles. Assuming then that at Oxford the area of illumination has a radius of forty-two miles, and that of thunder one of seven miles (and in this assumption we are probably not very far from the truth), we conclude that in the darkest hours "lightning without thunder" should occur at Oxford with a frequency which is expressed by the figures 35 : 1 as compared with "thunder with or without lightning." A deduction ought, of course, to be made for the effects of moonlight. But when this has been made, the figures quoted by your reviewer are not only satisfied by the hypothesis for the refutation of which he employs them, but further, if his mode of reasoning were legitimate, they would lead us to the conclusion that in nearly seven cases out of eight the thunder heard at Oxford is not the result of electrical discharge at all! Such thunder does not occur elsewhere, and was not in vogue at Oxford "in my time."

Practically, however, two considerations must not be omitted: (1) some localities enjoy a special immunity from thunderstorms, while others are responsible for the production of an exceptionally large number; in the former the frequency of illumination will be greater in comparison with the frequency of thunder, in the latter it will be less: (2) (and this is a consideration of much more importance, though frequently neglected when a conclusion is deduced from records of phenomena) the relative frequency of two sets of *occurrences* often differs widely from the relative frequency of the *records* of the occurrences. The relative frequency of records of thunder and of lightning is to a large extent dependent on the position of the observer's residence, his habits, the keenness of his eyes and ears respectively, and his attentiveness to the impressions which those organs respectively experience.

No one who has on a summer night carefully watched the gradual approach of a great thunderstorm, counting the flashes, and registering the time-interval and number of claps from the minute when the first flickers begin above the southern horizon to that at which the storm is in its full roar and rattle overhead; no one who in a long night journey by train has run into a thunderstorm whose distant coruscations he has noticed two or three hours beforehand; no one, at least, who after watching sheet-lightning in one particular direction has made careful inquiries as to the occurrence or otherwise of thunder over the district from which the light proceeded, will hesitate in pronouncing the verdict that ordinary sheet or summer lightning is simply the illumination produced by a distant thunderstorm.

W. Clement Ley

St. Andrews

ST. ANDREWS is the one rural university in Scotland. Its small constituency is of a somewhat peculiar kind, drawn from many sources, not by mere "gravitation," but by natural choice, and probably would not follow it if removed to a town. When the Tay Bridge has been set up again, St. Andrews will be within thirty-five minutes of Dundee. The changes which may result from this are difficult to forecast. Meanwhile the University is doing, although a limited, yet a good and useful work, and is blessed with many distinguished sons. If the career of each of these men could be traced from the Peebleshire U.P. manse, or the Forfarshire village schoolhouse, or the Cupar building-yard, to the place in life which they now fill and adorn, the history would be in many ways instructive, and it would be seen that the ideal of a "ladder" of merit (for lads of merit) is to some extent realised north of the Tweed.

The English universities, with all their wealth and all their noble endeavours, have never yet, like those in Scotland, had "their root spread out by the waters" of the life of the people. The Scottish universities fulfil the Christian precept of asking the poor to their feast. But as a consequence of this they have few rich friends, and have the more need of being visited with "the dew of heaven from above."

St. Andrews, however, has been unfairly dealt with in a special way, and it is an unfairness which can be remedied under the present Bill without making any demand upon the public funds. Three of her Chairs and more than half of her bursaries were left by the Commissioners of 1858 under the care of private patronage. The removal of this blot would inevitably be followed by an accession of healthy life. And the place is already by no means deficient in corporate vitality. Its undergraduates, numbering a few more than those at Exeter College, Oxford, and a few less than those at Balliol or Christ Church, have their football club, golf club, gymnastic club, two debating societies, musical association, domestic and Shakespearian association, and others, to which has recently been added a volunteer artillery corps. At football they have somehow managed to hold their own against the larger universities. Suppose that by the efforts of the new Commission some *Concordat* were arranged between this old yet vigorous life and the Herculean infant across the Tay, that by this means a complete Science Faculty could be established in this part of Scotland, and a new development given to the already existing St. Andrews Science Degree, would there not be then a promise for the future?

In spite of rail and telegraph, the feline attachment to places is still shared by man. To deracinate is easier than to plant, easier to plant than to make what is planted grow. Wise statesmanship will follow nature, and avail itself of elements which exist, if only life is found in them.

To return to the more general aspect of the question: the Scottish Universities have a claim to State recognition which has hardly been sufficiently considered. They are the spiritual progenitors (" though honest, yet poor," like Launcelot Gobbo's father) of all University life in Great Britain that does not directly flow from Oxford and Cambridge (see the *New Monthly Magazine* for the year 1825), and for much of this too. Prof. Stuart of Cambridge is a St. Andrews man. Had he been an Etonian or Harrovian it is not too much to say that the higher education in many English towns would be in a different position from that on which they congratulate themselves to-day. X.

Cape Bees and "Animal Intelligence"

I KEEP a large number of hives, chiefly of Cape bees, and find that their habits closely resemble those of European honey-bees; but in the course of my observations I have met with an instance of sagacity on the part of Cape bees, which, although it may also have been observed with regard to European or American bees, has not, so far as I am aware, been recorded in any treatise upon the subject. Last year my gardener hived a swarm of bees, which were not however satisfied with their new hive, their scouts having probably already selected some hollow tree for their future habitation. They accordingly left, but were soon again secured. In order, if possible, to prevent their deserting the new hive, I placed the queen in a queen-cage (a small perforated metal box with circular holes of the diameter of an ordinary pin's head), which I fixed to the roof inside the hive. A few days afterwards there were several honeycombs in the hive, and in most of the cells eggs had been deposited. Now there could be only three ways of accounting for these eggs in the cells: there might have been more than one queen in the swarm, or there might have been an egg-laying neuter among them, or else the eggs must have been those of the imprisoned queen. Accordingly I several times examined the swarm and the honeycombs (the hive being a frame hive), and satisfied myself that there was no other queen in the swarm. The queen was kept in the cage until some of the larvæ had come to maturity, the bees of course feeding her through the holes of the cage, and I found that the young bees were neuters, and not drones as they would have been if the eggs had been laid by a neuter. The only explanation, therefore, of the presence of the eggs in the cells was that they had been laid or passed by the queen through the holes of the cage, and taken up and deposited in the cells of some of the workers. This performance showed so much sagacity on the part of the bees, especially the mother bee, that I subsequently repeated the experiment with eight other swarms, and in two instances there was an exactly similar result. Two of the six remaining swarms were so dissatisfied with the new hives offered to them that they refused to build any comb, and ultimately deserted the hive, leaving the caged queen behind, although I was quite satisfied that neither swarm had a second queen among their number. I may here remark that it is much more difficult to retain a swarm of Cape bees in an artificial hive selected for them than appears to be the case in Europe or America, the explanation perhaps being that they are not sufficiently domesticated, and prefer being queenless in a natural hive selected by themselves to remaining with their imprisoned queen in a hive they do not approve of. It is possible of course that the two swarms which left their queen behind may have joined some other occupied hives or may have returned to their own former hive; but I may state that on each occasion I had removed the hives from which the swarms had issued to a considerable distance from their former position. The four remaining swarms upon which I experimented were satisfied with their new hives and built combs, but no eggs were found deposited in the cells. One of these swarms had an imported fertile Italian queen; the second and third had Cape queens, and the fourth had an Italian queen, the progeny of the imported one; the three first began laying in the cells soon after being released, but the fourth never laid eggs at all. As to the last of these queens, I fear she was rather roughly handled when caught, and that this may explain her not laying at all; but I may add that I have not yet succeeded in obtaining queens proved to be fertile from among the progeny of imported Italian queens. There are very few Italian drones in the colony, or at all events in the neighbourhood of Cape Town, and if the Cape drone does not cross with the Italian queen this would be a sufficient explanation of my failure. While upon this subject I may state that we have a yellow bee in South Africa somewhat resembling the Italian, but the neuters are a little smaller. They more closely resemble the Egyptian bees, judging by the descriptions I have read of the latter; but some of their habits are different, for they have only one queen in a hive, and they gather and use propolis, which the Egyptians are said not to do. But most of our Cape bees rather resemble the English bee, although considerably smaller, and the rings of their abdomen are of a lighter brown colour, and I confess till a few years ago I was not aware that we had any other variety. To my surprise, however, about three years ago a swarm of the yellow-winged bees arrived at my place. At first I took them to be Italians, but I had not yet then imported any myself, nor have I since been able to discover that any one else had done so. The queen and drones were exactly like the Italian queen and drones, but the neuters were a little shorter and more slender. I have unfortunately not secured any fresh swarms from the one which I hived, but the neuters that are now in the hive cannot be distinguished from the ordinary Cape worker. There are not at present any drones in the hive, and, as the hive has no frames, it is difficult (without first driving the swarm) to discover whether the queen, now in the hive, has the same appearance as the one which originally arrived. Strangely enough I continually find drones of the yellow variety in hives of the ordinary Cape brown bee. I sometimes, but rarely, see yellow workers visiting my flowers and fruit, and on a recent visit to Natal I saw numbers of bees visiting a sugar store in Durban, all of which were of the yellow variety. I was not sufficiently long in Natal to be able to say whether there are any of the ordinary Cape bees in that colony, but in the Transvaal I have seen both varieties in the fields.

Before concluding I wish, with your permission, to make a

few remarks upon a passage in Mr. Romanes' very interesting book on "Animal Intelligence." At p. 188 he says: "Bee-masters who attend much to their bees, so as to give the insects a good chance of knowing them, are generally of the opinion that the insects do know them, as shown by the comparatively sparing use of their stings." If by this he means that the bees recognise and become accustomed to the scent of persons who attend much to them, I quite agree with him, but I do not believe that their recognition goes any further. I keep two apiaries at a considerable distance from each other, to one of which my gardener, a coloured Malay, attends, and to the other a Kafir labourer. At first they were generally stung when passing too near the entrance of a hive, but now they pass and repass with impunity. They work with the bees more frequently than I do, and yet when either of them assists me in his own apiary, he receives more stings than I do. This I ascribe to the gardener's using snuff in his mouth very freely, and to the Kafir's very pronounced odour. To test the recognition of the bees I once requested the Malay and the Kafir to change clothes with each other, and wear thick veils over their heads and faces. They did so, and assisted me first in the apiaries to which they were respectively in the habit of attending, with the result that they received no stings, but when either began to work with the bees in the apiary he usually did not attend to, he was so stung about the hands that he had to beat a hasty retreat, whilst I remained uninjured, although not veiled. The two men are almost of the same size and build, so that if the bees had any power of general recognition they would probably (as some of the other servants did) have mistaken the one for the other. I can, therefore, only account for the conduct of the bees by the unpleasant, and to them strange, odour. At my request the gardener discontinued the use of snuff in his mouth for some time, and during that time he was not stung more than I was while working with bees, but if the Kafir stands before the entrance of an unaccustomed hive he is remorselessly stung. I may add that Cape bees are very much more vicious than European ones seem to be, and that, if not skilfully handled, they will unmercifully sting their most familiar friends. On one occasion a bunch of carrots was left near the gardener's apiary, which so enraged the bees that they stung him and every one else they came across, and very nearly stung a cow to death at a distance of about a hundred yards from the apiary; and on another occasion a horse, still wet with sweat, trespassed too near a hive, with the result that the whole apiary was in an uproar, and some of my children and servants were stung, the chief victim being a Malay girl who used to apply quantities of scented pomatum to her hair, and who was severely stung on the head. Mr. Romanes continues thus: "Again, many instances might be quoted, such as that given by Gueringius, who allowed a species of wasp, native to Natal, to build in the doorposts of his house, and who observed that, although he often interfered with the nest, he was only once stung, and this by a young wasp; while no Kafir could venture to approach the door, much less to pass through it." It does not appear whether any white stranger was ever stung, and the only inference that can be reasonably drawn from the conduct of the wasps is that they disliked the odour of Kafirs, which, as is well known, is peculiarly disagreeable. If a particular Kafir had been in the habit of passing through the door, the wasps would probably have become accustomed to his scent in the same way as a swarm of bees, upon the testimony of Sir John Lubbock, became accustomed to the scent of eau-de-cologne repeatedly dropped at the entrance of their hive.

J. H. DE VILLIERS

Wynberg House, Wynberg, Cape of Good Hope, April 3

The Metamorphic Origin of Granite

As I had charge of the granite quarries in Mull during the five years ending 1875, and am still closely connected with them, I would like to say that the conclusions stated in the Duke of Argyll's letter in your issue of last week (p. 578) are beyond all question correct, and are the same as I formed from independent observation while I lived at the quarries.

In addition to the facts mentioned in the Duke's letter, I would say that the structure shown by the granite while decaying under atmospheric action and the cleavage which it shows in the quarry all may point to its having been a stratified rock at one time; and in several places on the shore of the Sound of Iona and in North Bay Quarry, patches of semi-metamorphosed schist are found in the granite. One very fine specimen is on the north side of Fionphort Bay.

The change from schist to granite on the north side of the peninsula of Ross, which the Duke speaks of as "obscured at the head of Loch Laigh," does, according to my observation, not take place there, but a little further west, in a bay between Loch Laigh and the inlet leading to Ardfenaig. The change can be traced foot by foot there most perfectly, and any number of specimens of it in all stages can be picked up on the beach.

Though, however, the metamorphic origin of the Mull granite is, in my opinion, beyond doubt, I think that the metamorphic agent has yet to be discovered. The most plausible hypothesis is that it was a superincumbent mass of trap, but an inspection of the very destructive influence of the trap dykes that we meet with in the quarries upon the granite about them makes this very unlikely to my mind. For some distance on each side of such dykes the granite is quite useless.

9, Angel Place, Edmonton, April 23 WM. MUIR

Helix pomatia

As *Helix pomatia* appears to be very partial in its distribution in this country, it may be worth while to record the fact that I have met with it on and near the chalk downs in the neighbourhood of Epsom, and on the chalk downs above the village of Hambledon, in South Bucks; while Mr. J. E. Harting states that it is not uncommon on the chalk hills in the vicinity of Reigate and Dorking, and in parts of Kent.

Forbes and Hanley, in their "History of British Mollusca," say "it is entirely confined to the southern counties, living chiefly on cretaceous soils"; but we learn from Mr. Gwyn Jeffreys (NATURE, vol. xxvii. p. 510) that it is abundant at Woodford, in Northamptonshire; and from Mr. Blomefield (NATURE, vol. xxvii. p. 553) that it occurs sparingly in Gloucestershire, neither of these counties being cretaceous.

With regard to its possible introduction into this country by the Romans, we gather from Venables' trustworthy work on the Isle of Wight that *Helix pomatia* has not been met with in the island, although it was occupied—and probably permanently—by that people; but *H. scalaris*, which, according to some malacologists is a monstrous form of this species, has been found there. Its absence from the Isle of Wight may be said to be somewhat remarkable, seeing that the species extends in the south at least as far as the borders of West Sussex, and that the other British chalk-frequenting *Helicidæ*, *H. caperata*, *H. ericetorum*, and *H. virgata*, are very abundant in the island. Either of two causes may account for its absence from this locality:—it may be a geologically recent importation from its original (?) centre in France, and has not yet succeeded in navigating the salt waters of the Solent; or its exceptionally large size may have proved its destruction in its exposed favourite haunts. The latter supposition is the more probable one, as it would account for its general rarity, and at the same time help to explain the prevalence in the same exposed haunts of the smaller *Helicidæ*.

PAUL HENRY STOKOE

Wycombe Court, Bucks

The Zodiacal Light (?)

REFERRING to the sunset phenomena described by J. W. B., of Bath, in NATURE, vol. xxvii. p. 580, permit me to inform you that I also was an observer and was well aware from previous experience that it was not the zodiacal light, which, as seen in the evening from any latitude north of the tropics always inclines to the left, and, if seen in the morning, in the east, then to the right, whilst the phenomena in question appeared as a vertical column, of a warm tint, extending upwards to about 5° from where the sun had just set moving to the right, and descending with that luminary, continuing visible for about thirty minutes from the time I first noticed it immediately after the sun had gone down behind the low range of the Yorkshire Wolds, distant from my garden five or six miles in a north-west direction.

Having never before witnessed a similar phenomenon, although I have had for upwards of eleven years an uninterrupted view of the sunset region of the sky, and, except in midwinter, am nearly always at home at sunset, and on fine evenings in the garden, I was somewhat puzzled as to whether the cause was local and atmospheric or otherwise.

If your correspondent can refer to the "Heavens," by Guillemin, p. 86, 1st edition, or to Milner's "Gallery of Nature," 1st edition, p. 62, he will there see woodcut representa-

tions of the zodiacal light, or to "Chambers's Astronomy," 3rd edition, p. 92, where a short chapter is devoted to the subject.

Speaking from my own experience, the zodiacal light is best observed in this neighbourhood during the clear evenings of February or March, in the late twilight, and of course in the absence of moonlight.

On referring to my copy of the *Astronomical Register* for 1875, vol. xiii. p. 196, I find a letter from Mr. T. W. Backhouse in reply to a previous communication from Canon Beechey in the same volume, p. 174, describing what appears to have been a much finer display of this sunset phenomenon as seen by the rev. gentleman from Downham, Norfolk, than either your correspondent or myself witnessed.

Mr. Backhouse states: "It is purely an atmospheric phenomenon ascribed to the sun shining on particles of water or ice."

May I ask if the above explanation is an established fact or only a theory?

I shall be glad if you receive and can make room for the accounts of other observers, as I cannot think the appearance is a very common one—at least not in this neighbourhood.

Hull, April 24 WILLIAM LAWTON

REFERRING to the letters in your columns on this subject, I beg to forward two photographs of the sun, which show distinct horns of light on each side of the disk. They were taken—the sun high in the heavens at the time—some two months ago in a simple camera, without any special arrangement, except a rapid shutter, but the development was undertaken with some care, and arrested as soon as the light fleecy clouds around made their appearance. H. B. P.

Blackheath, April 27

[WE have received the photographs, which are certainly very remarkable if our correspondent can certify that the strange prolongations which appear on them are special to them, and not in any way dependent upon any possible reflection from the lenses employed.—ED.]

THE phenomenon described on pages 580 and 605, under the heading "The Zodiacal Light (?)" was that generally known as a "Sun Pillar." I send herewith an engraving of one seen from Sidmouth in 1871, full descriptions of which were given in the *Meteorological Magazine* for May, June, and July of that year.

Sun Pillar seen near Sidmouth, April 4, 1871.

I believe that it is merely a portion of a halo passing vertically through the sun; in the recent case, that portion of the halo which was *above* the sun was alone seen, sometimes the portion *below* it is seen alone, and occasionally both are visible, together with a parhelic circle (or parts of one), and then of course we have the rare phenomenon of the sun as the centre of a luminous cross. I have called this complete phenomenon of the solar cross rare, for I know of only three occasions of its being seen, and even these I have not verified in the originals, but those interested may search in *Hugenii Opuscula posthuma*, ii. 48, for the details of the phenomenon seen in Cassel in January, 1586, by Roth; and in the *Mém. de l'Acad. des Sciences* for 1693 and 1722, for descriptions by Cassini and Maléziec. G. J. SYMONS

62, Camden Square, N.W.

THE curious luminous projection after sunset on the 6th inst., noticed by several of your correspondents, was also seen for some time very soon after sunset in Herefordshire. Its shape was somewhat like a vertical pillar of soft, hazy, yellowish, luminous light, about the width of the solar disk, 10° in height above horizon, and finishing rather abruptly with a conical termination in a clear sky. R. P. GREG

Coles, Buntingford, Herts, April 29

ALLOW me to call the attention of such of your readers as are interested in the above phenomenon, to a communication from Mr. J. J. Murphy of Belfast, in your issue of July 13, 1876, and to another from myself, a fortnight later, describing a sun pillar seen in the north of Ireland on June 27, 1876. R. V. D.

Beragh, co. Tyrone, April 28

Mock Moons

THE mock moons mentioned in your last week's issue (p. 606), by Mr. Mott, were seen here. The circle subtended an angle of 50°. When first seen, a line drawn through the mock moons passed through the moon itself. At 11 p.m. such a line was 3° above the moon. At 1 a.m. the appearance was as at first. This change of level of the refracting cloud is what Mr. Mott alludes to when he says it "seemed to be unaccountably out of place." I was not aware that there was any fixed place for the brighter portions of the halo. SM.

Temple Observatory, Rugby

The Freshwater Medusæ

IT may interest some of your readers to know that the little freshwater Medusæ (*Limnocodium Sowerbii*), which appeared in the Victoria Regia Tank here on June 9, 1880, for the first time, again on June 12, 1881, and not at all during 1882, appeared again in the tank on Saturday morning, April 28, many of them being full grown individuals. The tank, which remains empty during the winter, was filled with water on March 8.

April 30 W. SOWERBY

The Circles of a Triangle

CANNOT the method of "portmanteau" words be advantageously applied? I beg leave to suggest the following names: *circumcircle, incircle, excircle,* and *midcircle;* these are for speech, in print or writing they might appear C⊙, I⊙, E⊙, M⊙.

April 28 W. H. H. H.

Flight of Crows

IN watching crows as they fly overhead, I often think they are not flying straight forward, but have the line from head to tail at an angle of about fifteen degrees with the line of flight. Can this be corroborated? I do not like to trust my own observing powers in such a matter. JOSEPH JOHN MURPHY

Old Forge, Dunmurry, co. Antrim, April 24

METAMORPHIC ROCKS OF SCANDINAVIA AND SCOTLAND

MUCH interest attaches to the researches of the Swedish geologists among the older crystalline rocks of Scandinavia. In the year 1873 Mr. A. E. Törnebohm published an important paper in which he showed that in the high grounds of Sweden Lower Silurian rocks, with recognisable fossils, pass up conformably into a vast overlying series of quartzites, schists, and gneisses. These metamorphic rocks were divided by him into two groups—the Seve group, composed mainly of quartzites and schists, and the Köli group, consisting

largely of mica-schists and clay-slates. In another memoir just published he furnishes additional information regarding the succession of these rocks. The old or fundamental (Archæan) rocks composed of gneiss, granite, &c., are overlain by thick masses of reddish sandstones, followed by quartzites and limestones, over which come Augen-gneiss, hornblende-schist, mica-schist, &c. This order of sequence, which is shown in numerous natural sections, will be at once recognised as that which Murchison first showed to be the stratigraphical succession in the north-west of Scotland. It is interesting to find that the parallelism which was traced many years ago between the structure of the Highlands of Scotland and the uplands of Scandinavia continues to be confirmed by the more detailed surveys of recent years.

OBSERVATION OF THE GREAT COMET OF 1882

(*Communicated by Vice-Admiral Rowan, Superintendent U.S. Naval Observatory*)

1883.	Washington mean time.	Comet – ★.		No. of comps.	Mag. of star.
		α.	δ.		
	h. m. s.	m. s.	′ ″		
April 4	8 29 49·8	–2 17·59	–1 17·6	12, 4	8

Comet.	log. $(p \times \Delta)$	Comet.	log. $(p \times \Delta)$.	Obs'r.	Comp. star.
α App.		δ App.			
h. m. s.	s.	° ′ ″	″		
5 57 20·58	9·5575	–9 18 27·5	0·7877	F.	W 1449

Mean Place of Comparison Star

Star.	α. 1883·0.	δ. 1883·0.	Authority.
	h. m. s.	° ′ ″	
W 1449	5 59 37·14	–9 16 53·4	Bessel.

Date.	Obs. – Comp.		Eph.
	$\Delta\alpha$.	$\Delta\delta$.	
	s.	″	
1825.	+4·06	+14	NATURE, vol. xxvii. p. 226, and *Ast. Reg.* No. 243, p. 72.

This observation was made with the 26-inch equatorial, and compared with the following of three bright points in the nucleus. If we had compared the middle point of the nucleus with the comet, the corrections would have been $\Delta\alpha = +1^{s}\cdot3$ $\Delta\delta = +0''\cdot3$.

E. FRISBY,
Prof. Math., U.S.N.

Washington, April 6

ANTHROPOLOGY[1]

I.

THE invitation to lecture on anthropology with which I have been honoured gives me freedom to speak both of the races of mankind zoologically, and also of the thoughts, arts, and habits which form their civilisation. It is on the development of civilisation that I especially wish to dwell, a subject of direct interest always and to all, and the more opportune now that the practical question of the instalment of a Museum of Civilisation in Oxford is under discussion. Still, man's bodily and mental history so act and interact on each other that it is well to carry on their study together. Both depend on the great principle of adaptive change, where rise in organisation gives fuller and freer existence, till "correspondence with the environment" fixes a more or less permanent state, or suppression or disuse brings on degeneration. These are processes systematised in the theories of development or evolution which have of late years become predominant, and which seek to account for the change of plants and animals on the earth by modified descent, and of mental and moral phenomena by modified sequence. There is a consideration I wish to bring prominently forward, as not having had the attention it deserves. It is that these processes of development, or evolution, or transformism were long ago recognised to no small extent by ethnologists. Thus Prichard, the leader of the monogenist school forty years ago, brought forward evidence for the derivation of the races of mankind from one original ancestral pair, whom he considered to have been negroes, whose descendants more or less varying by the operation of natural causes became modified or transformed into the various races adapted for life in the various climates of the earth. But this, so far as it goes, is the very theory of development or modified descent. Any ethnologist who argues on natural grounds "that all the races of man are descended from a single primitive stock," is an evolutionist within these limits; in fact these words are quoted not from Prichard or Quatrefages, but from Darwin. Within the last generation the science of man has had new evidence and argument brought within its range. The discovery that men were already making rude flint implements in the Quaternary period, when the contours of hill and valley were quite other than during the few thousand years known to chronology, has made a new scientific departure, placing primæval man in the hands of the geologists, who are now discussing whether he even existed in the yet more vastly remote Tertiary period. A yet greater move has been made by Darwin's systematic application of the principles of variation of breeds or races to account for the transitions between species or genera. How these have become transformed in the course of geological time is seen in Huxley's plate of the bones of the four-toed Orohippus, followed by the three-toed Miohippus and Hipparion, and this again by the horse of the present day. Zoologists thus enabled to reconstruct ideally the ancestry of the horse, are hopeful some day to discover likewise the fossil pedigree of the rider.

Thus it is plain why the new lines of biological research, whether into the general causes of variation in animals, or into the origin of the human species from a succession of lower mammalian forms, have not checked but stimulated the research which relates to man as man. Anthropologists do not feel as if their science had been plucked up by the roots and planted somewhere else; it is growing where it was, only cultivated higher than in old times. What substantial progress has been made of late years is well seen in the difficult department of craniology. That there really is something in the shape of a skull will be admitted when one compares the two before us on the table, types which illustrate an interesting point in the early history of our own country. The narrower skull belonged to one of that dolichocephalic Stone Age population whose remains were buried in the long-barrows on our downs. The broader skull belonged to one of the brachycephalic men of the later round-barrows. In the work of Greenwell and Rolleston will be found the anatomical comparison of these skull-types, and the evidence that the earlier tribes were not exterminated by the later

[1] Two lectures on "Anthropology," delivered on February 15 and 21 at the University Museum, Oxford, by E. B. Tylor, D.C.L., F.R.S.

invaders of the land, but that the two races lived and were buried together, and by intermarrying gave rise to a mixed population. What these early long-headed people were called, or what language they spoke, is still unknown. It is they to whom, on the strength of certain passages in classic authors, the name of Iberian has sometimes been given, and they have been identified with the Basques. But no absolute correspondence has been made out between them and any race past or present in Spain, so that Prof. Rolleston was wise in preferring to call the men of the English long-barrows by the local name of Silurians, and to rely on skulls for defining the type, and the burial-places for marking the state of civilisation, of an ancient race who thus take a well-marked place in the history of our land, but of whom we may possibly never learn much more.

The mixture of races which has gone on for ages in Europe makes European craniology a study of extreme difficulty, but to see its clearest results we must look to races long isolated and intermarrying till their skulls become almost uniform. How such a type will characterise a genuine race has been shown by Prof. Flower in describing the skulls from the burial caverns of the Kai Colo, the mountain people who appear to have been the original inhabitants of Fiji. These mountaineers, whose distinction it is to have had the narrowest skulls of any known race, are representatives of the frizz-haired blacks so widely spread in the island groups now called after them Melanesian. But the ordinary Fijian population, who have lately been incorporated in the British Empire, are not exactly Melanesians, nor are they Polynesians like the brown Samoans and Tongans of the islands to the eastward. It appears that these black and brown islanders have intermixed and become the joint parents of the present Fijian population. This is perfectly shown by their skulls, whose cephalic index of breadth (71) is intermediate between those of the two parent races, the ancient Melanesian mountaineers (66) and the Polynesians (83). Not only does the cephalic index of length and breadth follow this rule, but it proves true in the same way of the index of height, and of other measurements of jaw, eye, and nose, which almost absolutely follow the same rule of the mixed race between the two parent races. The gradation is so marked, that in the Fijian islands nearest the Polynesian islands the skull-measurements come nearest to the Polynesian type. It is I think the first time that anthropology has made so close an approach to mathematical accuracy in its inferences, and it must be admitted that when arithmetical rule thus finds its way into a descriptive science, the study is becoming serious. Let us now see what comparative philology has to say to this Fijian question. Every student who opens a Fijian grammar is apt to say, Here is a Polynesian language, like Maori or Tongan; the map shows in the names of the islands plain Polynesian words that a New Zealander would understand, such as *vanua* = land, *lima* = five; the Fijian not only has the familiar Polynesian *tabu* = sacred, but he can attach the Polynesian causative prefix *waka* to it and make the verb *wakatabu* = to tabu a thing or make it sacred. Yet this student, as he examines and analyses more deeply, is driven to admit that Fijian must not be catalogued among the Polynesian languages; indeed it seems as though the root and heart of it must be classed as Melanesian, belonging to the black not the brown race; nevertheless the black language has absorbed not only the words but the character of the brown language into an intimacy and depth of mixture hardly anywhere equalled. Prof. Max Müller, in the lectures which near a quarter of a century ago made a new era in the science of language in England, was careful to give the much-needed caution not to trust too much to language in settling questions of race. Here, however, is an example how language, in cases when it is possible to get its bearing clearly into view, may tell its story in perfect accordance with anatomy. The blended parentage of the Fijians is heard in their speech as it is seen in their faces.

Not less important as a distinctive mark of race is the hair. A single hair now enables the anthropologist to judge in what division of the human species he will class its owner; there is no mistaking a Chinese for a European, or either for an African. The cross-section of this single hair, examined microscopically by Pruner's method, shows it circular, or oval, or reniform; its follicle-curvature may be estimated by the average diameter of the curls as proposed by Moseley; its colouring matter may be estimated by Sorby's method. There has been even a systematic classification of man published by Dr. W. Müller, of the Novara Expedition, which is primarily arranged according to hair, in straight-haired races, curly-haired races, &c., with a secondary division according to language. Though we cannot regard such a system as good, the wonder is that it should answer so well as it does; indeed nothing could prove more clearly how real race-distinctions are, that a single bodily character should form a basis for rationally mapping out the divisions of mankind.

It is now well understood that the causes of race-colour are not so simple as Hippocrates thought when he described the nomad Scythians as burned tawny by the cold. But the study of anthropologists is still to notice the characters which mark off the white, yellow, brown, and black races, and to connect therewith the effects of climate and mode of life. The analogy of fair or blond skin to partial albinism is striking, and possibly points to some similarity of cause. A book has even been written by Dr. Poesche to explain thus the formation of the white race. The fair whites, according to this author, are semi-albinos, whose ancestors were once a browner race in Northern Asia, but turned fair in the swampy regions of the Dneiper, where men and beasts grow light in colour, horses grey, the leaves of the trees pale, and all nature dull and colourless. Such imaginative speculation is an example to be avoided by anthropologists, and yet the resemblance of blond to semi-albino skin is one which when worked out by careful observation will doubtless lead to discovery. A yet more striking case of the morbid appearance of race-character is seen in "bronzed skin," a symptom of "Addison's disease." Here the resemblance to mulatto complexion is so marked that in the reports of cases it is quite a regular thing for the physician to mention that he asked the patient if he was of negro blood. Even that well-known negro feature, the comparatively light tint of palms and soles, was there, though there was wanting one of the points which anthropologists look to when they suspect negro ancestry, namely, the yellowness of what we characteristically call the "white" of the eye. It is not however on merely superficial comparison that this analogy depends. Anthropologists unfortunately do not always hear of medical work bearing on their studies, and it is but lately that I learnt from Dr. Wilson Fox that an interesting microscopic section of "bronzed skin" was published years ago by Mr. Hutchinson in the *Pathological Transactions*. All who compare this with Kölliker's section of normal negro skin must admit the extraordinary similarity of coloration, in the manner in which the deep brown pigment cells and grains line the surface of the papillæ of the dermis or true skin. I shall not be charged with propounding here a theory that black men are white men thus transformed, for, indeed, one incident of the obscure disease in question is that the patient always dies. The importance of the comparison lies in its bridging over the physiological differences of race, by showing that morbid action may bring about in one race results more or less analogous to the normal type in another.

The differences in race-characters among mankind are

far better known than are the causes which bring them about. Yet it would be too much to say that we do not know how to alter the type of a race. For instance, stature is one point of race-type, and we know by actual experience that if a population of the Yorkshire dales is brought in to live in factory towns, in two generations they are found to be $1\frac{1}{2}$ inch lower in average stature than their countrybred kinsfolk. Indeed, it appears from Beddoe's careful statistics that the stature of the London population is gradually lessening. The great means of change of race-type is acclimatisation Dr. Acland has here called attention to the interesting problem presented by the tribes of "unhealthy districts" in India, who live where tribes allied to them in race and language cannot exist, nor can they themselves go back, without falling sick, to the plains whence their ancestors came. That this acclimatisation affects the secretions and hue of skin is certain, but this topic is one on which only a pathologist can speak with any authority. If, however, we look at the map of the world, it is as evident to us as it was to Hippocrates that race depends in some measure on climate and mode of life. The leading fact is the lie of the negro type along the equator, as contrasted with the xanthous or blond type in the northern temperate zone. The permanence of the races of mankind, such as the Egyptian, which the polygenist school interpreted as evidence that it was a species by itself, is better explained in Draper's words that "its durability arises from its perfect correspondence with its environment." It is only when moved into different conditions that a race has to change into harmony with these new conditions.

Turning now from the development of races to the development of their civilisation, the task is made easy by the help of evidence geological in its character. The presence of stone implements in every part of the world proves that they were once used there, and that the races using them had no metal. But now stone implements are distinguished into the ruder Palæolithic and the more finished Neolithic. The ruder, discovered in gravels of great antiquity with the remains of the mammoth and other prehistoric animals in Europe, must therefore be the older, but this also seems to be evident from their very nature. If men with bronze weapons had no more bronze, they might very likely fall back on the best substitute they could make, the hard, ground stone celt; but it seems against all reason that those who knew how to grind a hatchet on a whetstone should have lost that simple if laborious art. Thus culture confirms what geology teaches, that the rude stage of man's history to which the rude implement belongs is also the earlier stage, and the higher polished implement comes later. It comes on indeed into modern times, for the general extinction of the Stone Age in Australia or America only dates from this century, and even at this day in Australia the traveller learns from the blackfellow how the rude chipped axe-flake is to be gummed to the helve, or the white hunter sits down in California to be shown how to chip out the neat obsidian arrowhead with the point of deerhorn. In a few ages after metal has come in, the new people forgets that the old people ever used such things. Thus it comes to pass that, across the world from Iceland to Japan, stone hatchets and arrowheads dug up in the ground are supposed to be the material weapons hurled or shot from the sky, whose flight is seen in the lightning-flash Such "thunderbolts" have for ages been valued for magical power, especially the appropriate uses of guarding against fire and inflammatory disease; Pythagoras was purified with a thunderbolt, and stone arrowheads form the centre-pieces of some of the most beautiful of Etruscan gold necklaces. Even a bronze implement may be taken for a thunderbolt by those who have forgotten its nature; the bronze celt here produced was dug up in Wiltshire, where the lightning had struck an oak, and it has since for many years been the magical thunderbolt of a west country hamlet.

Even where the old use dwindles and changes, survival in altered shape may keep on the old ideas: our own life is full of survivals. In ceremonial processions we still see the javelins and halberds belonging to war before gunpowder, and though the mace no longer smashes helmets, it remains as an emblem of power and dignity. Our books are ornamented with gilt lines which once represented the real cross-binding; as in perhaps the most modern of survivals, where the tape which bound the registered letter has dwindled to blue cross-lines printed on the envelope. Language is full of such records of the past; as when one hears people declare they do not care a *groat*, a *doit*, or a *rap*, when they would not recognise if they saw them these old-fashioned varieties of small change. Thus what with the lasting on of old things among outlying peoples, and what with the survival of them among the civilised world, the thread of connection is by no means lost from remotest times. For my own part, when I look at the utter likeness of the working processes of the mind among the races most different in skin, and when I see the resemblance of rude ideas and customs throughout the inhabited world, I cannot but think that much of the thought and habit of mankind not only goes back to the remote Palæolithic age, but that it may be older than the divisions of race which separate us from the Chinese or the Negro. Let me offer examples of a mental state yet surviving which may have its origin in the very childhood of mankind. Uneducated men, from the savage to the peasant, remain more or less in that childlike state of mind where the distinction between dreams and real events is not yet perfectly made; dreams seem to be visits from phantom souls of others coming to the sleeper, or excursions of his own phantom or soul away from his body. The state of primitive thought in which psychology thus grows out of the phenomena of dreams has perhaps never been better displayed than in a recent account by Mr Im Thurn in the *Journal of the Anthropological Institute* of his Indian boatmen in British Guiana. One morning a young Macusi was so enraged against him that he refused to stir, declaring that his master, without consideration for his weak health, had taken him out in the night and made him drag the canoe up a series of cataracts. Nothing would persuade him that it was only a dream, and it was long before he was sufficiently pacified to throw himself sullenly into the bottom of the canoe. Food was scarce, and such dreams in consequence frequent, so that morning after morning the Indians were complaining that some man (whom they named) had visited their hammocks in the night, and beaten or otherwise maltreated them. In the middle of one night Mr. Im Thurn was awakened by his headman, an Arawak named Sam, who addressed him in these bewildering words: "George speak me very bad, boss; you cut his bits." On explanation, it proved that Sam had dreamt that George, one of the men under him, had spoken impudently to him, and had come at once to his master to demand that the culprit should be punished by cutting so many bits (*i.e.* fourpenny pieces) off his wages.

This instance of mental rudeness comes from among tribes who are hardly above the savage level, but not less remarkable survivals of primitive thought may be found among peasants. Thus that most archaic practice, the burial of objects for the use of the dead in the future life, is still continued in Europe. One of the latest instances comes from the village of Lückendorf in Saxony, where the schoolmaster, Herr Kühne, describes how when a mother dies in childbirth, they bury in the coffin all she wants for the child gone before—the little earthen pipkin and spoon, and a supply of groats, the baby-clothes, with needle and thread, thimble and scissors to mend them, and even a tiny model of the mangle,

because it is too large to bury. This is in a Wendish district, where prehistoric customs are more obstinately kept up than in purely German parts. Nothing could more perfectly illustrate the early animistic belief in the ghost turning to ghostly use the phantoms of objects laid for it in the grave. Thus we have, parallel with the rude material life of the Stone Age, traces of a corresponding intellectual rudeness, belonging to ages when men had not learnt to distinguish dreams from events, or to realise the meaning of death.

The problem of the order in which the races of men were formed and attained such culture as they have is obscure and perplexed enough, but it has some illuminating facts. The method by which an anthropologist judges of the centre of civilisation of a race is much the same as that of the botanist who looks for the district where a widespread cultivated plant is found wild, as the potato is in Chile, which accordingly he takes to be at or near the centre of distribution; only he has to guard against the possibility of the wild plant being only a cultivated variety run wild. Let us now apply this method to the geography of the Negro race. The negro or negroid spread over the African continent have never risen high in civilisation, scarcely of themselves getting beyond the barbaric stage. But on the other hand they are never very low; they are tillers of the soil, herdsmen, iron-workers, and no negroid tribe has been found in a clearly primitive savage state. The Bushmen, belonging to an allied variety of man, are outcasts and savages by degradation. If however we look along the map of the world for the eastern branch of the black race, we find in the Andaman Islands and in New Guinea and other islands Negro types more or less assimilated to the African, but living at lower stages of culture such as are possible in the rank forest-lands of the equator. In these two districts are found the only well-authenticated accounts of tribes with no knowledge of any means of making fire. The Andamanese have not the fire drill or any such fire-making instrument, but carry burning brands about with them, and if by any chance they lost their fire, they could kindle it anew at their volcanoes. In an outlying district of New Guinea, Mikluho-Maclay has found a Papuan tribe who only carry fire-brands, and do not know the fire-drill of other districts. This indicates very low culture, whether they are representatives of an originally fireless state, or whether by mere inertness they have disused and forgotten so useful an art as firemaking. In these regions is perhaps the Negro centre whence, rising to a somewhat higher level of culture, the western branch spread over Africa. Let us now look at the white men from this point of view. There may be remains of Stone Age Whites, but there are no certain remains of White savages of a low order. We may well doubt if there ever were any White savages; it is more likely that the White men were developed late in the race-history of the world from ancestors already far on in civilisation; in fact, that this civilisation with its improved supply of food, its better housing and clothing, its higher intellectuality, was one main factor in the development of the White type. Here, however, it must be remembered that there is not a White race in the sense in which there is a Carib race or an Andaman race. It includes several race-types, and even the same language, such as English or German, may be spoken by men as blond as Danes or as dark as Sicilians. The fair-haired Scandinavian type has something of the definiteness of a true race; but as one travels south there appear, not well-defined sub-races, but darkening gradations of bewildering complexity. The most reasonable attempt to solve this intricate problem is Prof. Huxley's view that the White race is made up of fair-whites of the Northern or Scandinavian type, and dark-whites who are the result of ages of mixture between the fair-whites and the darker nations, though it is perhaps hardly prudent to limit these dark ancestors to one variety as he does. If now we cannot trace the White man down to the low level or primitive savagery, neither can we assign to him the great upward movement by which the barbarian passed into civilisation. It is not to the Aryan of Persia nor to the Semite of Syria that the art of writing belongs which brought on the new era of culture. The Egyptian whose hieroglyphics may be traced passing from picture into alphabet had his race-allies in people of North Africa, especially the Berbers of the north coast, people whom no elasticity of ethnological system would bring into the white race. Of the race-type of the old Babylonians, who shaped likewise rude pictures into wedge-phonetic signs, we know but little as yet; at any rate their speech was not Aryan, and the comparisons of Lenormant and Sayce have given some ground for connecting it with the Turanian language, belonging to a group of nations of whom one, the Chinese, had in remote antiquity worked out a civilisation of which the development of an imperfect phonetic writing formed part. If the great middle move in culture was made, not by any branch of the white race, but by races now represented by the Egyptian and the Chinese, it is not less clear that these nations came to the limit of their developing power. The white races had in remote antiquity risen high in barbaric culture when their contact with the darker nations who invented writing opened to them new intellectual paths. The Greeks found in the ancient Egyptian theology the gods of the four elements, but they transferred this thought from theology to philosophy, and developed from it the theory of elements and atoms which is the basis of modern chemistry. They found the Babylonians building terraced temples to the seven planets in the order of their periods, and this conception again they transferred from religion to science, founding on it the doctrine of planet-spheres which grew into mathematical astronomy. It may moderate our somewhat overweening estimate of our powers to remember that the white races cannot claim to be the original creators of literature and science, but from remote antiquity they began to show the combined power of acquiring and developing culture which has made them dominant among mankind.

(*To be continued.*)

PROFESSOR ARTHUR ROCHE

M. ARTHUR ROCHE, Professor of Mathematics and Astronomy at the Lycée of Montpellier, died at that town on April 18 last, in the sixty-third year of his age. M. Roche's name is most intimately associated with researches on the figures of planets and comets, and the cosmogonic theory of Laplace. In the report on the labours of Roche made to the Academy of Sciences last week by M. F. Tisserand, his memoirs were thus classified:—1. Various memoirs on the equilibrium of a homogeneous fluid mass subjected to certain conditions. These had special reference to the beautiful researches of mathematicians on the equilibrium of a homogeneous fluid mass, animated by a movement of rotation around its axis, the molecules of which are attracted according to the law of Newton. M. Roche proposed to determine the figure of equilibrium by taking into account a new force—the attraction exerted by a centre situated at a great distance. M. Roche worked out this idea with great success, applying it specially to the moon, to the satellites of Jupiter and Saturn, to comets, and generally to the evolution of the solar system. 2. Memoirs on the physical constitution of the terrestrial globe, in which he came to the conclusion that the density at the centre is nearly double the mean density. 3. Memoirs on the internal condition of the globe, in which M. Roche was led to pronounce against the complete fluidity of the interior. 4. Various memoirs on the figures of comets.

5. Essay on the constitution of the solar system, in which M. Roche attempted to develop the beautiful cosmogonic theory of Laplace, giving precision to certain points and modifying it in others. M. Roche was a Corresponding Member of the Academy of Sciences in the Section of Astronomy, and had been nominated as a candidate for the place vacant by the death of M. Liouville.

THE LATE MR. W. A. FORBES

MR. WILLIAM ALEXANDER FORBES, Fellow of St. John's College, Cambridge, Prosector to the Zoological Society of London, and Lecturer on Comparative Anatomy to Charing Cross Hospital, whose untimely death on the Niger we announced last week, was born at Cheltenham on June 24, 1855, the second son of Mr. J. S. Forbes, the well-known railway director. He was educated at Kensington School and Winchester College, which he entered at the early age of eleven. On leaving Winchester in 1872, Forbes passed a year at Aix-la-Chapelle studying German, and then became a student of the University of Edinburgh, where he pursued the regular medical course, paying special attention to zoology and botany, and commencing collections of insects and plants. In 1875 Forbes transferred his residence to London, and entered himself as a student of London University with the idea of taking a medical degree in the metropolis. Here he became quickly intimate with other zoologists, who were very soon attracted by the astounding general knowledge of zoology and the acute intelligence of one so young. By the advice of the late Prof. Garrod and other friends Mr. Forbes was induced in October, 1876, to leave London and to become an undergraduate of St. John's College, Cambridge, where he was subsequently elected Scholar, and took his B.A. degree with a First Class in the Natural Sciences Tripos in 1879. The post of Prosector to the Zoological Society of London having become vacant in October, 1879, by the lamented death of Prof. Garrod, Mr. Forbes was appointed (*omnium consensu*) to that office in the January following. Indeed he had been designated by Garrod on his deathbed as his most obvious and proper successor, and had been appointed his literary executor.

Mr. Forbes entered upon the duties of his office with characteristic energy, and during the three following sessions of the Zoological Society brought before the scientific meetings a series of most interesting and valuable communications derived from his studies of the animals that came under his examination. He had a happy knack of putting forward abstruse points of anatomy in an understandable form, and especially directed himself to the muscular structure and voice-organs of birds, in continuation of the researches of his predecessor Garrod on the same subjects. In the summer of 1880 Mr. Forbes made a short excursion to the forests of Pernambuco, Brazil, of which he published an account in the *Ibis* for 1881, and in the following year passed his holiday in the United States, in order to make the acquaintance of his American brethren in science and their collections. In July, 1882, he left England on what promised to be a splendid opportunity of visiting the eastern tropics with every advantage and without much risk. Detained at Shonga—a station some 400 miles up the Niger below Rebba—by the breaking down of his communications, Mr. Forbes fell a victim to dysentery on January 14 last, thus adding another name to the long list of martyrs of science in that deservedly dreaded climate.

Mr. Forbes's published works consist chiefly of papers in the *Proceedings of the Zoological Society* and the *Ibis*, altogether about sixty in number. He was editor of the memorial volume of collected scientific papers of his predecessor Garrod, and just before he left England in July last had finished the last sheets of an excellent memoir on the anatomy of the petrels—since published in the "Zoology of the *Challenger* Expedition." This piece of work was originally undertaken by Garrod, but had been left almost uncommenced at the decease of the latter.

Of Forbes's private qualities as a most efficient and ready fellow-worker, a most charming companion and a most sincere friend, the writer is able to testify, not only from personal experience, but also from the universal regret expressed at the unhappy end of so promising a naturalist. P. L. S.

RECENT INFLUENCE-MACHINES

SEVERAL modified types of influence-machine have recently been brought before the public, and as they are both cheaper and more efficient than the older forms of Töpler, Holtz, and Bertsch, will probably find general acceptance. Of the newer forms, those of Voss and of Wimshurst are illustrated in the accompanying cuts.

In the Voss machine, which may be regarded as a modified Töpler machine, there are two disks of varnished glass, one stationary, the other rotating in front of it on an axis which passes through a central hole through the fixed disk. A pair of pulleys with a strap provide the rapid movement necessary. At the back of the fixed disk are fixed two armatures or inductors of varnished paper,

FIG. 1.—Voss's Influence-Machine.

with a narrower central band of tinfoil. These armatures are connected on the right and left respectively with two metal clamps which nip on to the edge of the disk and turn round in front of the front plate, each being provided at this part with a little metallic brush. Upon the front of the rotating plate are fastened six or eight metal buttons at equal intervals. These buttons are touched as they rotate by the metallic brushes. Nearly perpendicular, and in front of the front disk, is a brass rod, which need not be insulated, also furnished with spikes at each end, and with a little metallic brush to touch the buttons of the rotating plate. The action of the machine is as follows:—If a small charge of electricity—say a positive charge—be imparted to one armature—say that on the left—the buttons as they move past will be acted on inductively, and if, while thus under the inductive influence of the positive charge, they are momentarily touched by an uninsulated conductor, they will pass on electrified with a charge of the opposite sign. If the front plate rotates in the clockwise direction, each button as it moves through its highest position towards the right will thus acquire a small negative charge which will be given up on arriving at the right side, the projecting arm conveying the charge to the armature at the back. But as the button passes on downwards it will be influenced inductively by the armature behind it, and when touched by the lower end of the vertical conductor, will assume a positive electrification.

On arriving at the left side it will therefore give up a small positive charge to the left armature, thus charging it more highly than before. Every button as it goes round thus conveys the charges induced in it to the appropriate armatures, and exalts their charge. A very few turns given to the handle suffice to charge those armatures to their fullest extent, so that they begin to discharge pale sparks over the disks. But now begins another action. From right to left in front of the front disk lies an insulating bar of ebonite, holding at each end another brass comb, each connected by a crossbar of brass to the knob of a small Leyden jar. As the charges in the armature rise they act again upon these conductors fixed in front of them, and charge the jars, one positively, the other negatively. A pair of dischargers with ebonite handles serve to discharge the jars when full, and with every turn of the winch, when the knobs of the dischargers are separated by a few centimetres' distance a torrent of sparks is generated. If the machine is kept free from damp and dust, no initial charge is necessary, as the slight friction of the brushes suffices to give and sustain the requisite preliminary electrification.

Wimshurst's influence-machine is even simpler, and if anything, more efficient. It is the result of a long experimental research carried out with great care and skill by Mr. J. Wimshurst, who is well known as an accomplished amateur electrician. The latest of the many combinations which Mr. Wimshurst has designed is depicted in Fig. 2. It consists of two disks of common window glass mounted upon a common spindle, and provided with driving gear by which an equal speed is given to each, but in opposite directions. Each disk (about fourteen inches in diameter in the smallest size) is well varnished with shellac varnish, and carries twelve narrow strips of thin sheet metal cemented at regular intervals apart. In front, at about 45°, is fixed a diagonal conductor armed at each end with a small brush of metallic bristles, which touch the metal strips as they rotate. At the back a similar diagonal conductor is fixed, exactly at right angles to that in front. Right and left are two upright pillars of glass or ebonite which carry each a pair of metallic combs, and serve also to support the dischargers which are carried in an arch over the disks. It appears that in this machine the metal strips affixed to the plates act both as inductors and as carriers. Suppose, for example, that the front plate is rotating clockwise, and the back plate counter-clockwise. If the metal strips descending from the summit on the left on the back disk are charged positively, the metal strips ascending on the front disk from the left will, as they pass under the momentary touch of the brush, acquire a negative charge. As these negatively charged strips of the front plate advance towards the right they will come to a point where they are opposite the upper end of the hinder diagonal conductor, and here, whilst still acting as carriers to bring the negative charge round to the right side, they will act as inductors, and will influence the strips of the back disk, which will, as they are in turn touched by the hinder brush, acquire positive charges. The strips on the front disk will therefore constantly carry a negative charge as they move over the top from left to right, and those of the back disk will carry a positive charge from right to left. In the lower halves of their respective rotations the inverse of these actions will hold good, the front carriers conveying positive charges from right to left, the back ones conveying negative charges from left to right. The result will of course be that the two main conductors on the left and right will become respectively positively and negatively charged. Theoretically, a small initial charge must be imparted to some one or more of the carriers or to one of the two main conductors. Practically, if dry and free from dust, the machine excites itself, and after a couple of turns

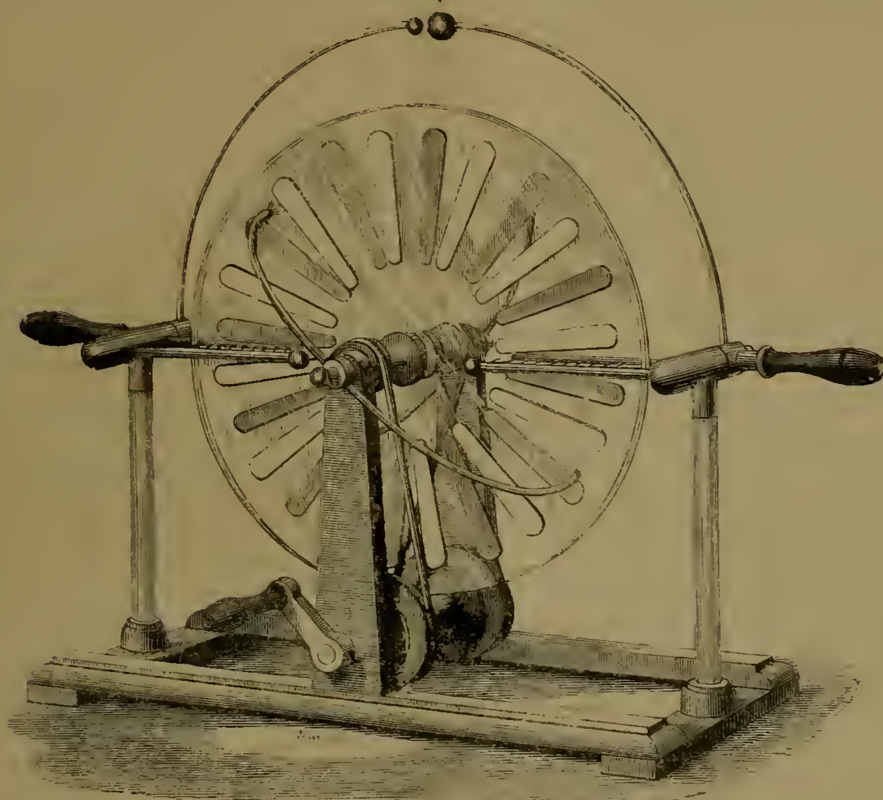

Fig. 2.—Wimshurst's Influence-Machine.

have been given to the handle, discharges sparks freely. If the two main conductors are respectively joined to the inner and outer coatings of a large Leyden jar, the discharges take place with short, loud sparks of great brilliancy. If from any cause the machine does not at once charge itself, a gentle rub with a silk handkerchief on either of the ebonite pillars will suffice to provide the requisite stimulus. The Wimshurst machine appears to be less liable than any other influence-machine to have the polarity of its charge reversed. It serves admirably for the production of the electric shadows discovered by Holtz and Righi. Mr. Wimshurst is much to be congratulated on the service he has rendered to experimental science in devising so useful and efficient an instrument.

THE ZENI NARRATIVE[1]

THERE is no greater puzzle in geographical literature than the so-called "Zeni narrative," which was published at Venice in 1558 by Francesco Marcolini, and claimed to be an authentic compilation by Nicolo Zeno of letters, in the possession of his family, which had been written at the close of the fourteenth century by two of his ancestors, the brothers Antonio and Nicolo Zeno, describing their adventures in the far north.

The story told by Nicolo Zeno was that when a boy he had found these letters in his father's palace, together with a map illustrating the travels of the Zeni brothers, and not knowing their value had torn many of them up. When he grew older he had however learnt to appreciate their true character, for like the rest of his family, one of the most illustrious in Venice, he was an accomplished scholar, and well acquainted with the results of geographical research. And collecting together all the letters that had escaped destruction, he compiled his narrative, and made a copy of the map, supplying from his own knowledge, and his interpretation of the travels of his ancestors, such names and other details as had become illegible from the then half-rotten condition of the original chart.

Ruscelli in 1561, and Moletius, one of the editors of Ramusio, in 1562, followed by the Venetian geographers generally, believed in the authenticity of the Zeni travels, as told by Nicolo the younger, who, as a Member of the Council of Ten, occupied one of the highest posts in the Republic, and was esteemed as a liberal patron of learning. But in other countries doubts were entertained in regard to the truth of the narrative, while in some quarters there arose an utterly untenable notion, that the book had been compiled with the object of securing to Venice the honour of having discovered the New World before Columbus set foot on it. In 1595 the Flemish geographer, G. Mercator, appeared as the first among many northern writers worthy of respect who refused to see in the story told by Nicolo Zeno anything more than a clever forgery. One of the latest, and probably the most formidable, of these detractors, was Admiral Zahrtmann, late Hydrographer to the Danish Admiralty. As an experienced seaman, an accomplished geographer, and a Dane well versed in the maritime history of the Danish Colonies with which he had long been intimately acquainted, he was eminently qualified to judge of the accuracy of a narrative, which professed to describe a voyage among islands and to regions, which the friends and foes of Zeno are alike agreed in believing we must recognise as the Faroe Isles, Iceland, and the eastern shores of Greenland. The substance of his careful analysis of the Zeni narrative, and of the map which accompanied it, was communicated in 1836 to the London Geographical Society, in the fifth volume of whose *Journal* it was subsequently published. And there is no doubt that notwithstanding the evidence that had been advanced in favour of the Zeni voyages by Hakluyt in 1600, and still more emphatically a century ago by Capt. Cook's companion, George Forster, English geographers allowed themselves to be powerfully influenced by the opinions of Zahrtmann. In our day, however, the tide of public favour has changed both abroad and in England. And in addition to the uncompromising testimony to the *bonâ fide* character and the general accuracy of the Zeno story, borne by Mr. R. H. Major in his edition for the Hakluyt Society, in 1873, of the "Voyages of the Zeni," and by M. G. Gravier in his "Découverte de l'Amérique par les Normands au 10ème Siècle," 1874, there is now the all-powerful evidence of Baron von Nordenskjöld to be adduced as corroborative, and seemingly conclusive, proof of the genuineness of this mysterious, and long-questioned story of early Venetian adventure in the northern seas.

While engaged in drawing up a history of north-eastern exploration for his "Voyage of the *Vega*," Nordenskjöld's attention was directed to the story of the Zeni voyages, of which he gives a Swedish translation in the number before us of the *Studier och Forskningar*, together with the result of his analysis of the narrative, and his comparison of the Zeno map with all the printed and manuscript maps known at the time of Marcolini's publication in 1558. Among the numerous interesting conclusions at which he has arrived, special attention is due to the following:—(1) That the general accuracy of the descriptions, for which there was no other known source, proves that the Zeni brothers must have been personally acquainted with the Faroe group and the other islands described in the narrative, as well as with the eastern shores of Greenland; and (2) that, considering the nature of the details given of the mode of life followed by the savages in regions lying in the north-west of the Atlantic, which are now known to us as Newfoundland, Canada, and the United States, but of which Europeans had no correct information until the colonisation of those lands in the seventeenth and eighteenth centuries, there is every reason to believe that the Venetian travellers conversed, as they assert, with persons who had visited these districts of the New World. Further, Baron von Nordenskjöld is of opinion that in the descriptions given by the Zeni's informants of the civilised communities, which they met with during their prolonged wanderings in these unknown western lands, we have evidence of the influence and persistence up to the close of the fourteenth century, when the Zeni are assumed to have been in the north, of the earlier Scandinavian colonies, which undoubtedly existed in the New World in the tenth and eleventh centuries.

The author shows that in the middle of the sixteenth century there were three maps in use, of the north and of the north-west, which, in addition to the Zeno map, had all been derived from northern sources, preceding the date of the discovery of America by Columbus. Of these the most important is a manuscript map, with descriptions of Northern Europe and of neighbouring lands, bearing the date of 1427, on which the Scandinavian countries are for the first time set down with anything like accuracy, and a considerable part of America is delineated. Our knowledge of this important pre-Columbian chart is entirely due to Baron von Nordenskjöld, who discovered it in a manuscript copy of Ptolemy's "Cosmographia," preserved in the Town Library of Nancy, of which he was permitted to make a facsimile, and to give a photographic copy in his *Studier och Forskningar*.

The value of this curious record of the geographical knowledge possessed in the early part of the fifteenth century of Scandinavia, and the adjoining seas, is increased by the fact that the map was laid down by a native of the Danish Island of Fyen, known as Claudius Clavus, or Cimbricus, who undertook the task for and

[1] "Studier och Forskningar, Föranledda af Mina Resor i Höga Norden; Ett Populärt Vetenskapligt Bihang til Vegas Färd kring Asien og Europa." A. E. Nordenskjöld. Häft 1. (Stockholm, 1883.)

at the instigation of the learned Cardinal, Gulielmus Filiastrus. Claudius' map, which is brightly coloured, and well supplied with the names and geographical determinations of places, ends at 74° N. lat., and begins at 55° N. lat., in which meridian a line is drawn through England, Holsatia (Holstein), and Pomerania, thus taking in the whole of the Baltic, whose islands and shores from the then Danish province of Halland, in Scandinavia, to the Gulf of Finland, are laid down with a fair amount of accuracy. In the far west we see Grönlandia, while on the shores of the Arctic Sea, named here "tenebrosum mare," we have at the very north of Scandinavia "Engrönuelandi," which would appear to have been an old designation of part of Finmark, and possibly the region from which Grönland derived its name.

In all respects the chart drawn by C. Clavus in 1427 is so far superior to the Donis map, printed at Ulm in 1482, which had formed the basis of Bordone's, and many other later maps, that, as Nordenskjöld points out, it must have been based on independent sources derived from the actual experience of seafaring observers. As, moreover, the Zeno map corresponds far more closely with the Clavus than with the Donis chart, with whose errors of position and distortions of outline it has little or nothing in common, there is not the slightest ground for asserting that the Benedictine monk, Nicolaus Donis, whose atlas is a mere copy of drawings to be found in the mediæval manuscripts of Ptolemy, was the authority from whom the younger Zeno derived his acquaintance with the far north, in which he included East Greenland and North-West America. We have no space to enter more fully into the interesting details with which Baron von Nordenskjöld supports his argument in favour of the authenticity of the Zeni narrative. But in conclusion we must draw attention to the success and ingenuity with which he has shown, that the often-sought-for and much-talked-of manuscript map of the north, which Admiral Zahrtmann saw in the University Library at Copenhagen, and declared to be the undoubted original from which Zeno's map had been derived, was simply a copy of Donis's chart. This fact he has so conclusively established, that henceforth Zahrtmann's charge against Zeno the younger must be considered to have lost one of its strongest supports; while future commentators on the Zeni voyages need no longer scour the libraries of Northern Europe in quest of a phantom map, whose disappearance soon after it had been seen by Zahrtmann has largely contributed to the tardy solution of the Zeno mystery.

NOTES

THE following awards will be made at the anniversary meeting of the Royal Geographical Society on the 28th inst.:—Founder's Medal to Sir Joseph Dalton Hooker, F.R.S., for his eminent services to scientific geography, extending through a long series of years and over a large portion of the globe, while engaged in voyages in the Antarctic and Australian Seas, and journeys in India and the Himalaya, in Morocco, and in the United States of America; and more especially for his long-continued researches in botanical geography, which have thrown light on the form of the land in prehistoric times, and on the causes of the present distribution of the various forms of vegetable life on the earth. Patron's Medal to E. Colborne Baber, Chinese Secretary of Legation, Peking, in recognition of the great value of his scientific work, chiefly geographical, during many exploratory journeys in the interior of China; and for his reports of these journeys, drawn up with admirable skill, accuracy, and completeness, which he presented to the Society, and which have been published, together with route maps engraved from his own finished drawings, in the first part of the "Supplementary Papers." The Murchison Grant for 1883 to Wm. Deans Cowan for his extensive surveys in the Tanala, Betsileo, and Bara provinces of Central Madagascar, an account of which was read by him to the Society in June, 1882, and published in the September number of the *Proceedings* of the same year; also as an encouragement to him in the new journey of exploration he is about to undertake in Western Madagascar. The Back Grant for 1883 to the Abbé Petitot for his geographical and ethnographical researches in the region of the great lakes of the Arctic basin, between Great Slave Lake and the Polar Sea, and his map of the basin of the Mackenzie. The Cuthbert Peek Grant for 1883 to F. C. Selous in acknowledgment of the value of his geographical researches in South Central Africa, including a journey in 1877 through the Manica country, north of the Zambesi, an examination of the hydrographical system of the Chobe, and two journeys by previously untrodden routes through Mashonaland, carefully prepared maps of which he communicated to the Society; also as an encouragement to him in the further researches in geography and natural history he has undertaken in the same region. The following will be elected as honorary corresponding members: Duca di Sermoneta (Prince Teano), president of the Italian Geographical Society, and of the International Geographical Congress at Venice, 1881; Dr. Schweinfurth, the eminent African traveller, now resident at Cairo; Edwin R. Heath, M.D., the explorer of the Beni River, South America, now residing at Wyandotte, Kansas, United States.

THE annual *soirée* of the Royal Society was held on the 25th ult. in the absence of the President, on account of indisposition. Among the recent scientific work illustrated was a photograph of the nebula in Orion, exhibited by Mr. A. A. Common, which is certainly one of the most interesting astronomical photographs which has ever been taken. We may also mention an interesting exhibit by Mr. W. Galloway, exemplifying the effects of coal-dust in colliery explosions, and "The Firedamp Cap," a phenomenon seen in mines. The only other exhibit of real general interest were some garlands from the tombs of Rameses II. and other kings, whose mummies were recently found at Thebes. Many of our readers have doubtless seen them in Egypt at the famous Boolak Museum, but those who have not done so must thank Dr. Schweinfurth for sending them over to Sir Joseph Hooker, and Sir Joseph Hooker for exhibiting them. These garlands are chiefly formed of leaves of *Mimusops Schimperi*, and petals of *Nymphœa cœrulea* and Lotus sewn together with fibres of date-leaf; others of the leaves of *Salix safraf*, with pods and flowers of *Acacia Nilotica*, *Sesbania Egyptiaca*, and *Carthamus tinctorius*, and petals of *Alcea ficifolia*.

DR. HANS GADOW has been appointed to the Strickland Curatorship in the University of Cambridge, vacated by the resignation of Mr. Salvin, F.R.S. Dr. Gadow began his biological studies under the late Prof. Peters in the University of Berlin, but graduated at Jena, whence he proceeded to Heidelberg, and worked there under Prof. Gegenbaur. Coming to England about two years ago, he was engaged, at the suggestion of Dr. Günther, by the Trustees of the British Museum to determine the specimens to be included in volumes viii. and ix. of their collection. The product of his labours in this direction is still in the press, but his contributions to the *Journal für Ornithologie*, the *Proceedings of the Zoological Society*, and other scientific journals, show him to be one of the most promising of the rising generation of ornithologists. In October last Dr. Gadow was appointed to deliver a course of lectures on the Morphology of the Vertebrata in the University of Cambridge, which has given much satisfaction to all concerned.

WE regret to announce the death of Dr. Wilhelm Peters, Professor of Zoology at Berlin University, and Director of the Zoological Museum of that city. He died on April 20, aged sixty-seven. The death is also announced of Dr. Gustav Radicke, Professor of Mathematics at Bonn University. He died at Bonn on April 18, in his seventy-third year.

The President of the Parkes Museum, H.R.H. the Duke of Albany, has fixed Saturday, May 26, for the opening of the Museum in its new premises, 74A, Margaret Street, W. The central position of the new premises will make the Museum more useful than it has hitherto been to professional men, owners of property, employers of labour, artisans and others, both men and women; and in order that the benefits of the Museum may be extended to all classes, it will be open daily between the hours of ten and seven, during which hours admission will be free, from five to seven and from two to nine on Mondays and Saturdays; while free admission to the library and reading-room may always be had on the recommendation of a member.

The honour of a baronetcy has been conferred upon Mr. Spencer Wells.

Messrs. Macmillan and Co. are about to publish "Elementary Lessons in Practical Physics," by Prof. Balfour Stewart and Mr. Gee, Demonstrator in the Physical Laboratory at Owens College, Manchester.

The *Indian Pioneer* states that a member of the Alpine Club, attended by an experienced Swiss guide, has left Darjeeling, for the purpose of attempting the highest possible ascent of the Kinchinjunger. The task will be a hard one, especially as the difficulties to be overcome are in many respects altogether different from those encountered in Switzerland.

A tornado of wide range and great force swept over the states of Mississippi, Georgia, and South Carolina on Sunday week, killing large numbers of people and injuring many more, and destroying hundreds of buildings. The first place struck is stated to have been Georgetown, Mississippi. The tornado is said to have cut a path 1000 yards wide through a swamp in Barnwell county, South Carolina, felling the timber as neatly as if it had been cut to form a highway.

The diary of the Marquis Tseng, Chinese Minister in London, to which attention has been already drawn in the *Pall Mall Gazette*, contains one or two passages which will be of especial interest to readers of Nature. His Excellency is in favour of the acquisition of a knowledge of foreign languages by Chinese youth; he thinks that, "if young people with good vocal organs were made to apply themselves, during the intervals of school duties, to the study of a foreign language, they could gain a fluent knowledge of it in four or five years." The sudden withdrawal of the Chinese educational mission in the United States a year ago was the subject of much astonishment abroad, but the Envoy's views on the subject before the mission was despatched in the first instance, will explain the mystery. "The result of sending boys who had not studied their own classics to devote themselves exclusively to the acquisition of Western knowledge in a country like America, where there was no distinction of classes, would be simply to contribute so many citizens to the United States, and to furnish the foreign firms at the Treaty Ports with compradores and interpreters." The advantages derived by the youths in America were far less than the successes of the pupils at the Foreign College in Peking and the schools at Shanghai and Foochow. A Mr. Chang, whom the envoy met in Shanghai, and whose opinion he seems to have valued highly, suggested the establishment at Government expense of a Chinese school for foreigners, where a knowledge of the Chinese language and literature might be attained. The students, he hoped, would translate foreign books for diffusion in China. In addition translations of the educational curriculum used in schools and colleges in the West should be made, and schools where young Chinese might be trained "upon the system practised in olden times, with a slight admixture of foreign methods," should be established. "Education," Mr. Chang says, "is the basis of State administration, and its success is essential to the establishment of proper government." Marquis Tseng does not precisely claim that China in times past had steamers and steam engines, although his language at first sight seems capable of such interpretation; he says, however, that China had no lack of mechanical appliances until her material prosperity declined, when her people fell into idle and thriftless habits, and the mechanical art was lost in transmission. He prophecies that the day will arrive here as it has in China, "when Western workcraft, now so deft, will grow inept, and Western ingenuity give way to homelike simplicity. The fact is," he concludes, "the earth's productions being limited, are not sufficient to provide for the manifold wants of its countless people, and deterioration is one of nature's laws." His Excellency is clearly a man of remarkable shrewdness and capacity; let us hope that to his other gifts he does not add that of prophecy.

The *North China Herald* reports that Dr. Bretschneider, the physician to the Russian Mission in Peking, and one of the ablest and most industrious students of China, is about to leave that country for ever. Dr. Bretschneider is, we believe, chiefly a botanist, and a few months ago we noticed an elaborate paper of his on Chinese botanical knowledge; but he has laboured in many other fields of research. One of his best known works is a pamphlet on the Early Chinese Travellers in Central Asia, which was published a few years ago. The same journal states that this gentleman, although he has already published much, is reserving his *magnum opus* until his return to Europe. The great advantage of sinologues working in China and Chinese literature on the subjects of which they are otherwise masters is obvious. Thus a botanist, with a knowledge of Chinese, will clearly work to greater advantage on Chinese knowledge of botany, the flora of China, and similar subjects, than he will in any other subject, or than a non-botanical Chinese scholar can do. Dr. Bretschneider seems during his long residence in China to have recognised this, and certainly in his hands the already great scientific reputation of the Russians in Peking has not suffered.

The work of education in Hong Kong would appear to be conducted under some curious difficulties. Dr. Eitel, the Inspector of Schools, in his last report mentions that he noticed several cases in which Chinese girls, living at a great distance from school, and having to traverse on their way to and fro the most crowded portion of the town, were dressed like boys, and attended the girls' schools all through the year in boys' dresses. This was owing to the prevalence of the practice of kidnapping girls, and the curious change of dress was adopted to deceive the kidnappers.

We notice in M. Bunge's review of "European Literature in Chemical Technology," published in the *Journal of the Russian Chemical Society*, the appearance of an elaborate Russian work, by M. Radivanovsky, on "Gunpowder, Pyroxyline, Dynamite, and other Explosives," in two large volumes, one of which is devoted to theory, and the other to practice. M. Bunge considers it as decidedly the best work on the subject in Europe for its completeness and lucidity of exposition.

M. Yagu, of the Russian Physical Society, while making experiments with a new parachute-hydromotor on the Neva, came to the unexpected result that the velocity of the current in this river is only half the rate in winter that it is during the summer. It is supposed that this retardation depends upon accumulations of ice at the outflow of the Neva from Lake Ladoga, which accumulations diminish the section of the channel.

M. Pompeieu has made, before an immense crowd, two successful ascents with an elongated balloon (measurement 1300 cubic metres, elongation 1 to 3½). On both occasions the

descent was very well executed, although the balloon was partly empty, having ascended to an altitude of 1200 metres. Four persons were on board. In the second ascent M. Pompeieu obtained a movement of his aërial craft in the required direction by only moving his rudder. This circumstance is accounted for by the balloon progressing with a less velocity than the wind, owing to its elongation.

THE French Military Engineers have suggested a scheme for extending the area of Paris by suppressing the old fortifications, which cover 2000 acres, and could be sold for building-ground. The proposal is simply to connect the several forts built by Louis Philippe by a trench sufficient to prevent a sudden attack. This new line of defence would utilise the Seine and Marne as a defensive work. The total area of Paris would then be 100 square miles instead of 30, as at present.

THE Anniversary Meeting of the Zoological Society was held on April 30, Prof. W. H. Flower, LL.D., F.R.S., President, in the chair. The Report of the Council on the proceedings of the Society during the year 1882 was read by Mr. P. L. Sclater, F.R.S., the Secretary of the Society. The Report stated that the number of Fellows on December 31, 1882, was 3213, the same as at the corresponding period in 1881. The total receipts for 1882 had amounted to 34,270*l.*, against 25,810*l.* for 1881. The ordinary expenditure for 1882 had been 26,109*l.*, against 24,651*l.* for 1881, and the extraordinary expenditure 3266*l.*, against 1036*l.* for the preceding year; besides which the sum of 1000*l.* had been devoted to the repayment of part of the mortgage debt due on the Society's freehold premises, which had been thus reduced to 5000*l.* The balance carried forward for the benefit of the present year was 3891*l.* The most important work undertaken in the Gardens during the past year had been the new Reptile House, a site for which, in the south-eastern corner of the Gardens, had been selected some time since. The building was stated to be 120 feet long, by 60 feet in width. Fixed cages for the pythons and larger reptiles would occupy three sides, while the south front was reserved for small movable cases. A large oval tank for crocodiles and two smaller ones for water tortoises would be placed in the centre of the building, which it was hoped would be ready for opening in July or August next. The visitors to the Society's Gardens in 1882 had been 849,776, against 648,694 in 1881, the number having been unusually augmented by the excitement caused at the removal of the large African elephant, "Jumbo," in the beginning of the year. The number of animals in the Society's Collection on December 31 last was 2355, of which 750 were mammals, 1364 birds, and 241 reptiles. The usual ballot having been taken, it was announced that Prof. Bush, F.R.S., Major-General Henry Clerk, R.A., F.R.S., the Hon. J. S. Gathorne-Hardy, Mr. Arthur Grote, and Lord Walsingham, had been elected into the Council in place of the retiring Members, and that Prof. W. H. Flower, LL.D., F.R.S., had been reelected President, Mr. Charles Drummond, Treasurer, and Mr. Philip Lutley Sclater, M.A., Ph.D., F.R.S., Secretary to the Society for the ensuing year. The meeting terminated with the usual vote of thanks to the Chairman, in returning thanks for which Prof. Flower called attention to the loss the Society had suffered by the death of two distinguished Foreign Members (Prof. Troschel and Dr. W. Peters), and more recently by the death of the accomplished Prosector, Mr. W. A. Forbes, at the early age of twenty-eight years.

THE Sunday Society opened the Suffolk Street Galleries on Sunday for four hours to persons who had previously written for tickets. The number of visitors was 1695 (from two to four there were 495, and from six to eight the attendance was 1200). During the evening a meeting was held in the large gallery, Mr. Mark H. Judge in the chair. On the motion of Mr. Hastings Sands, seconded by Mr. Robson J. Scott, a petition in support of Lord Dunraven's resolution was unanimously passed. The annual meeting of the Society will be held on Saturday at the Princes' Hall, Piccadilly.

THE Charing Cross and Waterloo Electric Railway Bill has been withdrawn for the present session.

ON April 8, at 9 p.m., an earthquake was observed in Finland, where this phenomenon is extremely rare. At Nykarleby the shocks were rather severe, and were accompanied by a subterranean rattling and rumbling noise; their direction was from S.W. to N.E. At Wasa the ground oscillated to an alarming extent. At Ytterjeppo even the houses were shaken to their foundations and their downfall was feared; the same intensity in the shocks was observed on the "domaine" of Back.

MR. R. MELDOLA writes to say that an error has inadvertently crept into his address, referred to in last week's NATURE (p. 615). The remark quoted was not made with reference to Mr. Wallace's paper, published by the Linnean Society in August, 1858, but with reference to his first paper, "On the Law which has Regulated the Introduction of New Species," published in the *Annals and Magazine of Natural History* for September, 1855. This mistake, however, does not affect the general tenor of our paragraph.

A PAPER issued by the Isthmus of Corinth Canal Company states that the explosions of the mines will be made with an electric machine moved by hand and Leyden jar. The total weight of dynamite required will amount to 2,500,000 pounds. The work is expected to last four years, and to cost about 1,100,000*l.* The canal will be 6300 metres in length, 22 in breadth, and 8 in depth.

THE Rev. James Sibree has issued in a separate form his instructive paper on Malagasy Place-Names, which originally appeared in the *Journal* of the Royal Asiatic Society.

DR. RUDOLF FALB, the well-known author of various works on earthquakes and volcanoes, has recently written an interesting little book entitled "Wetterbriefe." It contains reflections on meteorology, with special reference to the inundations of 1882, which the author considers to be periodical. The book is published by Hartleben of Vienna.

DR. JOS. CHAVANNE'S edition of Adrian Balbi's "Allgemeine Erdbeschreibung," to which we have already referred some time ago, and which is in course of publication by Hartleben (Vienna), has now reached the twenty-fourth part. It will be completed in forty-five parts.

THE illustration of the "Lion at Rest," lent to us by our Paris contemporary, *La Nature*, which appeared in our issue of April 19, was, we are now informed, engraved from a photograph by Mr. Thomas James Dixon, the copyright of which belongs to Mr. Henry Dixon, of 112, Albany Street, Regent's Park.

THE additions to the Zoological Society's Gardens during the past week include a Macaque Monkey (*Macacus cynomolgus*) from India, presented by Mr. H. G. Wainwright; a Leopard (*Felis pardus* ♂) from East Africa, presented by Capt. Percy Luxmore, R.N., C.B.; a Brown Bear (*Ursus arctos* ♂) from Kamschatka, presented by Mr. C. T. Kettlewell; a Ring-tailed Coati (*Nasua rufa*) from South America, presented by Mr. Dudley Sheridan; a Common Badger (*Meles taxus*), British, presented by Mr. J. Snowden Henry, F.Z.S.; a Woodcock (*Scolopax rusticola*), British, presented by Capt. Nicholls; two Edible Snails (*Helix pomatia*) from Cheltenham, presented by Lieut.-Col. C. S. Sturt, C.M.Z.S.; an Ashy-black Macaque (*Macacus ocreatus*) from the East Indies, a Senegal Parrot (*Pœocephalus senegalensis*) from West Africa, deposited; a Great Anteater (*Myrmecophaga jubata*) from Brazil, a Common Sparrow Hawk (*Accipiter nisus*), British, purchased.

CHEMICAL NOTES

THAT the statement of the "law of isomorphism" given by Mitscherlich is not applicable to all cases of isomorphous salts has been recognised for some time. M. Klein has recently described certain pairs of salts which crystallise in identical forms, but are not of similar chemical composition; thus *tungsto-boric acid*, $9WO_3 . B_2O_3 . 2H_2O$, is isomorphous with *silico-tungstic acid*, $12WO_3 . SiO_2 . 4H_2O$. M. Klein proposes to state the *law of isomorphism* in the following terms:—"Isomorphous bodies have either similar chemical composition, or exhibit only small differences in percentage composition; they contain either a common group of elements, or groups of elements of identical chemical formation which form by far the greater part of their weight" (*Compt. Rend.* xcv. 781).

THE rare metal thorium has been obtained in some quantity and in a pure state by Nilson. The properties of this metal are described in *Compt. Rend.* xcv. 727 *et seq.*: the sp. gr. is 11, and the atomic weight 232·36.

F. M. RAOULT (*Compt. Rend.* xcv. 1030) has studied the reduction of freezing-point of a liquid caused by the solution in it of a solid substance. He concludes that a molecule of any compound dissolved in 100 molecules of any liquid of a different nature lowers the freezing-point of the liquid by a nearly constant amount (about 0°·62). This law, he asserts, is general if it is admitted that physical molecules may be composed of two, and in some few cases of three chemical molecules.

WROBLEWSKI (whose experiments have been already referred to in these notes) states (*Ann. Phys. Chem.* [2], xix. 103) that if a little water is introduced into a tube containing CO_2, the whole cooled to 0°, the pressure increased till the CO_2 liquefies, and then suddenly released, care being taken that the pressure does not fall below 12·3 atmospheres, a thin opaque solid forms on the surface of the water, which solid is a definite hydrate of carbon dioxide. Further experiments are detailed, showing that the probable formula of this hydrate is $CO_2 8H_2O$.

M. SPRING continues his investigation on the influence of great pressure on chemical action (*Berichte*, xvi. 324). He has succeeded in preparing definite arsenides of zinc, lead, tin, cadmium, copper, and silver.

A NEW method for preparing the paraffins (C_nH_{2n+2}) has been found by Herr Köhnlein, a student in Prof. Lothar Meyer's laboratory at Tübingen; the method consists in heating together pure dry aluminium chloride and the normal iodide of the paraffin radicle required; *e.g.* $AlCl_3$ and C_3H_7I yield pure C_3H_8; $AlCl_3$ and C_2H_5I yield pure C_2H_6, &c.

AFTER having published his important work on the etherisation of alcohols, Prof. Menshutkin now publishes in the *Journal of the Russian Chemical Society* a new paper on the methods of qualitative determination of aniline and analogous bases which have no alkaline reaction, as well as of triethylamine and similar bases, and of ammonia. All these methods are a generalisation of the method of alkalimetry, and the discovery of them has afforded the author the possibility of studying the classic reaction of the permutation of bases in solutions of their neutral salts. This last is the subject of his first paper. The reaction being made under the most simple unvarying physical conditions, M. Menshutkin begins with the study of complete permutations, and shows that the theory of Berthollet as to the influence of the chemical mass is not true with regard to aniline, which is completely substituted in salts by bases whose temperature of combination with hydrochloric acid is greater than for aniline; the same is true with regard to triethylamine, which is also substituted completely, notwithstanding the increase of its chemical mass, and to ammonia. These researches have led the author to a new method of titration by means of the alcoholate of barium, and to a means of studying the formation and dissociation of acetylanilide, as well as of the amides.

ON THE SUPPOSED PRE-CAMBRIAN ROCKS OF ST. DAVID'S[1]

THE author began by briefly narrating the circumstances under which he had been led to study the geology of St. David's. He had visited the district twice—first in company with Mr. B. N. Peach, with whose cooperation nearly all the field work was done, and again in conjunction with Mr. W. Topley. The paper was divided into two parts, the first being mainly controversial, and the second descriptive.

According to Dr. Hicks, there are at St. David's three distinct pre-Cambrian formations: the "Dimetian," consisting of crystalline, gneissic, and granitoid rocks; the "Arvonian," formed of felsites, quartz-porphyries, hälleflintas, and other highly-silicated rocks; and the "Pebidian," composed of tuffs, volcanic breccias, and basic lavas. He regards the "Arvonian" as later than and unconformable to the "Dimetian," and the "Pebidian" as younger than, and unconformable to both; and he asserts that the basement conglomerate of the Cambrian system lies quite unconformably on all these rocks, and is in great part made up out of their waste.

Taking up each of these groups in the order of sequence assigned to them, the author maintained that the "Dimetian group" is an eruptive granite, which has disrupted and altered the Cambrian strata, even above the horizon of the supposed basal conglomerate. He described a series of natural sections where this relation is exposed, particularly one on the coast at Ogof-Llesugn, where the conglomerate has been torn off and involved in the granite, and has been intensely indurated, so as to become a kind of pebbly quartzite. No other rock occurs within the granite mass except dykes of diabase, which rise through all the rocks of the district, but are especially abundant in the granite. The veins of finer granite, so general in granite areas are conspicuous here. In short, whether studied in hand specimens or on the ground, the rock is so unmistakably an eruptive mass that the author could not understand how this view, which was that expressed on the Geological Survey maps, should ever have been called in question. The manner in which it has risen across the bedding of successive horizons in the Cambrian series proves that, instead of being a pre-Cambrian gneiss, it must be much younger than all the Cambrian rocks of the district.

The "Arvonian group" consists of quartziferous porphyries, or elvans, associated with the granite, and of the metamorphosed strata in their vicinity. Reference was made to natural sections where the actual intrusion of the elvans across the bedding of the rocks could be seen.

The "Pebidian group" comprises a series of volcanic tuffs and breccias, with interstratified and intrusive lavas. The author maintained that this group forms an integral part of the Cambrian system as developed at St. David's. It has been broken through by the granite and porphyries, and is therefore of older date. Instead of being covered unconformably by the Cambrian conglomerate, as asserted by Dr. Hicks, the volcanic group is overlain quite conformably by that rock; and seams of tuff are interstratified with the conglomerate and occur on various horizons above it. The conglomerate, instead of being mainly composed of fragments of the rocks beneath it, consists almost entirely of quartz and quartzite, only 4 per cent. of fragments having been found to have been derived from some of the projecting lava-islands underneath it.

From the evidence now brought forward, the author contended that as the names "Dimetian," "Arvonian," and "Pebidian" had been founded on error of observation, they ought to be dropped out of geological literature.

In the second part of his paper the author gave the results of the survey which he had made of the district with Messrs. Peach and Topley, and of his study of a series of more than 100 thin slices of the rocks collected at St. David's. He found that he could corroborate generally the descriptions of previous writers on the microscopic structure of the rocks, and that investigation with the microscope amply confirmed the deductions he had drawn from observations in the field.

1. *Order of Succession of the Rocks.*—The following rock-groups in the Lower Cambrian series are recognisable at St. David's, and are given in descending order:—

4. Purple and greenish grits, sandstones, and shales.
3. Green and red shales and sandstones, with thin tuffs (*Lingulella primæva*).
2. Quartz conglomerate.
1. Volcanic group (tuffs, schists, lavas).

The volcanic group forms the oldest part of the Cambrian series at this locality. The bottom is not reached, but about 1800 feet are visible. It consists mainly of purplish-red, green, grey, and pale tuffs, with occasional breccias and bands of olivine-diabase. Analyses of some of these rocks had been

[1] Abstract of a paper read at the Geological Society by Archibald Geikie, F.R.S.

made for the author by M. Renard of Brussels, and Mr. J. S. Grant Wilson of the Geological Survey of Scotland. The tuffs are partly basic, derived from the disruption of diabase lavas (48 per cent. of silica), partly acid, from the destruction of felsites (72 to 80 per cent. of silica). The microscopic structure of the tuffs was described, and slides and drawings were exhibited. The lavas are varieties of olivine-diabase. Their augite is remarkably abundant and fresh, and they contain scattered larger well-formed, as well as imperfect, crystals of olivine, generally in the form of hæmatitic pseudomorphs. No instance was observed of a siliceous lava having been erupted at the surface. The felsitic fragments in the tuffs must have been derived from the explosion of lavas that do not seem to have flowed out above ground. It was pointed out that this fact is exactly paralleled in the case of the volcanic group of the Lower Old Red Sandstone in the Pentland Hills.

In relation to the quartz-conglomerate, allusion was made to the constant recurrence of such conglomerates in the series of geological formations, and to the fact that they do not necessarily mark unconformability or the natural base of groups of sedimentary rocks.

2. *Geological Structure of the District.*—It was shown that the rocks have been folded into an isocline or inverted anticline, so that in one-half of the plication the dip of the strata is reversed.

The groups above mentioned are found in their proper order on both sides of the axis which runs through the volcanic group. The granite has risen irregularly through the eastern limb of the isocline. Small faults may occur here and there along the edge of the granite, but they do not in any way affect the general structure.

3. *The Foliation of the District.*—There has been extensively developed at St. David's a fine foliation of particular kinds of rock, more especially of certain fine tuffs and shales, which have passed into the condition of fine silky unctuous hydro-mica-schists or sericite-schists. A series of microscopic slices was described, which showed that the original clastic structure of the beds remains quite distinct, though an abundant development of fine flakes of a hydrous mica has taken place. This structure more particularly characterises the fine parts of the volcanic group, but it occurs also on various horizons in the groups above the conglomerate, thus linking the whole as one great continuous series of deposits. The author connected it with the plication of the district, and pointed out the great interest attaching to these fine schistose bands as revealing some of the incipient stages of the same process that had changed wide regions of sedimentary strata into crystalline schists.

4. *The Granite, Quartz-Porphyries, and accompanying Metamorphism.*—The petrographical characters of these eruptive rocks were described, and their perfect analogy to the familiar granites and elvans of other districts was pointed out. Specimens were shown illustrating the gradation from a true granite into spherulitic quartz-porphyry. The quartz-porphyries of St. David's (described by Mr. Davies, Dr. Hicks and others) exhibit spherulitic structure in an exceptionally perfect manner. Between the felso-spherulites the base is thoroughly micro-crystalline and not felsitic. The rocks belong to a group intermediate between granites and felsites. They occur in bosses, elvans, or dykes round the granite, cutting through all horizons of the volcanic group, and approaching, if they do not actually intersect, the quartz-conglomerate. The metamorphism associated with the granites and porphyries is best seen near the latter. It consists chiefly in the intense induration of certain bands of rock which have been converted into flinty aggregates (adinole). The alteration takes place usually along the bedding, which is nearly vertical; but veins of the same siliceous material ramify across the stratification of the shales. Examined microscopically, the adinole is found to have acquired a micro-crystalline structure, nests of quartz and orthoclase and porphyritic crystals of plagioclase having been developed, together with fine veins and filaments of crystalline quartz. These veins are here and there crowded with approximately parallel partitions of liquid inclusions showing freely moving bubbles. An analysis of a portion of the adinole, made for the author by M. Renard, shows the percentage of silica to be 78·62 with 5·80 of soda, indicating possibly the formation of albite. The author deferred generalising on the question of the metamorphism he described, but pointed out that a further study of the St. David's rocks could hardly fail to throw important light on the theory of metamorphism.

5. *The Diabase Dykes and Sheets.*—These are the latest rocks at St. David's, as they traverse all the others. Their macroscopic and microscopic characters were described, and allusion was made to the perfect fluxion-structure found in many of the dykes.

The paper closed with a summary of the geological history of St. David's. The earliest records are those of the volcanic group, which show the existence of volcanic vents in that region in an early part of the Lower Cambrian period. The volcanic accumulations were covered conformably by the conglomerate and succeeding Cambrian groups; but the same kind of tuffs continued to be ejected after the deposition of the conglomerate. At a later time this thick conformable succession of beds was plicated, and underwent a partial metamorphism, whereby some of the fine tuffs and shales were converted into sericite-schists. Subsequently a mass of granite rose through one side of the fold, accompanied by elvans of spherulitic quartz-porphyry, whereby a second, different, and feebler kind of metamorphism was induced. The last episode was that of the diabase dykes, which, crowded together in the granite, suggest that the granite boss stands on an old line of weakness and of escape for eruptive material from the interior.

As the conclusions drawn by the author from his study of the microscopic structure of the rocks of St. David's had been called in question at the reading of the first part of the paper, he took an opportunity before the reading of the second part to submit a series of typical specimens and microscopic slides to Professors Zirkel of Leipzig, Renard of Brussels, and Wichmann of Utrecht. These observers amply sustained his deductions. M. Renard came from Brussels to be present at the reading of the second part, and in the course of the discussion stated that Professors Zirkel, Wichmann, and himself had arrived at the following conclusions regarding the rocks of St. David's:—

1. The so-called "Dimetian" rock of St. David's is unquestionably a true granite. 2. The quartz-porphyries are just such rocks as might be expected to occur as apophyses of the granite, and the specimens from Bryn-y-Garn, Rock House, and St. David's left no doubt on their minds that such is really their origin. They cannot be confounded with rhyolitic lavas. 3. The conglomerate from the granite-contact shows secondary quartz between its pebbles. 4. The bands of fine tuff found intercalated with, and on various horizons above, the conglomerate, consist of true tuff, and cannot have been derived from the mere superficial waste of older volcanic rocks. 5. Fine foliation is well developed among the strata above the conglomerate as well as in the volcanic group below.

SOLAR PHYSICS[1]

THE lecturer introduced his subject by drawing attention to the circumstance that the idea of the sun being an exceedingly hot body was of very modern date, that both ancient and modern writers up to the early portion of the present century attributed to him a glorious and supernatural faculty of endowing us with light and heat of the degree necessary for our wellbeing, whilst even Sir William Herschel had attempted to find an explanation to account for his idea that the body of the sun might be at a low temperature, and inhabitable by beings similar to ourselves, which he did in surrounding the inhabitable surface by a non-conducting atmosphere—the penumbra—to separate it from the scorching influence of the exterior photosphere.

It was not till the views of Kant, the philosopher, had been developed by Laplace, the astronomer, in his famous "Mécanique Céleste," that the view gained ground that our central orb was a mass of matter in a state of incandescence, representing such an enormous aggregate as to continue radiation into space for an almost indefinite period of time.

The lecturer illustrated by means of a diagram the fact that of all the heat radiated away from the sun only 1/2,250,000,000 part could fall upon the surface of our earth, vegetation and force of every kind being attributable to this radiation, whilst all but this fractional proportion apparently went to waste.

Recent developments of scientific research had enabled us to know much more of the constitution of the sun and other heavenly bodies than had formerly been possible. Comte says in his "Positive Philosophy" (Martineau's translation of 1853) that "amongst the things impossible for us ever to know was that of telling what were the materials of which the sun was

[1] Abstract of Lecture at the Royal Institution, by Sir William Siemens, F.R.S., April 27.

composed" ; but within only seven years of that time Messrs. Bunsen and Kirchhoff published their famous research showing that, by connecting the dark Fraunhofer lines of the solar spectrum with the bright lines observed in the spectra of various metals, it was possible to prove the existence of those substances in the solar photosphere, thus laying the foundation of spectrum analysis, the greatest achievement of modern science. Dr. Huggins and others, applying this mode of research to other heavenly bodies, including the distant nebulæ, had extended our chemical knowledge of them in a measure truly marvellous.

Solar observation had thus led to an analytical method by which chemistry had been revolutionised, and it would be, in the lecturer's opinion, through solar observation that we should attain to a much more perfect conception of the nature and effect of radiant energy, in its three forms of heat, light, and actinism, than we could as yet boast of. The imperfection of our knowledge in this respect was proved by the circumstance that whereas some astronomers and physicists, including Waterston, Secchi, and Ericsson, had, in following Sir Isaac Newton's hypothesis, attributed to the sun a temperature of several millions of degrees Centigrade, others, including Pouillet and Vicaire, in following Dulong and Petit, had fixed it below 1800° C. ; between these two extremes other determinations based upon different assumptions had placed the solar temperature at between 60,000° and 20,000°.

The lecturer, having conceived a process by which solar energy may be thought self-sustaining, had felt much interested for some years in the question of solar temperature. If the temperature of the solar photosphere should exceed 3000° C., combustion of hydrogen would be prevented by the law of dissociation, as enunciated by Bunsen and Sainte-Claire Deville, and his speculative views regarding thermal maintenance must fall to the ground. To test the question he in the first place mounted a parabolic reflector on a heliostat, with a view of concentrating solar rays within its focus, which, barring comparatively small losses by absorption in the atmosphere and in the metallic substance of the reflector should reproduce approximately the solar temperature. By introducing a rod of carbon through a hole at the apex of the reflector until it reached the focus, its tip became vividly luminous, producing a light comparable to electric light. When a gas burner was arranged in such a way that the gas flame played across the focal area, combustion appeared to be retarded but was not arrested, showing that the utmost temperature attained in the focus did not exceed materially that producible in a Deville oxyhydrogen furnace or in the lecturer's regenerative gas furnace, in which the limit of dissociation is also reached.

Having thus far satisfied himself, his next step was to ascertain whether terrestrial sources of radiant energy were capable of imitating solar action in effecting the decomposition of carbonic acid and aqueous vapour in the leaf-cells of plants, which led him to undertake a series of researches on electro-horticulture extending over three years, a subject which he had brought before the Royal Society and the Royal Institution two years ago. By these researches he had proved that the electric arc possessed not only all the rays necessary to plant-life, but that a portion of its rays (the ultra-violet) exceeded in intensity the effective limit, and had to be absorbed by filtration through clear glass, which, as Prof. Stokes had shown, produced this effect without interference with the yellow and other luminous and intense heat-rays. He next endeavoured to estimate the solar temperature by instituting a comparison between the spectra due to different known luminous intensities. Starting with the researches of Prof. Tyndall on radiant energy, supplementing them by experiments of his own on electric arcs of great power, and calling to his aid Prof. Langley of the Alleghany Observatory to produce for him a complete spectrum of an Argand burner, he concluded that with the temperature of a radiant source the proportion of luminous rays increased in a certain ratio : whereas in an Argand oil-burner only 2½ per cent. of the rays emitted were luminous, and mostly red and yellow, a bright gas flame emitted 5 per cent., the carbon thread of an incandescent electric light between 5 and 6 per cent , a small electric arc 10 per cent., and in a powerful 5000-candle electric arc as much as 25 per cent. of the total radiation was of the luminous kind. Prof. Langley, in taking his photometer and bolometer up the Whitley Mountain, 18,000 feet high, had proved that of the solar energy not more than 25 per cent. was of the luminous kind, and that the loss of solar energy sustained between our atmosphere and the sun was chiefly of the ultra-violet kind, which rays, if they penetrated our atmosphere, would render vegetation impossible. It was thus shown that the temperature of the solar photosphere could not materially exceed that of a powerful electric arc or indeed of the furnaces previously alluded to, leading him to the conclusion already foreshadowed by Sainte-Claire Deville and accepted by Sir William Thomson, that the solar temperature could not exceed 3000° C. The energy emitted from a source much exceeding this limit would no longer be luminous, but consist mainly of ultra-violet rays, rendering the sun invisible, but scorching and destructive of all life.

Not satisfied with these inferential proofs, the lecturer had endeavoured to establish a definite ratio between temperature and radiation, which formed the subject of a very recent communication to the Royal Society. It consisted simply in heating a platinum or iridio-platinum wire, a metre long and suspended between binding screws, by means of an electric current, the energy of which was measured by two instruments, an electro-dynamometer giving the current in amperes, and a galvanometer of high resistance giving the electromotive force between the same points in volts. The product of the two readings gave the volt-amperes or watts of energy communicated to the wire, and dispersed from it by radiation and convection. A reference to the lecturer's paper on the Electrical Resistance Thermometer, which formed the Bakerian Lecture of the Royal Society in 1871, would show that the varying electromotive force in volts observed on the galvanometer was a true index of the temperature of the wire, while being heated by the passage of the current ; a law of increase of radiation with temperature was thus established experimentally up to the melting-point of iridio-platinum, which when laid down in the form of a diagram gave very consistent results expressible by the simple formula—

$$\text{Radiation} = Mt^2 + \phi t,$$

M being a coefficient due to substance radiating.

Sir William Thomson had lately shown that the total radiating energy from a unit of surface of the carbon of the incandescent lamp amounted to 1/67 part of the energy emitted from the same area of the solar photosphere, and taking the temperature of the incandescent carbon at 1800° C. (the melting-point of platinum which can just be heated to the same point), it follows in applying Sir William Thomson's deductions to the lecturer's formula that the solar photosphere does not exceed 2700° C., or, adding for absorption of energy between us and the sun, about 2800° C.—a temperature already arrived at by different methods. The character of the curve was that of a parabola slightly tipped forward, and if the ratio given by that curve held good absolutely beyond the melting-point of platinum iridium, it would lead to the conclusion that at a point exceeding 3000° C. radiation would become as it were explosive in its character, rendering a rise of temperature beyond that limit difficult to conceive.

Clausius had proved that the temperature obtainable in a focus could never exceed that of the radiating surface, and Sainte-Claire Deville that the point of dissociation of compound vapours rises with the density of the vapour atmosphere. Supposing interstellar space to be filled with a highly attenuated compound vapour, it would clearly be possible to effect its dissociation at any point, where, by the concentration of solar rays, a focal temperature could be established, but it was argued that the higher temperature observable in a focal sphere was the result only of a greater abundance of those solar vibrations called rays within a limited area, the intensity of each vibration being the outcome of the source whence it emanated : thus, in the focal field of a large reflector, the end of a poker could be heated to the welding point, whereas in that of a small reflector the end of a very thin piece of wire only could be raised to the same temperature. If, however, a single molecule of vapour not associated or pressed upon by other molecules could be sent through the one focus or the other, dissociation in obedience to Deville's law must take place irrespective of the focal area ; but inasmuch as the single solar ray represented the same potential of energy as numerous rays associated in a focus, it seemed reasonable that it should be as capable of dealing with the isolated molecule as a mere accumulation of the same within a limited space, and must therefore possess the same dissociating influence. Proceeding on these premises, the lecturer had procured tubes filled with highly attenuated vapours, and had observed that an exposure of the tubes to the direct solar rays or to the arc of a powerful electric light affected its partial or entire dissociation ; the quantity of matter contained within such

a tube was too slight to be amenable to direct chemical test, but the change operated by the light could be clearly demonstrated by passing an electric discharge through two similar tubes, one of which had and the other had not been exposed to the radiant energy from a source of high potential. If space could be thought to be filled with such vapour, of which there was much evidence in proof, solar rotation would necessarily have the effect of drawing such vapour towards its polar surfaces and emitting it equatorially by an action independent of solar gravity, and which might be likened to that of a blowing fan. When reaching the solar photosphere, this circulating dissociated vapour would, owing to its accumulated density, flash into flame, and could thus be made to account in great measure for the maintenance of solar radiation, whilst its continual dissociation in space would account for the continuance of solar radiation into space without producing any perceivable calorific effect.

Time did not permit him to enter more fully on these subjects, which formed part of a solar hypothesis which he had ventured lately to bring forward, his main object on this occasion having been to elucidate the point of cardinal importance to that hypothesis, that of the solar temperature.

The lecture was illustrated by several experiments, showing the methods by which the dependence of radiation upon temperature had been arrived at.

UNIVERSITY AND EDUCATIONAL INTELLIGENCE

CAMBRIDGE.—Mr. H. Marshall Ward, M.A., late Scholar of Christ's College, First Class in the Natural Sciences Tripos, 1879, Lecturer at Owens College, and Fellow of Victoria University, has been elected Fellow of Christ's College.

It is proposed to appoint a Curator of the new Archæological Museum at Cambridge at a stipend of 150*l* a year. Valuable contributions towards developing the Museum in the direction of ethnology have been promised.

In a discussion on the proposed immediate appointment of a Professor of Physiology, it was mentioned that enlarged class-rooms and a lecture-room, which did not exist, would be needed. A hope was expressed that the Professorship of Pathology would be filled up as soon as there was a reasonable prospect of sufficient appliances in the form of laboratory, &c., being provided for the Professor.

MR. W. N. STOCKER, M.A., Fellow of Brasenose, has been appointed Professor of Physics at the Royal Indian Engineering College, Cooper's Hill. Mr. Stocker took a first-class in mathematics and also in natural science, and has been for the last eight years Demonstrator in the Clarendon Laboratory.

SCIENTIFIC SERIALS

Journal of the Russian Chemical and Physical Society, vol. xv. fasc. 1.—Researches on the naphtha of Caucasus, by MM. Beilstein and Kurbatoff. The naphtha from Bakou consists mostly of hydrocarbons of the C_nH_{2n} series, identical with the products of hydrogenisation of the aromatic series C_nH_{2n-6}. That of the Tzarskiye Kolodtsy has a different composition; it contains but little of the hydrocarbons of the C_nH_{2n} series, but chiefly those of the C_nH_{2n+2} types, with a mixture of those of the aromatic series C_nH_{2n-6}. This analysis explains why the petroleum derived from the Bakou naphtha, although having a greater density together with the same volatility, burns brighter than the American, as also the higher qualities of the oils received from this naphtha. Its hydrocarbons being all liquid it contains but little paraffin, and the greasing oils may be cooled to lower temperatures, without liberating paraffin.—On the use of hyposulphite of ammonium, instead of the sulphide of ammonium, in qualitative analysis, by A. Orlovsky.—On the hydrogenisation of turpentine and cymol, by P. Orloff.—Additions to the theory of the action of chloride of ammonium.—On the evaporation of liquids, by B. Sreznewsky, being the conclusion of a treatise which has appeared in several preceding numbers of the *Journal*. The conclusions arrived at are: the velocity of evaporation is not constant; the velocity of evaporation of drops depends upon their height, and increases as the height diminishes; at a height of an average size it is proportioned to the periphery of the basis.—An aërial calorimeter (a project of), by N. Hesehus.—Elementary demonstration of the pendulum formulæ, by V. Wolkoff.

Vol. xv. fasc. 2.—On the transformation of the primary radical of propyl into a secondary, being a continuation of the researches undertaken by MM. Kékulé and Schröter, on the transformation of bromide into isopropyl under the influence of alluminium bromide.—On the heat of dissolution of mixtures of salts, and on the principle of maximum work, by P. Chrustchoff.—Analysis of the mineral waters of Slavinsk, in the Government of Lublin, by M. Kondakoff. They may be considered as one of the best iron mineral waters, as they contain the least mixture of other mineral substance; that is, 0·19 to 0·22 parts of carbonate of iron out of 3·18 to 3·38 parts of other salts, against 0·37 to 4·36, contained in the water of Spa, or 0·45 to 6·14, and 0·24 to 5·45 in those of Altwasser and Reinerz.—On the chloride of pyrosulphuryle, by D. Konovaloff.—Analysis of sulphur concretions in the fireproof clay from Bakhmut, by M. Kondakoff.—On the structure of nitric compounds of the fatty series, by M. Kissel.—On the permutations of bases in solutions of their neutral salts, by Prof. Menshutkin (analysed elsewhere).—On the specific heat of several products of distillation of naphtha, by E. Kuhlin.—On a secondary product obtained during the preparation of allyldimethyl carbinol, by W. Dieff; it distilled at 165° to 185°, and its structure may be represented as $C_9H_{18}O$.—On the critical temperature of isomeric and homologous series, by A. Nadejdine. The supposition formerly made by the author as to the critical temperature increasing in the same proportion as the temperature of boiling is confirmed by experiments with a sufficient degree of accuracy; it would result that the functions which express the dependency of the critical temperature upon the molecular structure are the same as those expressing the same dependency of the temperature of boiling, and differ only by their constants.—On comets and solar radiation, by M. Schwedoff.—Several conclusions from the theorem of Carnot, by M. Sreznewsky, being a confirmation of the formula of Kirchhoff ("Ueber einen Satz der mechanischen Wärmetheorie") for the expression of the absorption of heat during the formation of saturated solutions, and a verification of it for a certain number of salts.

SOCIETIES AND ACADEMIES

LONDON

Royal Society, March 8.—"Note on the Reversal of Hydrogen Lines; and on the Outburst of Hydrogen Lines when Water is dropped into the Arc." By Professors Liveing and Dewar.

The concentration of the radiation of hydrogen in a small number of spectral lines would lead us to expect that the absorption of light of the same refrangibility as those lines would, at the temperature of incandescence, be correspondingly strong, and that therefore the hydrogen lines would be easily reversed. The mass of hydrogen which can be raised to a temperature high enough to show the lines is, however, so small that, notwithstanding the great absorptive power of hydrogen for the rays which it emits, the reversal of the lines has not hitherto been noticed. In fact, the lines are very readily reversed, and the reversal may be easily observed.

When a short induction-spark is taken between electrodes of aluminium or magnesium in hydrogen at atmospheric pressure, a large Leyden jar being connected with the secondary wire of the coil, the hydrogen lines show no reversal; but if the pressure of the hydrogen be increased by half an atmosphere or even less,[1] the lines expand and a fine dark line may be seen in the middle of the F line. As the pressure is increased, this dark line becomes stronger, so that at two atmospheres it is very decided. As the F line expands with increase of pressure, the dark line expands too, and becomes a band. It is best seen when the pressure is between two and three atmospheres. When the pressure is further increased, the dark band becomes diffuse, and at five atmospheres cannot be distinctly traced. No definite reversal of the C line was observed under these circumstances. The dispersion used, however, was only that of one prism.

By using a higher dispersion the reversal of both the C and F lines may be observed at lower pressures. For this purpose a Plücker tube was used, filled with hydrogen and only exhausted until the spark would pass readily when a large jar was used.

The light of the narrow part of the tube is, under these cir-

[1] The metallic gauge connected with the Cailletet pump used is not at all sensitive, so the pressures here mentioned are only approximate.

cumstances, very brilliant, while the spark in the broad ends is wider and less bright, but does not fill the tube. On viewing such a tube end on, and projecting the image of the narrow part of the tube on to the slit of the spectroscope, a continuous spectrum of the width of the image of the narrow part of the tube is seen, besides the lines of hydrogen given by the discharge in the wide part of the tube. These lines extend above and below the narrow continuous spectrum if the electrode is well placed so that half an inch or so of the spark in the wide part of the tube may intervene between the narrow part of the tube and the spectroscope. The continuous spectrum of the narrow part of the tube seems due chiefly to the expansion of the hydrogen lines when the discharge occurs in so confined a space, and it is much brighter than the lines given by the spark in the wide part of the tube. Where the latter cross the continuous spectrum a very evident absorption occurs. The authors observed it with a diffraction grating. The C line in the third order falls so near the F line in the fourth that both may be observed together. F is much more expanded than C, and the reversal consequently less marked though quite plain. The other lines being still more diffuse, their absorption could not be traced.

The authors have before observed (*Proc. Roy. Soc.* vol. xxx. p. 157) that the C and F lines of hydrogen are visible in the arc of a De Meritens' magneto-electric machine taken in hydrogen; though in the arc of a Siemens' machine the C line can only be detected at the instant of breaking the arc, the F line hardly at all. When, instead of taking the arc in hydrogen, small drops of water are allowed to fall from a fine pipette into the arc taken in air in a lime crucible, each drop as it falls into the arc produces an explosive outburst of the hydrogen lines. Generally the outburst is only momentary, but occasionally a sort of flickering arc is maintained for a second or two and the hydrogen line C is visible all the time. The lines (C and F) are usually much expanded, but are frequently very unequally wide in different parts of the line. F is weaker, more diffuse, and more difficult to see than C, and is visible for a shorter time. There is no sign of reversal. In the explosive character of the outburst and the irregularity in the width of the lines the effect resembles that of an outburst of hydrogen in the solar atmosphere. The elements of the water are, it must be supposed, separated, but from the explosive character of the effect they are not uniformly distributed in the arc. The arc being horizontal, and the image of it projected on to the slit of the spectroscope, it was really a very small section of the arc which was under observation, and this renders the variation in the width of the lines the more remarkable.

April 5.—"On a hitherto unobserved Resemblance between Carbonic Acid and Bisulphide of Carbon." By John Tyndall, F.R.S.

Chemists are ever on the alert to notice analogies and resemblances in the atomic structure of different bodies. They long ago indicated points of resemblance between bisulphide of carbon and carbonic acid. In the case of the latter we have one atom of carbon united to two of oxygen, in the case of the former one atom of carbon united to two of sulphur. Attempts have been made to push the analogy still further by the discovery of a compound of carbon and sulphur analogous to carbonic oxide, but hitherto, I believe, without success. I have now to note a resemblance of some interest to the physicist, and of a more subtle character than any hitherto observed.

When, by means of an electric current, a metal is volatilised and subjected to spectrum analysis, the "reversal" of the bright band of the incandescent vapour is commonly observed. This is known to be due to the absorption of the rays emitted by the hot vapour in the partially cooled envelope of its own substance which surrounds it. The effect is the same in kind as the absorption by cold carbonic acid of the heat emitted by a carbonic oxide flame. For most sources of radiation carbonic acid is one of the most transparent of gases; for the radiation from the hot carbonic acid produced in the carbonic oxide flame, it is the most opaque of all.

Again, for all ordinary sources of radiant heat, bisulphide of carbon, both in the liquid and vapourous form, is one of the most diathermanous bodies known. I thought it worth while to try whether a body reputed to be analagous to carbonic acid, and, like it, so pervious to most kinds of heat, would show any change of depoitment when presented to the radiation from hot carbonic acid. Does the analogy between the two substances extend to the vibrating periods of their atoms? If it does, then the bisulphide, like the carbonic acid, will abandon its usually transparent character, and play the part of an opaque body, when presented to the radiation from the carbonic oxide flame. This proves to be the case. Of the radiation from hydrogen, a thin layer of bisulphide transmits 90 per cent., absorbing only 10. For the radiation from carbonic acid, the same layer of bisulphide transmits only 25 per cent., 75 per cent. being absorbed. For this source of rays, indeed, the bisulphide transcends, as an absorbent, many substances which, for all other sources, far transcend *it*.[1]

Chemical Society, April 19.—Dr. W. H. Perkin, president, in the chair.—The following gentlemen were elected Fellows: T. L. Briggs, J. A. Basker, J. B. Coleman, W. H. Cannon, E. C. Conrad, C. Gillett, E. C. Henning, N. K. Humphreys, L. Levy, A. Ness, V. I. Schopoff, A. E. Wilson.—The following papers were read:—On the gases evolved during the conversion of grass into hay, by P. F. Frankland and F. Jordan. The authors find that comparatively dry grass soon evolves considerable quantities of carbonic anhydride with mere traces of hydrogen and hydrocarbons; this evolution of gas occurs in air and in an atmosphere of carbonic anhydride or hydrogen; in oxygen a notable proportion of nitrogen accompanies the carbonic anhydride. Under water, grass also evolves carbonic anhydride with some hydrogen, due probably to lactic fermentation, acetic, lactic, and propionic acids being simultaneously formed.—Note on an apparatus for fractional distillation under reduced pressures, by L. T. Thorne. The object of this apparatus is to facilitate the removal of the various fractions of the distillation without breaking the continuity of the distillation.—Notes on the condition in which carbon exists in steel, by Sir F. A. Abel, C.B., and W. H. Deering. Two series of experiments are given by the authors; in the first the differences between cold rolled, annealed, and hardened samples of the same steel are investigated. The steel disks were subjected to the action of a saturated solution of potassium bichromate containing 5 per cent. by volume of sulphuric acid. In each case a blackish residue consisting of a carbide of iron was left; in the case of the cold rolled and annealed disks, the carbon in this residue corresponded pretty closely with the total carbon present; but in the hardened disk only one-sixth of the total carbon was found in this residue. In the second series of experiments, the action of various strengths of bichromate solution on cold rolled steel is studied, and it is proved that, if the oxidising solution be not too strong, a residue consisting of a definite carbide Fe_3C is left, and that the carbon is therefore not simply diffused through the mass, but exists as a definite compound capable of resisting the action of a solvent which rapidly dissolves metallic iron.—On the spectrum of beryllium with observations relative to the position of that metal among the elements, by W. N. Hartley. From a photographic study of the spectrum, the author concludes that beryllium is the first member of a dyad series of elements of which in all probability calcium, strontium, and barium are homologues.

Linnean Society, April 19.—Sir John Lubbock, Bart., president, in the chair.—Messrs. T. W. Coffin, F. H. Collins, C. D. F. De Lanne, D. Morris, J. Jardine Murray, and Hon. J. B. Thurston were elected Fellows of the Society.—Mr. J. Britten exhibited and made remarks on specimens of *Arum italicum* from Torquay, South Devon.—Mr. G. F. Angas showed several vegetable products from the Island of Dominica, among others an unusually large seed-pod of *Cassia fistula*, and other examples of Leguminosæ, also Polyporus fungi from the Roseau Falls.—Mr. F. V. Dickins called attention to a Japanese work issued by the University of Tokio, giving descriptions and illustrations of plants grown in the Botanic Gardens of Koishkikawa.—A paper was read by Sir John Lubbock on the sense of colour amongst some of the lower animals (vol. xxvii. p. 619).—There followed a communication by Prof. P. T. Cleve of Upsala, on the diatoms collected during the Arctic expedition of Sir George Nares.—The Rev. A. E. Eaton gave a digest of an extensive monograph of the Ephemeridæ or Mayflies, part i. In this the subject is prefaced by an historical account and general view of the group; the genera are defined, and a

[1] Nearly twenty years ago I observed, among other changes of diathermic position, the reversal of bisulphide of carbon and chloroform, when the pale blue flame of a Bunsen burner was the source of heat. When, for example, the rays issued from a luminous jet of gas, the absorptions of the bisulphide and of chloroform were found to be 9·8 and 12 per cent. respectively; whereas when the Bunsen flame was employed, the absorptions of the same two substances were 11·1 and 6·2 per cent. The cause of this reversal doubtless is that in the Bunsen flame hot carbonic acid is the principal radiant (*Phil. Trans.*, 1864, p. 352).—April 6.

tabular conspectus of the present known species indicated.—A paper was read on the joint and separate work of the authors of Bentham and Hooker's "Genera Plantarum," by George Bentham.

Zoological Society, April 17.—Prof. W. H. Flower, LL.D., F.R.S., president, in the chair.—The Secretary read a report on the additions that had been made to the Society's Menagerie during the month of March, and called special attention to three Sirens (*Siren lacertina*) from South Carolina, presented by Dr. G. E. Manigault, C.M.Z.S., and to an American Teetee Monkey of the genus *Callithrix*, which it was difficult to determine satisfactorily in its living state, but which was certainly new to the Society's Collection.—Prof. Flower, F.R.S., gave an exposition of the systematic classification of the Mammalia which he had recently prepared for use in arranging the specimens in the Museum of the College of Surgeons, and in a treatise on the subject of Mammals in the "Encyclopædia Britannica."—A communication was read from Mr. W. L. Distant, containing the first of a series of contributions to an intended monograph of the Homopterous family Cicadidæ. In the present paper the author gave the results of an examination of the Cicadidæ contained in the Dresden Museum (including the specimens collected in Celebes by Dr. A. B. Meyer), and added the descriptions of other species belonging to the collections of Dr. Signoret and the author. Eleven species were described as new from various localities.—Mr. Sclater read a second paper on the birds collected in the Timor Laut or Tenimber group of islands by Mr. H. O. Forbes, based on additional specimens lately received. The avifauna of the group, as indicated by Mr. Forbes's collection, contained 59 species, of which 22 were peculiar to these islands.—A communication was read from Mr. F. Moore, F.Z.S., containing the first part of a monograph of the butterflies belonging to the groups *Limnaina* and *Euplœina*.

Physical Society, April 14.—Prof. G. Carey Foster in the chair.—New Members: Mr. W. F. Smith, Mr. George Forbes, M.A.—Mr. W. Lant Carpenter read a paper on science demonstration in Board schools, in which he showed the drawbacks of the present system of leaving science to be taught by the other masters, and pointed out the marked advantages of the system followed in Birmingham and Liverpool, where skilled lecturers are appointed to go from school to school, and provided with an assistant demonstrator and proper apparatus. Mr. Carpenter advocated the extension of this system to London and the country in general. He also showed the evil of the present system of cramming for examinations. Dr. W. Carpenter pointed out the advantages of object lessons in training the minds of children. Dr. J. H. Gladstone stated that much had been done in London to introduce object lessons, and that under the Mundella code science would be taught in all Board schools to all the children, who, however, might have the opportunity of choosing between science and literature. Mr. W. Baily, Prof. Foster, and Prof. W. Chandler Roberts, also advocated the system of special science teachers.—Prof. Roberts then took the chair, and Mr. Glazebrook explained a new polarising prism which he had devised to prevent displacement of the pencil of rays. He also showed how the curved diffraction-gratings of Prof. Rowland do not always give perfect definition, and calculated the aberration of the rays.—The Secretary then read a paper by Mr. W. H. Stokes and Mr. A. E. Wilson on experiments on the viscosity of saponine. When a disk is rotated in water, the resistance to its motion is greatest when the plate is immersed a little below the surface; but with saponine the viscosity is greatest when the disk is not wholly, but only partially, immersed below the surface.

Entomological Society, April 4.—Mr. J. W. Dunning, M.A., F.L.S., &c., president, in the chair.—The death of Prof. P. C. Zeller of Stettin, one of the Honorary Members of the Society, was announced and commented upon.—Two new Members were elected.—Mr. W. F. Kirby exhibited specimens of *Acridium succinctum*, Linn., one of the most destructive species of migratory locusts in India.—Prof. Westwood mentioned that a Myriopod, *Polydesmus complanatus*, Linn., had lately been erroneously announced to be the cause of the potato disease.—Rev. A. E. Eaton exhibited a patent revolving object-holder used by mineralogists, which seemed likely to be useful to entomologists also.—Mr. E. A. Fitch exhibited galls of *Cecidomyia violæ*, Loew., and of *Aploneura lentisci*, Licht.—Sir S. S. Saunders read a short paper on the classification of the germ-feeding races of fig-insects.—Mr. H. Goss exhibited specimens of *Pimelia angulata*, Fabr., from the temple of the Sphinx at Ghizeh.—Papers read:—On a small collection of Clavicorn *Coleoptera* from North Borneo, by Mr. A. S. Olliff; Descriptions of new genera and species of *Hymenoptera*, by Mr. P. Cameron; and notes on new or little-known species of *Hymenoptera*, chiefly from New Zealand, by Mr. W. F. Kirby.

Edinburgh

Royal Society, April 16.—Mr. Murray in the chair.—Mr. Sang read a paper on some properties of the curve of simple flexure, of which he gave neat geometrical demonstrations. A simple construction was given for finding the radius of curvature at any point and so affording a ready means for tracing the curve. The related theorems in pendulum motion were also given.—Dr. Knott communicated the results of electrometer measurements of the resistance of electrolytes, which had been carried out lately in the Edinburgh University Laboratory. The method seemed capable of giving fairly accurate values.—In a note on the electrical resistance of hydrogenised palladium, Dr. Knott gave 1·51 as the ratio of the resistances of the fully-charged and pure palladium, the increase of resistance being very nearly proportional to the charge for smaller charges. It was also noted that the electromotive force between palladium and platinum dipping in dilute sulphuric acid was greatly increased for a slight charge of hydrogen, falling off again very markedly as the charge reached its maximum.—Dr. Macfarlane, in a note on plane algebra, or double algebra, as De Morgan named it, demonstrated with facility certain theorems that ordinarily require considerable algebraic manipulation.—Prof. Tait presented a continuation of his theoretical investigations on heat conduction in heterogeneous bodies, as modified by the Peltier and Thomson effects, and gave the result of his investigation of the thermoelectric position of pure ruthenium. On the diagram this metal lies below iridium, to which it is in other thermoelectric respects very similar.

Berlin

Physical Society, April 6.—Dr. Aron reported on the accumulators, on which he has been making experiments for several years past. Even before M. Faure's discovery, at the time when M. Planté announced his first essays with the secondary batteries, Dr. Aron was endeavouring to determine a convertible electric element which, being theoretically possible, might also be available for practical purposes. He first of all tried to make the Daniell chain convertible by using, instead of the two amalgamating fluids, hydrate of soda and sulphate of copper which do not amalgamate, but without success. Like many others he repeatedly tested Planté's already published statements regarding convertible cells of plates of lead immersed in diluted sulphuric acid, and which had to be charged in a very definitely prescribed way, but without any certain results. The cell sometimes became charged and discharged alternately, at other times not. He accordingly tried plates of lead which had been previously crystallised by corrosion, and these he found far more reliable. He therefore constructed accumulators of plates of lead in sulphuric acid to which some nitric acid had been added. Although more certain in their application, these were by no means equal to the practical requirements. The favourable results of the corrosion, as regarded the crystalline surface, a point also confirmed by Planté himself, was explained by Dr. Aron, who attributed it to the disintegration of the metal. He therefore tried to increase the effect by using lead-sponge, but without result. At that time he also thought of red lead, but made no experiments with it, because he knew of no means of fixing this powder to the lead plate conductor. It is now known that M. Faure simply spread the red lead on the plates, and thus produced his powerful accumulators possessing great storage capacity. When this became known, Dr. Aron carried out an extensive series of similar experiments in order to test its practical value, and even increase it. For the latter purpose he introduced a substantial improvement by attaching the red lead with collodium, which in the practical application of the chains is of course out of the question. But as regards their practical utility the accumulators have fallen far short of the hopes generally entertained of them. The main difficulty lies in the thin plates of lead which, when thickly covered with red lead, although very effective, become corroded and useless after being once used, while thick plates, by the formation of sulphate of lead, are rendered ineffective. As to the theory of accumulators, to rightly understand it, it is very important to bear in mind the

fact established by Messrs. Gladstone and Tribe, that in the cell, consisting of two plates in diluted sulphuric acid, the electric current changes the sulphate of lead generated at the positive pole into peroxide of lead, $PbSO_4 + H_2O + O = PbO_2 + H_2SO_4$, whereas at the negative pole the sulphate of lead is simply decomposed into sulphuric acid and disintegrated lead. Hence, after charging, the cell consists of Pb | H_2SO_4 | PbO_2 | Pb, a combination which yields a very powerful discharge, available also for a protracted period. To this theory it has been objected that at the negative pole the sulphate of lead cannot be decomposed into lead and sulphuric acid. But Dr. Aron has satisfied himself that, under the influence of the hydrogen beginning to be generated, very thin layers of sulphate of lead become so reduced, thicker layers alone resisting decomposition. The process at the positive electrode being really such as is described by Gladstone and Tribe, the above theory of accumulators may, broadly speaking, be accepted as correct. As regards the peroxide of lead, the speaker pointed out that this combination is admittedly of a brown colour, whereas the substance deposited on the positive plate is black. From a more searching examination of this substance, it resulted that it is not the peroxide, but a hydrate of the peroxide of lead. And Dr. Aron suspects that there is here less question of a hydrate PbO_2H_2O than of a combination of the oxide of lead with peroxide of hydrogen. A series of theoretically interesting isolated phenomena may possibly be produced by following up the processes here in question. But in the present conditions Dr. Aron holds the practical application of the accumulators to be hopeless.—Prof. Neesen briefly described a slight improvement in the quicksilver air-pump, illustrating it with a diagram.

PARIS

Academy of Sciences, April 23.—M. Blanchard in the chair.—The death of Prof. Roche of Montpellier, Correspondent in Astronomy, was announced. (A report on his work by M. Tisserand is inserted in *Comptes Rendus.*)—A new method for determination of the right ascension of polar stars, and of the inclination of the axis of a meridian above the equator (continued), by M. Lœwy.—On some relations between the temperatures of combustion, the specific heats, the dissociation, and the pressure of explosive mixtures, by M. Berthelot.—Note on the inland African sea, by M. Cosson.—On a manner of determining the angle of position of a point of the surface of a star with the aid of a horizontal telescope, by M. Trépied.—On the use of the horizontal telescope for observations of solar spectroscopy, by M. Thollon. His apparatus is essentially a horizontal telescope deprived of the tube and reduced to its most simple expression. It is more easily managed than an equatorial. The mirror used is guided by the two hands, and the solar surface is explored at will. The author shows how he solved the difficult problem of determining position.—Determination of a particular class of surfaces with plane lines of curvature in a system, and isotherms, by M. Darboux.—On the reduction of ternary positive quadratic forms, by M. Minkowski.—Law of periods (concluded), by M. de Jonquières.—On a relation of involution, concerning a plane figure formed of two algebraic curves, one of which has a multiple point of an order of multiplicity inferior by unity to its degree, by M. Fouret.—Study of infra-red radiations by means of phenomena of phosphorescence, by M. Becquerel. He indicates the results of his method with telluric bands, the absorption spectrum of water and of some earthy metals, and the emission spectrum of metallic vapours.—On the specific heat of some gases at high temperatures, by M. Vieille. He verifies, for the gases H, O, N, and CO, the identity of the molecular heats with constant volume up to 2700°. The measurement of pressures leads him to attribute to certain reactions temperatures much higher than have been supposed practically realisable.—On the variation of indices of refraction of water and quartz under the influence of temperature, by M. Dufet. He indicates a new application of Talbot's fringes in measurement of this variation. The number for quartz is almost identical with that obtained by M. Fizeau.—Experimental studies on the production of vowels in whispered speech, by M. Lefort. Air is blown into a cavity of variable capacity, open and closable at the upper part. The sounds characteristic of vowels are thus produced. The author claims to prove that the vowels are not timbres (as generally taught); they are notes of different heights of the instrument of speech (quite distinct from the vocal instrument). Various timbres may be communicated to them by action of the muscles of the organ of voice.—On the liquefaction of nitrogen, by MM. Wroblewski and Olszewski. Nitrogen cooled in a glass tube to − 136° C., and under a pressure of 150 atm., does not liquefy. On sudden release there is tumultuous ebullition. Gradual release, not passing 50 atm., yields the liquid, clear and colourless, with a distinct meniscus; it evaporates very quickly. The liquefaction of CO under like conditions on April 21 was announced.—On iodised apatites, by M. Ditte.—Action of water on Theil's lime, and the existence of a new hydraulic compound, *pouzzo-portland*, by M. Landrin. The composition of this compound is silica 44·55, lime 55·45. It is the principal element of all Theil's compounds.—On some phenolic derivatives, by M. Henry.—Jurassic Echinida of Algeria, by M. Cotteau. Of the 47 species found, 28 occur in Europe about the same stratigraphic levels. Some curious species peculiar to Algeria are noted.—Clayballs of Macaluba, by M. Contejean. These were found (of all sizes from a cannon-ball to a boy's marble) in the dried bed of a ravine, near the mud volcano named. They are of coarse clay, with small crystals of gypsum, giving a rough surface. It is thought they are formed by the autumn rains, and are dissolved by the heavier winter rains.—The perception of white and of complex colours, by M. Charpentier. His curves show, *inter alia*, that what artists term warm colours are distinguished from a colourless ground *more* easily than white, the cold colours *less* easily.—On the functions of pyloric appendices, by M. R. Blanchard. These appendices digest effectively cooked starch, less effectively raw starch, and transform albuminoids; as they do not effect emulsion and decomposition of fats, they are but imperfect representatives of the pancreas.—On the bite of the leech, by M. Carlet. He detached the animal from the shaved skin of a rabbit at different stages. Suppose a scarifier, with three toothed and equidistant blades withdrawing from one another while they press into the skin, and operating several times successively in the same place: this gives a pretty exact idea of the mechanism.—Comparative study of the bacteria of leprosy and of tuberculosis, by M. Baber. The differential properties indicated by Koch do not, he holds, exist; but there are others, bearing on chemical and molecular reaction, on form, and on arrangement in the tissues.—Influence of sensitive (nerve) roots on the excitability of motor-roots, by M. Canellis. Section of the sensitive root increases considerably the excitability of the motor nerve.—Immunity of workers in copper during the last epidemic of typhoid fever; confirmation of anterior observations, by M. Burq.—Influence of altitudes on phenomena of vegetation, by M. Angot. The harvest-time for inter wheat is retarded in France on an average four days where the altitude is increased about 100 metres.

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THURSDAY, MAY 10, 1883

EDUCATION IN THE UNITED STATES

United States Report of the Commissioner of Education for the Year 1880. (Washington: Government Printing Office, 1882.)

ANOTHER valuable survey of education in the United States has been published, relating to the year 1880; a survey made by the Bureau whose duty and purpose, it is laid down, shall be "to collect statistics and facts showing the condition and progress of education in the several States and Territories, and to diffuse such information respecting the organisation and management of schools and school systems and methods of teaching, as shall aid the people of the United States in the establishment and maintenance of efficient school systems, and otherwise promote the cause of education throughout the country." The Bureau has no authority, it tells us, and seeks none, to interfere with school organisation, but aims to report institutions precisely as they are; and the variety of experiments tried in the States, which in kindred and spirit of government are so close to our own, must make this publication a repertory of experiences of the utmost value to the English educationalist.

The following is the immense provision made for education in the United States:—For public or common schools, every sixteenth section of public land in the older States, and every sixteenth and sixtieth in the newer ones: calculated to equal nearly 68 million acres; for seminaries or universities, two townships, or 46,080 acres in each State, and in some instances a greater quantity. An additional grant in 1862 of 30,000 acres for each senator which each State was entitled to send to Congress was awarded for the establishment and support of agricultural and mechanical colleges, amounting to 9,600,000 acres. Total, 78 million acres!

Yet, with this immense provision, the Old World difficulty is making itself felt strongly now in America as population increases, which was not foreseen when each State laid it down that education should be provided for every child, viz. that a considerable proportion of that population now will not avail themselves of this education. In very few States is the increase in scholars nearly in proportion to the increase in population, and our Report gives serious confirmation to the alarming statistics lately brought forward by the Rev. Joseph Cook in his Boston lectures. Private effort to attract children to school by providing them also with clothing is said now to be "occupying a very important place." Like other signs of "Progress and Poverty" which Mr. Henry George urges so warmly, there is now enough truancy and absenteeism from school to become a serious hindrance to education. In some New England cities truant officers are appointed, but in other cases the popularity of education without class-feeling allows the important business to be left in the hands of the police.

Cities (under which definition are enumerated 244 municipalities of above 7500 inhabitants) contain one-tenth of the teachers and one-sixth of the school population, and expend more than one-fourth of the money. "While the municipal systems of the United States are more defective, more assailed, and doubtless requiring greater efforts to reform them than any other part of the civil machinery, the city school affairs are in the main well systematised." The Boards of Education are variously constituted in different cities. In some cases the members of the Board are elected directly by the people; in some they are appointed by the Mayor; and in the District of Columbia by the Commissioners.

The powers of School Boards in the United States are in some instances restricted to the care and management of the public schools, while in others they extend to the charge of school funds. In nearly all the cities referred to above, superintendents are appointed—with few exceptions men of superior ability and specially adapted to the work of school supervision, who combine a great deal that is done in England partly by the Boards and partly by the Government Inspector at his occasional visits. They bring to bear more special knowledge of the subject than the former, and give far more time to each school than the latter can.

This Report contains a review of education through the decade, and perhaps the most striking thing is the absence of uniformity in the circumstances and changes in the different States. Thus, in Maine and New Hampshire during the last ten years, and in Indiana during the last year, population has decreased, but the attendance at schools has nevertheless increased. In Rhode Island, New York, and Iowa the reverse has taken place; the population has increased, but school attendance has decreased. In Arkansas a change of system made in the middle of the decade has resulted in a reduction of everything; the reduced number of scholars attending, however, having largely increased again the last year. In New Jersey and Pennsylvania there is an improvement every way; while in Massachusetts the attendance equals the school population. In Virginia the increase in every particular has been great. The Maryland schools only suffer from a decrease in the income for public school purposes.

The Report is very satisfactory as to the difficult matter of educating the coloured race. In 1870, out of 2,500,000 above fifteen years of age less than 150,000 had attended school. At the time of this present Report (1880) there is a total attending school of more than 800,000—over 15,000 of whom are, moreover, attending the higher grade schools. Those of them who are attending normal institutes for coloured teachers manifest great interest in the opportunities for improvement thus afforded. There is still, however, great deficiency of such trained teachers, and the poverty of the country is so great that the schools in rural districts are held in their churches, and the duty of assistance to them is urged by the Commissioner upon the national Government that has made them free. Considerable help has been given to the work among them by the Peabody fund, but the religious denominations of all the States have done most—in fact, five-sixths—of the work. Of 44 normal schools, 29 are under their auspices; of 36 institutions for secondary instruction, 31; 13 of the 15 universities or colleges; and all the schools of theology. But in all the States with mixed population now, except Delaware, Kentucky, and Maryland, school funds are devoted to school population without regard to colour.

In our crowded island we need not refer again to the

other special difficulty of the United States. The scattered population leads inevitably to small schools: in Maine, 1200 out of 4000 had average attendances ranging from two to twelve; this leads, of course, to low pay; and this to low attainments on the part of the teachers, of whom not more than 4 per cent. have had normal training. A great drawback to teachers also is the uncertainty of their tenure of office. In some States the School Committee have no power to hire teachers for more than a year, and engagements are seldom made for a longer time. In others, men are employed for winter and women for summer terms, thus causing an uncertainty in the profession, which must be highly mischievous. It is a feature in American education, that in both elementary and secondary schools more than half the teachers are women. In this respect the United States differs from every other nation; and a fear is expressed lest it may involve the sacrifice of some of the conditions essential to the development of strong self-reliant characters. As the Transatlantic ladies are supposed not to be wanting in these themselves, let us hope that it may not have such an effect; but that it may be said of this arrangement that—

"Emollit mores, nec sinit esse feros."

The Commissioner in his Report says that, "carefully considering the position of woman in the work of education, what she has done, and may do, as a teacher, what her nature and experience may fit her to do better than man, as an officer, inspector, or superintendent, he has favoured the opening of appropriate offices to her in connection with institutions and systems of instruction." He "regrets to say that women have shown more indifference to this opportunity than he expected." There are 227 women's colleges in which every advantage is offered that men have, but they are not popular. Still he points out that since women were elected in 1873 to the Boston School Board, and subsequently admitted to that and other Boards, the employment of them on sub-committees, for which they were best adapted, has been the introduction of a new force; in other words, it is in the line of progress.

The Report urges the desirability of well-trained teachers, more particularly in the case of scientific knowledge. "Such knowledge finds its application in all arts and industries, and in all measures for the preservation of health and life, and it offers the only means of dissipating the fears and superstitions, and correcting the foolish practices arising from ignorance of the phenomena and laws of nature." It points out also that the general Government is doing more in behalf of scientific work and publications than all the other agencies put together.

Partially, no doubt, the result of a feeling making its way among educationists, but partially also a sign of the moderate level of education reached, is the small number (448) in Ohio who learn Greek. A curious mark of changed relations is to be found in the fact, that still fewer (418) learn French; while nearly 100 times the number (40,813) learn German; against nearly 650,000 who learn spelling.

Where the ordinary primary education is good in America, evening schools of elementary grades are less sought after than those of advanced grade, except in cities where there is a large foreign population. In communities, distinguished alike for intelligence and business enterprise, evening high schools are especially appreciated, the most promising artisans and clerks looking to them for the means of continuing their studies.

The peculiarly American institution of summer schools is being turned to admirable use by teachers occupied with regular school duties during the rest of the year, who go with scientific expeditions and to stations maintained by the universities, and profit by the facilities for study and investigation thus offered them in combination with fresh air and change of scene.

There has been scarcely any increase since 1875 in the number of universities or colleges as they are indiscriminately called, but the new States are many of them overprovided with these higher-branch schools, while deficient in the elementary schools at present more necessary. The disproportion between colleges and preparatory schools in certain States may be judged by the report that while Tennessee has twenty-one colleges, Massachusetts with a larger population reports seven. The former State has two preparatory schools; the latter twenty-three. Under such circumstances it is not strange that some of these universities or colleges should be doing the work of the lower grade schools, as thirteen are reported as doing only.

In 1871 Arkansas established an industrial university which soon after possessed classical, agricultural, engineering, commercial, and normal courses, and a preparatory department. In various other States similar centres of education in practical subjects were opened, and the variety of subjects and arrangements for teaching them, which are to be found in so many independent centres, will be found very instructive to all who are inquiring about technical education, especially agricultural. Several of these courses are such as have been approved of after varied experiment; an advantage which they have over the schools of science not endowed by the national grant, where the will of the founder has had a contrary effect.

Michigan University has inaugurated an excellent work in providing that a faculty will visit once every year any public high school in Michigan on request of its School Board, and report its condition. "If the faculty shall be satisfied that the school is taught by competent instructors and is furnishing a good preparation for any one or more of the regular courses of the University, then the graduates from such preparatory course or courses will be admitted to the freshmen class of the University without examination, and permitted to enter on such undergraduate course as the approved preparatory work contemplated." This is a method of making the same labour serve the double purpose of inciting the school to efforts, and also of matriculating the University students. The matriculation examinations at so many of these universities were naturally of most various standards, and some approach towards a uniform standard has been made between ten principal colleges in New England.

In the Illinois State University a peculiar government has been tried called "The Students' Government," by which every official was selected or appointed by the president whom they had elected, and all the forms of a Republican Government are gone through; forming an excellent practice to the students and probably raising a good *esprit de corps*.

The comparison of the state of medical instruction at

the present with what it was ten years ago, although showing great improvement, still draws a discreditable picture of what so important a profession is allowed to remain in America; and quite a romantic tale is told of the means by which men getting a living by selling false degrees were brought to justice. The number of the universities and other bodies which claim the right to bestow degrees makes the tracking down such forgeries very difficult.

The business of nursing the sick is rising to its proper position as that of an intelligent assistance to the profession of medicine. Our Report wisely recounts the good results to be gained by student-nurses, though chiefly moral qualities are inculcated.

In the schools of science, the number of students which increased so largely in 1878, but fell off in 1879, has begun to increase again; the number of institutions as well as teachers having increased steadily all the time. Our Report says:—"The multiplication and growth of schools of science has been a marked feature in the recent history of education in America. Either the stimulus given to them by the national aid, or the sentiment which compelled Congress to give help to higher education, has carried forward and deepened the interest in industrial, scientific, and technical instruction. Students are now more frequently choosing lines of study which lead to a life of business activity or to prominent positions in industrial pursuits. Colleges that a few years since held strictly to a rigid classical course are feeling the new impulse and are striving to add to their efficiency by making provision for special instruction preparatory to definite occupations. Men of wealth are endowing schools of science and technology more richly than other institutions; for they believe that the practical education which has now come to the front will do more than anything else to promote the industry and prosperity of individuals, and to utilise the resources of the nation."

The requirements for admission to the scientific department of colleges and schools of science are not so great as to classical collegiate courses.

It is rather curious that the study of Latin is allowed to be dropped in a law school of Harvard; but the following remarks made upon the value of law schools, as compared with that of articling pupils to lawyers, may well be applied not to them only but to all technical instruction:—"In schools systematic training is received. Less opportunity is afforded for desultory and spasmodic reading. Regular habits of study are required. Examinations to be passed give steadiness and thoroughness to the work. Companions make emulation. The desire for the respect of the professors is a further stimulus to faithfulness, and they are ready to aid in the understanding of intricate questions. Underlying principles are given an attention which corresponds to their relative importance."

Forestry is taught in some of the higher institutions, with plantations of trees arranged in their natural orders; and its value is pointed out, both as a branch of knowledge to the students, and also as adding to the knowledge of the range of possible and profitable cultivation of many species.

A system of teaching the deaf and dumb to read from the lips of others instead of the old finger reading is described as wonderfully successful and fast gaining ground.

Not a small advantage will science gain if the system of making full inquiries into the antecedents of every case of the above, as also of blindness, is patiently and thoroughly carried out. Some generalisations have already been made with regard to the latter. In the case of 100 feeble-minded scholars their weakness is traced to consumption in their stock. An inquiry into colour-blindness in the Boston schools leads to the recommendation that a systematic process of giving instruction in colour, its names, and shades, should be introduced into primary schools.

The importance of reform schools is steadily and strongly upheld. The needs of their inmates are wisely consulted by an education more moral than intellectual being instilled into them, and by a knowledge also of some method of gaining a living when dismissed being carefully given to them. The better feelings are drawn out and encouraged by a system of rewards for all good conduct, instead of only punishments for bad. Two curious observations are recorded: one is, that working among flowers has a softening tendency upon such characters; and the other, that prisoners are, in general, singularly short of mathematical ability.

The increase in the number of free libraries since the previous year's Report alone nearly equals the entire number of them in England, making a total nearly reaching 3000. Though many of these are very small and to be compared with school libraries here, yet they average all through 4000 volumes in each. A large increase also is noticeable in Kindergarten schools, in schools for nurses, in deaf and dumb, orphan, and reform schools.

The Bureau is indebted to private enterprise for a competition on the subject of schoolhouse plans organised during the year by the "Plumber and Sanitary Engineer." It has drawn forth from the committee of award a sketch of the qualifications they believed to be necessary for a public school building in a large and densely populated city. They lay down ten primary requisites which every plan ought to contain; and the Commissioner hopes that an impulse has been given by their report, which will not be lost or wasted.

Education, we are told, has become in every section of the country a matter of more active public interest than usual. City and country papers have given a place in their columns to the subject, besides periodicals discussing them. It is rather curious to us in aristocratic England to find not selfishness and stupidity only but demagoguism also charged with creating discouragements! W. O.

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications.*

[*The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to insure the appearance even of communications containing interesting and novel facts.*]

The Microphone

It is probable that the writer of the note at p. 588 has not had an opportunity of seeing the paper of mine to which he refers, and an abstract of which is given at p. 376 of the present volume of Nature.

The adhesion between metallic contacts consequent upon the passage of a current has been carefully investigated by Mr. Stroh, who observed it in the case of all of a great number of metals with which he experimented. My first observations on the subject (one of which is mentioned in the paper) were made with the refractory metal platinum, and not with bismuth, as the writer of the note seems to infer; and though Mr. Stroh's explanation—that the adhesion is due to fusion—is quoted, I express no opinion of my own on the matter. Whatever may be the cause, it seems evident enough that such adhesion must necessarily be detrimental to the perfect action of a microphone, though I am not aware that attention has been previously directed to this point.

It is not correct to attribute to me the opinion, as stated in the note, "that the heat generated by the current plays an important part, for in carbon this reduces the resistance, whilst in metals it increases it." On the contrary I give reasons for believing that at least a moderate degree of heat increases the resistance of loose carbon contacts. Increased *current*, however, is accompanied by diminished resistance, and while I am not prepared to say that heat plays no part whatever in the matter, it appears to me probable that the effect is mainly owing to some other incident of the stronger current, *e.g.* greater difference of potential.

My experiments on metals were not, as the writer supposes, entirely confined to bismuth. More than a hundred observations were recorded of the resistance of platinum and copper contacts under different conditions, and some of these are referred to in the paper. Owing, however, to the low specific resistance of these metals, the methods which I had applied with success in the case of carbon were found to be unsuitable, and the results obtained, though not on the whole inconsistent with those yielded by bismuth, were unsatisfactory and inconclusive. Bismuth was chosen for the bulk of the experiments, principally on account of its bad conductivity, which renders changes in the resistance of the contact easier of observation; but since it was my object to contrast the behaviour of metals with that of carbon (which is infusible), its ready fusibility is another advantage. If I had desired to make a good metallic microphone, I should probably have thought with the writer of the note that bismuth was "the very metal which ought to have been avoided." But for experiments conducted with the object of ascertaining the causes of the generally recognised fact that metals, as a class, are inferior in microphonic efficiency to carbon, it is evident that the metal which gives the poorest microphonic effects is *the very one which ought to be selected*, on account of the probability that with such a metal these causes would be most conspicuous.

As a matter of strict scientific exactness I agree with the writer that "no conclusion of any value as to metals in general can be drawn from experiments on bismuth alone." But since the physical properties with respect to which bismuth differs from carbon, and which have any probable connection with microphonic action, seem to be common in various degrees to all metallic bodies, I venture to predict with tolerable confidence, that if the experiments described in the paper are repeated with suitable apparatus, it will be found that all the conclusions arrived at with regard to bismuth (as summarised in the abstract before referred to) are also true to a greater or less extent for any other ordinary metal. SHELFORD BIDWELL

Wandsworth, April 22

[The necessary brevity of the note to which Mr. Bidwell refers precluded lengthy quotations. At the same time it was only natural to draw attention to the weak point in Mr. Bidwell's argument, namely, that the behaviour of the metals generally could not with any certainty be argued from observations made, as Mr. Bidwell admits, on the very metal which for practical ends ought to be avoided. It is greatly to be wished that Mr. Bidwell will so far further improve the capabilities of his apparatus as not only to be able to get conclusive results with other metals, but also so as to enable him to say why his apparatus gave results that were unsatisfactory and inconclusive with good conducting metals such as platinum and copper.]

The Soaring of Birds

FOR more than twenty years I have watched with admiration the soaring of the black vulture of Jamaica (*Vultur aura*). When once well up in the air it rarely moves its wings, except to change the direction of its flight. It can soar whenever there is even a light wind.

I entirely concur with Mr. Hubert Airy in the main point of his general conclusion, as given in vol. xxvii. p. 592. "Variations in the strength and direction of current" can, as he says, be so "utilised" by birds as to enable them to soar. But a high wind is not necessary; and a downward current, even when approaching the perpendicular, may, if of sufficient velocity, be utilised.

Whenever there is a wind there will be ascending and descending currents in some places. This will be so even in a level plain which presents no considerable obstacles, such as trees or buildings, to the stream of air. The plain will be bounded by hills of varying height, and it will vary in breadth. A stream of water would merely flow more rapidly through the narrower channels; but a stream of air, being highly elastic, will also rise and fall, and it will have its eddies in planes more or less inclined to the horizon, and will often acquire a rolling motion. Assuming the existence of ascending and descending currents, the soaring is a very simple matter. *The bird contrives to remain much longer in the upward currents than in the downward.* It will glide along the ascending side of a wave of air and cut across the descending side. It will make many spiral turns in an ascending current of sufficient amplitude. I have often seen the vulture ascend thus for more than 2000 feet, keeping near a steep mountain side. If the bird encounters a descending current, of which it is instantly aware through the diminished pressure on its wings, it will either wheel to the right or left to get out of it, or, altering the pitch of its wings, will descend swiftly so as to acquire the necessary impetus for a rapid escape, or will do both.

It can also avail itself of inequalities in the velocity of horizontal currents flowing parallel to one another at the same elevation. The bird, let us suppose, encounters a strong horizontal current, as warm as it is rapid, issuing from a mountain valley or a cutting through a forest. Instantly throwing its wings into a plane nearly vertical, it receives on them the force of the current, and in a few seconds acquires its velocity. Pitching its wings also for a downward flight it shoots quickly through the current, having acquired a speed more than sufficient for the recovery of its original elevation. If the current be very strong and very narrow, it need not be horizontal, but may approach the perpendicular. The bird will not remain in it long enough to be carried far down, while it acquires an impetus much more than compensating for the slight loss of elevation. It must be remembered that when the bird is gliding at a high rate of speed, the resistance of the air, through its inertia, to any movement except in the plane of the wings, almost equals that of a solid body, and a change of direction causes a very slight loss of momentum.

What rapidity of currents is necessary for soaring must depend in great measure on the structure of the bird. The vulture is, I believe, comparatively heavy, but I think that, having once acquired speed by a short and steep descent, it can glide through still air (or at right angles through air having a uniform horizontal notion) at the rate of twenty miles an hour, descending not more than one in twenty. If, therefore, the bird could be always in an upward current of only one mile an hour, it could maintain itself in the air. A gentle breeze of ten miles an hour, with one mile an hour of ascent—and a much steeper ascent than this must be frequent enough where there are hills—would suffice to sustain the bird; and as an average of ten miles an hour implies local or occasional gusts of greater velocity, of which the bird knows how to avail itself, it could ascend in such a current, and so be able to work to windward. If besides hills of moderate inclination, there are also trees, walls, houses, the air will often be driven upwards, vertically or nearly so, with as great or even greater speed than that of its average horizontal movement; and of this upward movement the birds avail themselves most skilfully. I have frequently seen the vultures working their way thus against a high wind. Their movements are very irregular. Sometimes, to avoid a violent gust, they will drop almost perpendicularly to within a yard or two of the ground, and shooting abruptly sideways with the high velocity gained by the drop, will get into an upward current in which, if ample enough, they will wheel, or else will cross and recross it, till they have gained a sufficient elevation, and then, taking advantage of a lull, will glide to windward.

With a breeze of only five miles an hour, there will be in many places upward currents of high inclination caused by the usual irregularities of surface. Keeping sometimes in these and sometimes in currents more slightly ascending, for, say, two-

thirds of its time, and utilising also, as I have above explained, the more rapid of the descending currents, the bird can more than sustain itself. It can at will glide to windward at the rate of fifteen miles an hour against the breeze, losing of elevation only one in twenty.

R. COURTENAY

L'Ermitage, Hyères (Var), France, April 28

Flight of Crows

I CAN corroborate the observation of Mr. Murphy as to the oblique flight of crows. When I have seen them so flying there has always been a cross current, and they have merely kept their heads a little to the wind.

Cambridge THOS. MCKENNY HUGHES

Sheet Lightning

Du choc des opinions jaillit la vérité. I still adhere to your assertion that sheet lightning is not, at least in most cases, the mere reflection of a common but distant storm. On the highlands of Ethiopia, in the years 1842 to 1848 I was diligently engaged in investigating the electrical phenomena so frequent in that region. The details of my observations were printed in 1858 by the French Institute, and I have published again my results in my "Observations relatives à la Physique du Globe" (Paris, 1873). The following cases may be of interest :—

Near the zenith eight successive flashes of lightning were seen 21 seconds before their thunder, which lasted exactly 12 seconds. Another day it lasted 24·4s. thirty successive times, and, as previously, without any rain. My greatest observed interval was 111·2s., corresponding to a distance of 38,500 metres, &c.

I have seen more than once straight or zigzag lightning unaccompanied by thunder. One afternoon it went to and fro *twice* between two horizontal cloud banks, and ended in sheet lightning which illuminated, not the lower dark bank, but only the under surface of the upper cloud. I have observed frequently thunder without lightning and lightning without thunder.

When in Adwa I recorded silent sheet lightning towards Gondar, 240 kilometres distant, where a violent storm was raging at the same time. Before leaping to a hasty conclusion, let us hear a case bearing pointedly to the opposite opinion : in 1845, at Saga (latitude 8° 11′), a semi-transparent fog which had mantled over the valley, and could not be more than 3500 metres distant, gave out a flash of sheet lightning without thunder.

Although my numerous observations have given me a strong bias in favour of your opinion, I do not wish to impose it on reluctant philosophers, but suggest the following system to clear up the question :—Let two observers, A and B, 40 or 50 miles asunder, mention instances of lightning seen in each other's true bearing. If they can also secure the help of a third observer located on or near the straight line from A to B, and who can watch in two opposite directions, many important results may be obtained.

ANTOINE D'ABBADIE

Paris, May 5

The American Trotting-Horse

MR. BREWER's memoir on the evolution of the breed of the American trotting-horse (NATURE, vol. xxvii. p. 609), and the statistical tables that accompany it, are full of interest, but I only propose now to concern myself with the latter, which may be easily and usefully discussed by employing a statistical method that I have long advocated. In explanation I will begin by extracting the final terms of four of the lines of his table, as follows :—

Year.	2.27 or better.	2.25 or better.	2.23 or better.	2.21 or better.	2.19 or better.	2.17 or better.	2.15 or better.	2.13 or better.	2.11 or better.
1871	99	40	17	12	6	1			
1874		98	40	16	11	5	1		
1877			105	51	19	8	2		
1880				106	41	14	6	2	1

The meaning of these entries are, that in the year 1871 there were 99 horses that could trot a mile in 2 minutes 27 seconds, or less ; that in the same year there were 40 that could trot it in 2 minutes 25 seconds, or less ; and so on. Their significance is that the rate per mile of the hundred fastest American trotting-horses has become 2 seconds faster in each successive period of 3 years, beginning with 1871, and ending with 1880 ; also that the *relative* speed of the hundred fastest horses in each year is closely the same, though their absolute speed differs.

We may read the table in another way. If the number of horses that can run a mile in 2 minutes 27 seconds or less is 99, we may infer without risk of sensible error that the 99th horse in the order of running accomplishes a mile in *that time exactly*, because the 100th horse certainly takes a longer time, and it is statistically incredible that the rate of the 99th and of the 100th horses should differ by more than a barely perceptible interval. For the same reason we may infer that the 40th horse in that same year runs a mile in 2 minutes 25 seconds, and so on. We can now draw curves, and by graphical interpolation find with the greatest facility the mile rate of the horse in *any* order of running in any year that we please to select. I have selected the 100th, 50th, 20th, and 10th horse respectively for each year beginning with 1874, when we are informed that the returns first begin to be accurate, and have thrown the results into the following simple table. The curves obviously required a little smoothing here and there, and in three or four places the readings have been thereby modified by one or two tenths of a second. Otherwise they are given directly from the rough plottings.

Number of Seconds and Tenths of Seconds in Excess of Two Minutes that are required for Running One Mile by the Horses whose Order in the Rate of Running in each Year is given at the Top of the Columns

Year.	100th.	50th.	20th.	10th.
1874	25·1	23·4	20·5	18·8
1875	24·1	22·5	19·9	18·2
1876	23·5	21·6	19·5	17·7
1877	22·9	21·0	19·0	17·4
1878	22·1	20·2	18·5	17·0
1879	21·3	19·6	18·0	16·6
1880	20·8	19·3	17·6	16·0
1881	20·4	18·8	17·2	15·7
1882	19·9	18·4	17·0	15·4
Anticipated 1890	16·8	15·5	14·4	13·4

Mem.—The first horse runs the mile in about 5 or 6 seconds less than the tenth horse.

It will be found on plotting the figures in the vertical columns into curves, that they run with much regularity and differ little from straight lines. The general conclusion to be derived from them is that the improvement of the running shows as yet little tendency to slacken, though no doubt if the number of horses bred for trotting ceased to increase yearly at the same large rate as hitherto, it might do so. Supposing, however, the conditions to be maintained, I should anticipate that in 1890 there will be about 15 horses that will run a mile in 2 minutes 15 seconds or less, and that the fastest horse of that year will run a mile in about 2 minutes 8 seconds.

FRANCIS GALTON

The Shapes of Leaves

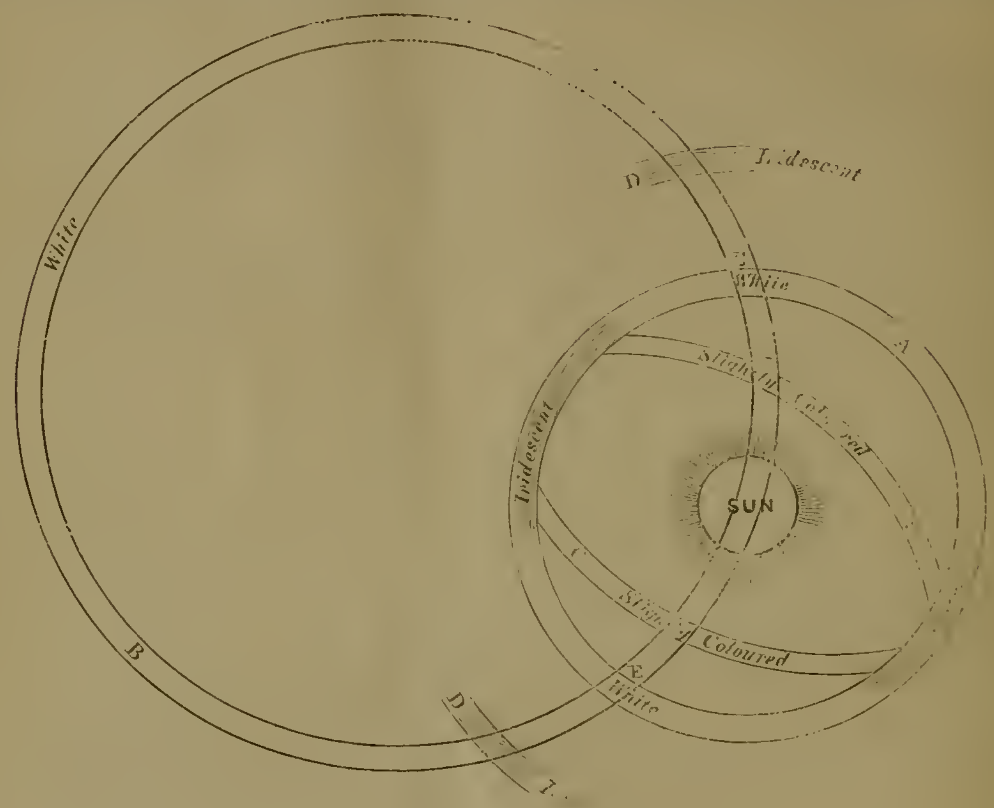
MR. GRANT ALLEN's papers in NATURE will evidently serve to direct attention to a most interesting subject which hitherto appears to have been much neglected. Every contribution of observed facts may tend to throw further light upon it, and I therefore venture to remark that one cause of the frequently filiform character of the leaves of water-plants appears to be the elongating action exercised upon the cells by the pressure of a rapid current of water, since it is obvious that growth must take place in the direction of the least resistance. With a radiate-veined leaf the tendency must be towards lateral pressure, which would compress and elongate, and so give a linear form to the leaf-cells. I have been much interested to observe that on the seashore, in places where Fuci are exposed to this action by the ebbing tide, as when growing on the edge of a large boulder or hanging over its sides, the fronds and even the receptacles become unusually elongated.

On the other hand, where a freshwater stream mingles with the salt water in pools left by the tide, and the endosmotic action of the water set up by its reduced density is greater, the algæ become broader if flat, or of more inflated character if tubular. This is well seen in *Dumontia filiformis*, *Enteromorpha intestinalis*, and *Chondrus crispus*. The influence exerted by the character of the surrounding medium and pressure may also be observed in that interesting genus of freshwater plants, Callitriche.
E. M. HOLMES

Solar Halo

I BEG to forward herewith diagram and remarks of solar halo as observed here to-day, thinking it may be of interest, being an unusual phenomenon. The cause depends upon many circumstances necessary for such observations, chiefly a calm reflecting surface of water in front, behind, and around the observer, making their appearance local as well as unfrequent.

The halo marked A, caused by the sun's rays passing through the thin cirri, was reflected from the surface of the water on the English Channel side of the island, producing the large white halo B, and passing over the sun's centre, the non-concentric arcs C being most probably reflected from the harbour side, the bright iridescent arcs at D on the large white halo B being the reflected crossings of the two halos at E. I have fixed the points of the zenith and due south horizon as approximately as possible; the observer facing due south, the iridiscent arc D was nearly



Solar Halo as observed at Portland, Dorset, April 28, 1883.

vertical, and about the same distance from the sun's centre to the estimated centre of large halo B, viz. 12°; the diameter of halo B was a little over double that of A.

Latitude of Verne ... ... 50° 32′ 86″ N.
Longitude of Verne ... ... 2° 23′ 40″ W.

Altitude of highest point of Verne 495 feet above sea-level (Ord. B.M.).

Time (local): first observed at 12.20 p.m.; brightest aspect 12.30 p.m.; duration about three-quarters of an hour. Cloud, thin cirri, with cumulo-stratus low in northern horizon. Amount, 9. Wind, S.
E. CARDWELL,
Late Supervisor Meteorological Department, Bombay
Verne Citadel, Portland, Dorset, April 28

I SEND inclosed a diagram of a system of solar halos observed here on Saturday last. If one may credit the oldest inhabitant, the phenomenon is very rare in these latitudes; in fact the ancient mariners frequenting the New Key End declare they never saw the like in all their wanderings.

My attention was first called to it at 12.25 p.m., when it presented the appearance I have depicted; but I am told that earlier in the day a white halo was seen *south* of the sun. The smaller circle had the sun for its centre, round which the sky was of a leaden cast as far as the edge; the fringed portions represent brilliantly coloured partial halos, or coronæ. The larger circle was, as near as I could guess, 40° diam. Its circumference cut the centre of the smaller circle, was brilliantly white, perfectly defined as seen from here, and narrow. I am told that, seen from high ground some four miles from here, it presented the appearance of two horns. The phenomenon lasted after I observed it about an hour, during which time a peculiar haze was drifting over the sky, which, when viewed carefully, seemed to have a hair-like structure, especially when seen passing the bright edge of the larger halo. I may add that the surface wind was southerly, the drift of the haze S.S.E.: a line joining the centres of the circles would point E.N.E. at about one o'clock.
THO. B. GROVES
Weymouth, April 30

Mock Moons

THE explanation of the phenomenon observed on the 16th ult., which is given by "Sm." of Rugby, appears to me to be scarcely satisfactory. According to the usual explanation of halos, parhelia, and paraselenæ, which attributes them to refrac-

tion by prismatic ice-crystals at high elevations, the parhelia always appear on the horizontal parheliacal ring which passes through the centre of the sun, and generally at the intersection of this ring with the vertical halo. The two parhelia must therefore always lie in a line parallel to the horizon, and at the same elevation as the sun itself. The same laws regulate the appearance of the paraselenæ or mock moons. It therefore surprised me to learn that the left-hand mock moon appeared at a greater distance from the horizon than the right-hand one. It seemed to me to be "unaccountably out of place." That the circle should have subtended an angle of 50°, as stated by "Sm.," is in itself unusual. The normal diameter is understood to be from 44° to 47°. Did "Sm." actually measure it? To my judgment it was considerably more than this, but of course mere estimates are not trustworthy. I do not see how a "change of level of the refracting cloud" should alter the position of the mock moons. This must depend upon the relative positions of the moon and the observer's eye. If the cloud is not in the right place no mock moons will be seen. I should be glad of a satisfactory explanation of the phenomenon recorded.

Birstal Hill, Leicester, May 7 F. T. MOTT

REFERRING to a letter from Mr. F. T. Mott in NATURE, vol. xxvii. p. 606, I find that at midnight on April 16 the moon's apparent altitude at Leicester was not more than 26°; so that after allowing for the difficulty of seeing the actual horizon, and taking also into account the breadth of the halo, it seems improbable that the halo observed by Mr. Mott was of unusual size.

I have, however, seen a description somewhere of a *parhelion*—measured with a sextant about the end of last century—which had a semidiameter of 26°. It would be interesting to know whether such irregularities in the dimensions of these phenomena have been accurately ascertained. R. C. JOHNSON

19, Catherine Street, Liverpool

Sun Pillar of April 6, 1883

IT may be of interest to record the various points from which the above phenomenon was seen. I was at St. David's with a party of geological students, and we watched it for some time as we were returning from the coast at sunset.

Cambridge THOS. MCKENNY HUGHES

Fibreballs

I READ with much interest the letters of Prof. G. H. Darwin and "J. H.," NATURE, vol. xxvii. pp. 507, 580. On the coast of South Australia, especially on the Coorong beach, I have seen fibreballs in great quantity; some larger than a cricket ball, and perfectly spherical, hard, and well-matted; others tapering and having the form of an exceedingly long ellipse. I brought home many specimens. These are now in the Wragge Museum at Stafford; and I shall be happy to have some forwarded for Prof. Darwin's inspection.

Fort William CLEMENT L. WRAGGE

Helix pomatia

ONLY a few more lines to say, in consequence of the communication of Mr. Stokoe in your last number (p. 6), that he will find the mollusca in their geological relations treated in the introduction to my work on "British Conchology," vol. i. p. cix. The distribution of *H. pomatia* in this country and on the Continent is noticed in pp. 177 and 178 of that volume, and in the supplement to the fifth volume. J. GWYN JEFFREYS

1, The Terrace, Kensington, May 4

I HAVE found this freely in the hedge-bottoms of Hertfordshire lanes, where the soil was a dark alluvial mould, certainly not cretaceous. I suspect that even in its known localities it is very local. HENRY CECIL

Bregner, Bournemouth, May 5

IN two of the localities mentioned for this snail—Dorking, Surrey; and Woodford, Northamptonshire—there seems some reason to suspect it to be a modern introduction. From 1849 to 1852 I lived within two miles of Woodford, and often found the shells in a small wood known as Woodford Shrubbery. It was commonly said in the neighbourhood at that time that the snails were brought from abroad by the gentleman—I think General Arbuthnot—who had formed the Shrubbery some thirty years before that date.

I also found, many years ago, shells of the same species about the foot of Box Hill, near Dorking, and was told by a former resident in that neighbourhood that the snails were brought from Italy by Mr. Hope, of Deepdene, who was well known in the early part of this century as a writer on the mediæval architecture of Italy. I give the statements for what they may be worth.

Loughton J. C.

Intelligence in Animals

IN addition to the long list of "emotions which resemble human intelligence as occurring in animals below the human species," as given by your correspondent on the authority of Dr. Romanes (NATURE, vol. xxvii. p. 580), and the instance of "benevolence" subsequently cited, I venture to submit the following as illustrating something very like the emotion of contempt.

Until recently our domesticated animals included two cats—one a very fine tabby (a trimmed male) of somewhat morose nature, and a pretty little black cat, a half-bred Persian (a female) of very gentle character. On a noticeable occasion the tabby cat caught a mouse and ate it all up with much relish in a corner of the room. The proceeding was watched with much interest by the black cat from her place on the hearthrug. After the tabby had finished his repast he also took up his place on the hearthrug. The black cat then went over and smelled the spot where the dainty morsel had been devoured. Upon this the tabby cat came up and "boxed" the black cat's ears once or twice, as who should say, "What business have you with my affairs? catch your own mice!" W. R. HUGHES

Handsworth Wood, near Birmingham, May 5

MAY I contribute another case of higher thought in the lower animals. At the farm of Granton Mains, near Edinburgh, an old cat had become blind; her daughter had kittens. The daughter was observed bringing in a sparrow to the boiler-house, where her blind mother and her half-grown kittens were warming themselves: the kittens came up to get the sparrow, but their mother kept them off and gave the sparrow to her mother, and watched whilst she ate it. She was frequently seen to give other food to her blind mother.

My children have a fox terrier bitch, "Dot." Dot loves to kill anything from a cat to a mouse, and sometimes a wild rabbit gets into the garden, and it is a red-letter day for Dot and the children. But the children have also tame rabbits; of course any one who knows dogs will understand that it is simple to teach them not to touch pets—for instance, the cat of their own house. But Dot had a curious case to decide. The children had found a nest of wild rabbits, and two of the tame rabbits (black and white) had made a hole in a bank and there had young ones. This nest was respected by Dot. The children took the young wild rabbits (gray) and fostered them on the tame ones by slipping them into the nest. A few days after this, Dot must have discovered these gray young ones with the black and white. Had she found them anywhere else, one snap, and they were dead; but this was the line she took: she was found at the front door under the porch with one of the young gray rabbits, quite fifty yards from the nest; it was quite unhurt, although it died afterwards, I believe from cold and exposure at the time. Are we to suppose that Dot wished to ask the question, "May I kill this gray one?" DUNCAN STEWART

Knockrioch, May 2

THE SOLAR ECLIPSE OF 1883

THIS eclipse, as our readers have already been made aware, took place on Sunday last, and we may hope, although we shall not know for more than a month, that the weather was favourable. We shall not hear whether the French arrived in time, but we do know that the English observers met the American party, consisting of Prof. Holden, Dr. Hastings, Mr. Rockwell, Mr. Preston,

	SIDEROSTAT.					EQUATORIAL.				PHOTOHELIOGRAPHS.	
Time.	Hilger.	Rowland grating. 1st order.	Rowland grating. 2nd order.	Prismatic camera.	Slit Spectroscope. 2 prism.	7 prism. F.	Grating. F. Red 1st order.	Grating. F. Blue 2nd order.	¼ in. slide. Dense prism F.	Large photo-helio-graph.	Corona camera.
Before Totality Minutes.											
10						expose	expose	expose			
9											
8									expose		
7									expose		
6	ref. spectrum 30 scs.								expose		
5									expose		
4									expose		
3							expose	expose	expose		
2						expose			expose		
Seconds.											
60									expose		
40		expose	expose				run ¼ in.				
20		expose	expose								
2	expose & start clock										
1											
Totality		expose	expose	expose col. plate	expose	expose	expose	expose	expose	expose	expose 1 sec.
280											expose 20 sec.
230				shut							
220				expose gel. plate							expose
210									expose	expose	
200		expose	expose								
120				shut							
110				expose col. plate							
100											
90											shut
70											expose 3 sec.
50										expose	
40											expose 10 sec.
20											expose 2 sec.
Just before end		expose	expose	shut	shut	expose	expose	expose	expose	shut	
After Totality Seconds											
1							run ¼ in.				—
4		expose	expose								
10		expose	expose								
Minutes											
1		shut	shut				expose	expose	expose		
2	shut										
3									expose		
5						expose	expose	expose	expose		
7									expose		
9									expose		
10						shut	shut	shut	shut		
	refs. 25				2 sec.	10 sec.	10 sec.	10 sec.	1 sec.	run	run

Lieut. Brown, and Mr. Upton, the first mentioned astronomer being in charge, at Panama. They expected to arrive at Callao on the 20th March last, and to leave either in the *Hartford* or the *Pensacola* within the next few days. That would give them ample time to reach the Caroline Islands, and make the arrangements necessary for the observation. It was the intention of Prof. Holden to take the combined English and American party on to Flint Island if he found that Dr. Jannsen had already established his party on Caroline. This, of course, was a very proper decision, as it would double the chances of favourable weather. We give the time-table for observation supplied to the English observers, which they were instructed to carry out down to its most minute detail, if all the instruments were landed and set up without damage.

It will be seen that the English attack was to be entirely photographic; no eye observations were to be made. And if all has gone well, something between fifty and sixty photographs may be hoped for. The table perhaps requires a little explanation, which we will now proceed to give. It followed from work undertaken with that special object in Egypt last year, that eclipse observations can now definitely begin ten minutes before totality, and end about ten minutes after. With an eclipse therefore of about five minutes' duration, as in the present case, the work ranges over a period of five-and-twenty minutes, and if the plates are as sensitive as they can now very well be, it is quite easy to see that a very large number of photographs may be taken. The greatest interest of course attaches to the spectroscopic photographs to be taken by means of a siderostat and the equatorials. As in former eclipses the plates exposed in the photoheliographs will secure the appearance of the corona surrounding the image of the dark moon; but on the present occasion an attempt was to be made to take these photographs on a much larger scale than usual, a scale of four inches to the moon's diameter. Coming to the spectra themselves, we find

four spectroscopes fed by the light reflected by a siderostat of 30 cent. diameter, these four spectroscopes being bracketed together very much like a Gatling gun, and pointed to the siderostat, which has a very excellent clock attached to it. The first spectroscope, called the "Hilger," in the programme, is an integrating one, and will integrate for us the light of the whole region round the sun during the entire period of totality on a plate which is allowed to fall very slowly by means of a clock-work arrangement. If any change, therefore, takes place in the spectrum of these regions during this period, it will be recorded on this moving plate in historical sequence, so that, the beginning and end of exposure being known, the time at which any definite change takes place can be determined. The Rowland grating coming next on the list, which was generously given to Mr. Norman Lockyer by Professor Rowland, is one of ten feet focus, and has a large surface with 14,000 lines to the inch, forming of course a most excellent and simple prismatic camera, the first and second order spectra both being utilised. The prismatic camera and slit spectroscope of two prisms were two instruments arranged by Capt. Abney for the eclipse last year. They are on the model of the instruments designed for the eclipse of 1875 in Siam, but have the advantage of possessing plates which are sensitive to the whole of the spectrum. The work to be done by the equatorial is of a very similar nature to that to be attempted with the siderostat, except that it was intended by varying the time of exposure from long to very short periods to make certain of something. All the cameras, except the "Hilger," in which the plate moves by clockwork, are fitted with long plates, of which only small strips are exposed at a time, and the exposure is managed, not by changing the plate as in the ordinary method, but by turning a screw. The word "expose" in the table therefore shows the precise moment, at which, if the instructions are carried out, a new strip of plat will be exposed to the action of the light before, during, and after totality, and it will be seen that the exposures are varied both before and after totality, so as to get the greatest possible difference in time during which each part of the plate receives its impression. From a letter received by Mr. Norman Lockyer from Messrs. Lawrance and Woods, we know that the American astronomers intended giving them all possible facilities for carrying out the combined Royal Society and Solar Physics Committee's programme; and that the attention of the English observers will be concentrated on the siderostat and equatorials, as two officers of the American ship have been told off to work the photo-heliographs and look after the eclipse clock, which is so arranged that it keeps all the observers together by indicating to each one of them the exact number of seconds still left for his work, with the additional advantage that each number of seconds announced by the officer in charge is a distinct order to do a certain thing, as in the case of the various exposures indicated in the list.

LECTURES TO WORKING MEN

THE three courses of Lectures to Working Men given at the Museum of Practical Geology, Jermyn Street, during the present session, by the staff of the Normal School of Science and the Royal School of Mines, came to an end last Monday, and, as on former occasions, it gave rise to regret that more cannot be done in this direction, both with regard to the number of courses given, and the number of persons accommodated in each case. The theatre at Jermyn Street restricts the audience to something over 600, while of late years the applications for tickets have never been less than 2,000. The tickets for each course—for which sixpence is charged as a registration fee—are given only to *bonâ fide* working men, who must bring with them a paper on which is stated their name and occupation. Some of the lectures of the last course—that given by Mr. Norman Lockyer, on "The Earth and its Movements"—were listened to by the Japanese Minister, and an official connected with the Education Department of Japan. At their request a list was drawn up showing the trades of the audience. This list, in the case of 500 who attended the last course, we are permitted to give, and we think our readers will find it an interesting one. Seeing that there is this anxiety on the part of working men to learn, and that less than one in three of those so desirous of learning can have an opportunity of doing so, we trust that in future years the Lecture Theatre at South Kensington will be utilised in this direction, as well as that at Jermyn Street. There is little doubt, of course, that a Liberal Government, represented by the Treasury officials, naturally anxious in all ways to protect the public purse against all claims, whether good or bad, might object to this being done at the public cost, but seeing that the lectures are given as a labour of love by the various professors such an objection would scarcely be urged, and we confess too that we should not only like to see the theatre at South Kensington utilised in this way, but the theatres at University College, King's College, and other institutions that might be named. We do not believe that the professors at these institutions are less anxious for the progress of knowledge among the working classes than those who are connected with the Government School, and this being so, we may hope to see at some future time a united effort to supply what is at present a great want, and a gap in our educational programme.

Trades of 500 of the audience at the last course of Lectures to Working Men, April and May, 1883:—

1 Bag Maker.
6 Bakers.
2 Basket Makers.
28 Boot and Shoe Makers.
1 Brewer.
2 Brush Makers.
1 Billiard Table Maker.
6 Builders (Foremen).
1 Butler.
3 Brass Finishers.
2 Bricklayers.
7 Bookbinders.
6 Cabinet Makers.
52 Carpenters and Joiners.
8 Coach Painters.
9 Compositors.
8 Carvers.
1 Cigar Maker.
4 Chemists and Druggists.
13 Clerks.
3 Curriers.
6 Dentists.
4 Designers.
1 Die Sinker.
1 Draper.
2 Draughtsmen.
25 Engineers.
2 Engravers.
1 Envelope Maker.
1 Fishing Rod Maker.
5 Gasfitters.
1 Gardener.
7 Gilders.
1 Greengrocer.
2 Grainers.
3 Glasscutters.
4 Gun Makers.
7 Hatters.
1 Hairdresser.
1 Hinge Maker.
1 Hammerman.
1 Hemp Dresser.
23 House Painters.
6 Iron Founders.
23 Instrument Makers.
37 Jewellers.
1 Lamp Maker.
1 Lead Glazier.
3 Lithographers.
1 Locksmith.
4 Stonemasons.
1 Mattress Maker.
2 Milkmen.
6 Opticians.
9 Pianoforte Makers.
3 Perfumers.
6 Photographers.
1 Picture Frame Maker.
16 Plumbers.
2 Pocket Book Makers.
2 Polishers.
10 Porters and Messengers.
6 Portmanteau Makers.
6 Plasterers.
1 Quarryman.
6 Salesmen.
5 Saddle & Harness Makers.
1 Saw Maker.
1 Soda Water Bottler.
7 Shop Assistants.
4 Stationers.
1 Smith.
1 Stoker.
2 Storekeepers.
17 Tailors.
4 Teachers.
1 Traveller.
6 Tinmen.
5 Turners.
1 Twine Spinner.
2 Umbrella Makers.
6 Upholsterers.
8 Watch Makers.
1 Warehouseman.
3 Wheelwrights.
7 Zinc Workers.

CIRRIFORM CLOUDS

IN a "Note on a Proposed Scheme for the Observation of the Upper Clouds" the Rev. W. Clement Ley has written an abstract of part of a large work on clouds, which he is now preparing for publication. This note has been circulated with a view of obtaining suggestions on the scheme of classification, observation, and telegraphy, which the writer has submitted to his colleagues of the Committee on Cirrus observations, nominated by the International Meteorological Committee in 1882.

The author follows the primary outlines of cloud-classification proposed by Luke Howard, dividing the objects of observation into cirriforms, cumuliforms, stratiforms, and composites; while in the subdivision of these primary types he has been induced by reasons, the cogency of which he hopes to demonstrate, to deviate very considerably from Howard's classification. The true cirriforms, to the discussion of which the note is restricted, are divided by Mr. Ley as follows: cirrus, cirro-filum, cirro-velum (with its variety mammatum), cirro-nebula, and cirro-granum. The author has been, "after many years devoted to the consideration of the subject, reluctantly compelled to give up the employment of the two terms 'cirro-stratus' and 'cirro-cumulus.' Their use has led to endless confusion. In point of structure the clouds usually called cirro-cumuli belong essentially to the higher stratiforms, consisting of nubecules separate, or partly coalescing, occupying a layer of atmosphere of very small vertical thickness, but of very great horizontal extent, and they are not formed in nature by those processes which are productive of clouds either of the cirrus or of the cumulus type. They are not, in fact, either in appearance or in mode of physical formation, either compounds of cirrus with cumulus or hybrids between cirrus and cumulus. Therefore in practice the use of the word cirro-cumulus has led to a large number of clouds of no great elevation being classified among the cirriforms, a result which was of little consequence when the laws regulating the upper currents of the atmosphere had received no examination, but which must be absolutely fatal to a scheme based upon those laws, according to which new and most valuable results will be attained. The name cirro-stratus is almost equally objectionable, and for similar reasons."

Six pages of this note are devoted to instructions on a system of observing and reporting by telegraph the structure and movements of the upper clouds; and the author shows that, if this system be adopted on an extensive scale, results of great practical importance may be anticipated. The indispensable pre-requisite is a clear and scientific classification of clouds according to physical structure.

SCIENTIFIC PROGRESS IN CHINA AND JAPAN

VARIOUS steps in the progress of China and Japan in the adoption of Western science and educational methods have from time to time been noticed in these columns. To the popular mind the names of the two countries are synonymous with rigid unreasoning conservatism and with rapid change respectively. The grave, dignified Chinese, who maintains his own dress and habits even when isolated amongst strangers, and whose motto appears to be, *Stare super vias antiquas*, is popularly believed to be animated by a sullen, obstinate hostility towards any introduction from the West, however plain its value may be; while his gayer and more mercurial neighbour, the Japanese, is regarded as the true child of the old age of the West, following assiduously in its parent's footsteps, and pursuing obediently the path marked out by European experience. There is considerable misconception in this, as indeed there is at all times in the English popular mind with regard to strange peoples. Broadly speaking, it is no doubt correct to say that Japan has adopted Western inventions and scientific appliances with avidity; that she has shown a desire for change which is abnormal, and a disposition to destroy her charts and sail away into unsurveyed seas, while China remains pretty much where she always was. She is now, with some exceptions, what she was twenty, two hundred, perhaps two thousand years ago, while a new Japan has been created in fifteen years. All this, we say, is true, but it is not the whole truth. China also has had her changes; not indeed so marked or rapid, not so much in the nature of a *volte-face* on all her past as those of her neighbour. The radical difference between the two countries in this respect we take to be this: that while Japan loves change for the sake of change, China dislikes it, and will only adopt it when it is clearly demonstrated to her that change is absolutely necessary. To the Japanese change appears to be a delightful excitement, to the Chinese a distasteful necessity; to the former whatever is must be wrong, to the latter whatever is is right. As a consequence of this difference between the two peoples, when China once makes a step forward it is generally after much deliberation, and is never retraced. Japan is constantly undertaking new schemes with little care or thought for the morrow, but with the applause of injudicious foreign friends. In a short time she discovers that she has underrated the expense or exaggerated the results, and her projects are straightway abandoned as rapidly and thoughtlessly as they were commenced. Swift suggested as a suitable subject for a philosophical writer a history of human projects which were never carried out; the historian of modern Japan finds these at every turn. Where, for example, are the results of the great surveys, trigonometrical and others, which were commenced in Yezo and the main island about ten years ago? A large, expensive, but highly competent foreign staff was engaged, and worked for a few years; but suddenly the whole survey department was swept away, and the valuable instruments are, or were recently, lying rusting in a warehouse in Tokio. The same story may be told of scores of other scientific or educational undertakings in Japan. An able and careful writer, Col. H. S. Palmer, R.E., who has recently, with a friendly and sympathetic eye, examined the whole field of recent Japanese progress, in the *British Quarterly Review*, is forced to acknowledge this. "Once having recognised," says this officer, "that progress is essential to welfare, and having resolved, first amongst the nations of the East, to throw off past traditions and mould their civilisation after that of Western countries, it was not in the nature of the lively and impulsive Japanese to advance along the path of reform with the calmness and circumspection that might have been possible to a people of less active temperament. Without doubt many foreign institutions were at first adopted rather too hastily, and the passing difficulties which now beset Japan are to some extent the inevitable result." It would be blindness to deny that the net result of the Japanese efforts is progress of a very remarkable kind, but it is a progress which in many respects lacks the firm and abiding characteristics of Chinese movements.

The proverb, *Chi va piano va sano*, which was recommended ten years ago to Japanese attention by an eminent English official, and apparently disregarded by them, has been adopted by their continental neighbours. To the blandishments of pushing diplomatists or acute promoters, the Chinese are deaf. However we may felicitate ourselves on our inventions, scientific appliances, "the railway and the steamship and the thoughts that shake mankind," our progress, the newspapers, the penny post, and what not, China will not adopt them simply because *we* have found their value and are proud of them. But if, within the range of her own experience, she finds the advantage of these things, she will employ them with a

rapidity and decision surpassing those of the Japanese. A conspicuous instance of this will be found in her recent action with respect to telegraphs. For years the Chinese steadily refused to have anything to do with them; the small land line which connected the foreign community of Shanghai with the outer world, was maintained against the violent protests of the local authorities, and the cable companies experienced some difficulty in getting permission to land their cables. But during the winter of 1879-80, when war with Russia was threatening, the value of telegraphs was demonstrated to the Peking Government. The Peiho at Tientsin was closed by ice against steamers, and news could only be carried to the capital by overland couriers from Shanghai. Before a year elapsed a land line of telegraph was being constructed between this port and Tientsin; in a few months the line was in working order, and the Chinese metropolis is now in telegraphic communication with every capital in Europe.

This conservatism, respect for antiquity, conceit, prejudice, call it what we will, has something in it that extorts our respect. Let us imagine a dignified and cultivated Chinese official conversing with a pushing Manchester or Birmingham manufacturer, who descants on the benefits of our modern inventions. He would probably commune with himself in this wise, whatever reply Oriental politeness would dictate to his interviewer: "China has got on very well for some tens of centuries without the curious things of which this foreigner speaks; she has produced in that time statesmen, poets, philosophers, soldiers; her people appear to have had their share of affliction, but not more than those of Europe; why should we now turn around at the bidding of a handful of strangers who know little of us or our country, and make violent changes in our life and habits? A railway in a province will throw thousands of coolies and boatmen out of employment, and bring on them misery and starvation. This foreigner says that railways and telegraphs have been found beneficial in his country; good, let his countrymen have them if they please, but let us rest as we are for the present. Moreover, past events have not given us such faith in Europeans that we should take all they say for wisdom and justice." A day will undoubtedly come when China also will have her great mechanical and scientific enterprises; but what we contend for here is that nothing we can say or do will bring that time an hour nearer. European public opinion is to China a dead letter; she refuses to plead before that tribunal. Each step of her advance along our path must be the result of her own reflection and experience; and our wisest policy would be to leave her to herself to advance on it as she deems best.

SINENSIS

PROF. LINDSTRÖM ON OPERCULATE CORALS[1]

THE extinct stony corals, the calicles of which are provided with calcareous opercula, have ever been a puzzle to naturalists, since they are almost entirely without parallel amongst existing Anthozoa. The genera and species are not numerous, and are all of Palæozoic age. By far the finest and best preserved specimens of the most important forms are found in the Silurian strata in the Island of Gothland in the Baltic, and are collected for the National Museum at Stockholm, where they come under the care of Prof. Lindström, the author of the present memoir, so justly distinguished for his palæontological researches generally, and especially for those on corals. In this memoir he gives a *résumé* of all the forms of operculate corals as yet known, embodying an immense amount of important new information derived from his own prolonged investigations on a series of most remarkable specimens which I had the advantage of seeing and having explained to me by him in the summer of last year. The whole paper forms a most valuable contribution to our knowledge of these especially interesting and peculiar corals.

FIG. 1.—*Goniophyllum pyramidale* (*mutatio secunda*), viewed from above, with the opercular valves *in situ* of the natural size.

The first operculate coral described was *Goniophyllum pyramidale*, which Bromell in 1729 placed amongst the corals. The best known, and the one concerning which there has been the greatest difference of opinion, is *Calceola sandalina*, which was first figured in 1749 from the collection of Rosinus of Hamburg, by Brüchmann, who pointed out the resemblance of the coral to the front of a woman's slipper.

Brüchmann referred Calceola to the corals just as Bromell had Goniophyllum, but this was mainly because neither he or other early authors following him were acquainted with the opercula belonging to the specimens. Linné placed Calceola with the Mollusca as *Anomia sandalina*. Later it was referred to the Brachiopoda, a position in which a large number of eminent modern authorities retain it. If Calceola stood alone, the gravest doubts might certainly be entertained as to its having any relations to the corals; but now that a series of clearly allied forms such as Goniophyllum and Rhizophyllum, also bearing opercula, have had their structure so fully and satisfactorily worked out as has been done by Prof. Lindström, it is hardly possible not to follow him in placing the whole amongst the Anthozoa. The curious arrangement of the septa in Calceola closely resembles that in Goniophyllum as regards the septa both in the calicles and on the opercula. It is almost impossible to doubt the Anthozoan nature of Goniophyllum, whilst both it and Rhizophyllum, which has like Calceola an operculum of a single piece only, demonstrate their close relation to numerous recognised Palæozoic corals by exhibiting intracalicynal gemmation, and developing, like many other corals, abundance of roots.

The author divides the *Anthozoa operculata* into two families—

I. Calceolidæ (or Heterotœchidæ), distinguished by having the septa on the inner face of the operculum not alike and a median septum the largest.

II. Aræopomatidæ (or Homotœchidæ), with the septa on the operculum all alike and no defined median septum.

The Calceolidæ include all those forms in which the operculum, whether composed of one or four valves, has this valve or valves marked inside with a stout prominent median septum.

The family falls into two groups—the one in which the operculum consists of a single valve containing three genera, namely, the well-known Calceola, distinguished by not multiplying by budding, being thus never compound, by having no root-tubes, and not showing vesicular structure internally; and two others—Rhizophyllum and Platyphyllum—in both of which calicynal gemmation occurs and the internal structure is vesicular, somewhat as in Cystiphyllum.

In Rhizophyllum, a genus founded by Lindström, the

[1] "Om de Palæozoiska Formationernas Operkelbärande Koraller." Af G. Lindström, Bihang till K. Svenska, Vet. Akad. Handlingar, B. 7, No. 4.

corallum is very much like that of Calceola in shape, but more elongated. It may be simple or form large masses by two modes of budding—either calicynal budding, or budding from the abundant rootlets. In the specimen of

FIG. 2.—*Rhizophyllum elongatum.* View of the interior of the calicle to show the six young calicles developing as buds within it.

Rhizophyllum here figured, six young corals are seen in the act of developing as buds in the interior of the calicle.

In consequence of budding taking place from the "rootlets," the author advances the new suggestion that these bodies, the nature of which has always been doubtful, are to be regarded as stolons. The operculum of Rhizophyllum, by which the mouth of the calicle is completely closed, is, as in Calceola, of a semicircular shape, with a prominent ridge-like median septum on its inner face. In *Rhizophyllum attenuatum*, a compound form from Louisville, in Kentucky, first described by Mr. V. W. Lyon under the genus Calceola, the stolon tubes partly serve to connect together adjacent calicles, partly become themselves developed into fully-formed calicles. Specimens of *Rhizophyllum Gothlandicum* are found in abundance in the Island of Gothland with their opercula detached. The separate opercula are also common; specimens with the opercula *in situ* are comparatively rare. A remarkably perfect one is shown in the accompanying figure.

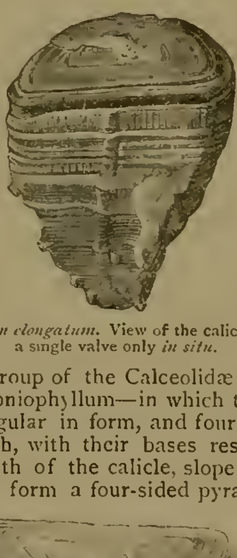


FIG. 3.—*Rhizophyllum elongatum.* View of the calicle with the operculum, a single valve only *in situ*.

The second group of the Calceolidæ contains only a single genus—Goniophyllum—in which the mouth of the calicle is rectangular in form, and four opercular valves are present which, with their bases resting on the four sides of the mouth of the calicle, slope inwards to meet one another, and form a four-sided pyramidal roof over

FIG. 4.—*Goniophyllum pyramidale* (*mutatio prima*). View of the mouth of the calicle, with the opercular valves *in situ*, the dorsal right and left valves being entire, the ventral incomplete.

the calicle. From Gothland Prof. Lindström has been able to obtain two or three specimens with all the four opercular valves *in situ*. So that doubts as to the connection of the valves with the coral calicle are now inadmissible. On careful study of these specimens and numerous others with a fewer number of valves *in situ*, he finds that the four valves always present well-marked differences and form, and septal striation, so that it is possible to distinguish with certainty an anterior and posterior, or ventral and dorsal, and a right and left valve, and pick these out from any collection of well-preserved loose valves. The anterior and posterior valves are trapezoid in form, the lateral triangular. A most remarkable discovery he has made is that these corals must have shed their valves periodically, and replaced them, and that when shed the valves frequently happened to become attached to the wall of the calicle, and fused with it. In the accompanying figure of *Goniophyllum pyramidale*, seen from behind, a well-formed operculum is seen near the base firmly attached to the wall of the calicle near its base. Such

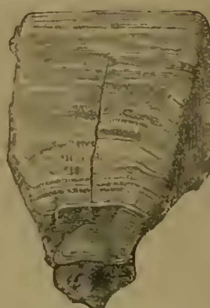


FIG. 5.—*Goniophyllum pyramidale*, viewed from the ventral side to show the shed opercular valve (*a*) coalesced with the wall of the calicle near its base.

specimens are not uncommon in Goniophyllum, but in another genus, a new one, Aræopoma, belonging to the second family of operculate coral, devoid of a median septum, and which has four triangular valves like Goniophyllum, very many specimens bear numerous valves fused to their outer walls, the valves being of increasing size from below upwards, in accordance with the growth and expansion of the coral and the mouth of its calicle. In one abnormal specimen of *Goniophyllum pyramidale* there are five opercular valves present, a minute extra triangular valve being interpolated at one of the corners.

Prof. Lindström, whose important researches on the development of the septa in so-called Rugose corals are familiar, has been able to trace the development of the corallum in *Goniophyllum pyramidale*. He finds that in the youngest stage of the calicle there is no septum present at all, then that one septum is formed on what may be termed the dorsal side of the calicle, and since in this genus and several others it remains conspicuously prominent, it may be termed the primary septum. Two further septa, the light and left median, are next formed, and last of all the ventral septum, long after the others. He points out that a similar process of development is followed in most Rugose corals, and that it is therefore erroneous to treat of four septa as primary in these forms.

In Rhytidophyllum shaped somewhat like Calceola, but belonging to the Aræopomatidæ, by reason of the absence of a defined median septum on the operculum there is only a single opercular valve. A further genus of the group is possibly represented by a single broken operculum, of which further specimens have not yet been found.

In connection with the operculate corals, Prof. Lindström describes certain coral forms in which remarkable exothecal structures are present, which may be considered as more or less homologous with opercular valves. In *Pholidophyllum tubulatum*, a compound coral, first described as *Tubiporites tubulatus* by Schlotheim in 1813,

and which has lately been made a subject of research by G. von Koch, the costæ are in solitary specimens clad each with a longitudinal rib composed of a double row of rhomboidal calcareous scales placed close together at an angle to one another as shown in the figure. These rows of scales form an almost complete outer covering to the corallum. The scales seem to be wanting in colonial specimens. They are extremely conspicuous and definite in some of the best preserved solitary specimens, and their regularity of disposition is such that it is impossible to believe that they do not definitely belong to the coral. They are shown enlarged in the figure. In vertical sec-

FIG. 6.—*Pholidophyllum tubulatum.* A calicle viewed from the side to show the rib-like prominences formed upon the costæ by the double rows of calcareous scales.

tions of the coral they are seen to be attached at their bases to the wall of the calicle. In *Syringophyllum organum* similar scales occur covering the exterior of the corallum, but these have a remarkable definite form somewhat like that of the bowl of a teaspoon with the handle cut off short.

As an illustration of the possibility of Anthozoa bearing opercula and scales, but not as implying any direct relationship between the Operculata and the Alcyonaria, Prof. Lindström points to the structure of *Primnoa lepadifera*, and figures one of the polyps viewed from above, showing how eight of the valve-like calcareous scales present in this species close in over the summit of the polyp, forming a conical operculum over it somewhat as in Goniophyllum, whilst the remaining scales form a representative of the calicle. He shows that, as in Goniophyllum, the opercular valves differ in form and size according to their position when *in situ*.

In his concluding paragraph he states that he considers the Calceolidæ to be probably nearly allied to such forms as Omphyma and Chonophyllum, whilst the Aræopomatidæ, on account of their vesicular internal structure, approach more nearly to Cystiphyllum. He does not consider the presence or absence of an operculum in corals to be necessarily of an important classificatory value, and cites the case of the presence and absence of opercula in Gastropoda as a parallel one of minor systematic importance. In this it is rather difficult to agree with him. Of course the opercula in Gastropods and corals are alike only in name, and as they occur so rarely in the latter the suspicion naturally arises that the two groups of the operculate corals may be more closely allied than he suspects. However, no man knows them better than he, or has a better right to an opinion on the subject, and his conclusion, guarded as it is, must be treated with all respect.

A very interesting result attained and given in tabular form on p. 91 is that in successive Upper Silurian strata a series of three modifications of form of *Goniophyllum pyramidale*, starting from *Goniophyllum pyramidale, primigena,* the form occurring in the lowest beds can be traced succeeding one another in time. Similarly *Rhizophyllum Gothlandicum, forma primaria* passes by a modification, *R. G. mutalia*, into *R. Gervillei*, of specific rank in the Devonian formations. Other operculate corals show similar modifications in progress of geological time. Prof. Lindström's important memoir cannot here be followed farther. It is illustrated by nine lithographic plates most beautifully executed in Stockholm, from which the engravings here given are copied, and are some of the most excellent ever published of corals.

H. N. MOSELEY

BARON NORDENSKJÖLD'S EXPEDITION TO GREENLAND

[THE following Programme, drawn up by Baron Nordenskjöld for his expedition to Greenland, has been kindly placed at our disposal by Mr. Oscar Dickson, who, with his well-known enlightened liberality, provides all the expenses. The Programme has not hitherto been made public, even in Sweden, as Baron Nordenskjöld did not wish to be interrupted in his preparations with correspondence on his plans and theories. The expedition leaves Sweden in the *Sofia* on the 20th instant, and will call at Thurso, where Baron Nordenskjöld will join the vessel.]

NINE centuries have elapsed since the Norwegian, Erik Röde, discovered Greenland, and founded Scandinavian Colonies; from these, Norwegian navigators some ten years after sailed south to "Vinland," the fecundus, *i.e.* to the shores of the present Canada and the United States, thereby acquiring the honour for the Norse race of being the real discoverers of the New World. It is not known whether these voyages led to any fixed settlements being established in America, but we know, on the other hand, from a number of Icelandic sources, that the colonies in Greenland became very flourishing. There were upwards of three hundred farms, "Gaarde," of which about two hundred, embracing twelve parishes, were situated in the "Österbygd," and about one hundred, embracing three or four parishes, were situated in the "Vesterbygd." During four centuries the country formed a bishopric, from which funds towards the Holy Wars were even contributed. Unfortunately, the connection between the colonies and the mother country ceased after a couple of centuries, while Greenland's ancient Norse population was extirpated, either through plagues or by "Skrællings," *i.e.*, the Eskimo who descended from the North. Another explanation of their disappearance is that they lost their nationality, and were absorbed into the Eskimo population, during their contact with the more numerous tribes of the Polar regions, whose mode of living was more suited to the climate of the country and the resources at their disposal. However that may be, there remains the fact that one of the most distinct and enterprising peoples in the world have been annihilated, or, perhaps, absorbed in one distinguished as among the lowest, both physically and intellectually. The old country, belonging to the Norwegian Crown, was even so far forgotten, that it was only Columbus's discovery in the south which recalled the attention of the Norsemen to the fact that they had once colonised a part of this world, which was being parcelled out among the southern nations by "Bulls" from Rome, as if it had just been discovered.

By the aid of traditions and old journals of navigation several attempts were made to reach the old, forgotten colonies from Iceland, but these were frustrated by the enormous masses of drift ice, which then seemed to inclose the shores of Greenland's east coast, more than formerly. Eventually, John Davis, in his attempts to find the north-west passage, discovered that the west coast was easily reached, and that the seas around it offered a fine hunting-ground for the profitable whale-fisheries. This, with the reported discovery of gold in the country resulted in the despatch of several Danish hunting and commercial expeditions, which did not, however, meet with much success, until the Norwegian

Hans Egede, by his zeal for bringing the blessings of the Gospel to the descendants of the old Norse colonists, caused trading and missionary stations to be established on the west coast. These were subsequently considerably augmented and extended, and henceforth held by the Danish crown, under "The Commercial Association of Greenland."

Greenland was thus inhabited by Norsemen from 983 until the 15th century, and its west coast has, during the last hundred and sixty years, been a place of sojourn for many able Danish administrators and missionaries. Besides this, nearly all expeditions which have been bound for the American Polar sea have stayed here more or less, while the west coast has on many occasions been the object of carefully prepared expeditions of research. This part of Greenland is, therefore, scientifically and ethnographically one of the best-known of the Arctic lands. But in spite of this we encounter here several of those *lacunæ* in the knowledge of the globe which it is of great importance that we should fill up, and some of these it is now my intention to deal with.

The east coast of Greenland was visited in 1822 by the Englishman, Scoresby, jun., and in 1823 by Sabine and Clavering, in 1829-30 by the Dane, W A. Graah, as well as by the second German expedition under Koldewey in 1868-69, and also by some whalers. It is, however, in its greatest extent wholly unknown, a circumstance, which must be detrimental to the proper understanding of the history of the first Norse colonisation of Greenland, and of early voyages of conquest and discovery therefrom to the shores of America. Thus, until the east coast of Greenland is fully explored, one must continue to doubt the very forced explanation of the site of these colonies which is now predominant in the world of science. And it is, on the other hand, not worthy of the geographical discoveries of the 19th century, that a coast-line extending south to the latitude of Stockholm should be so utterly a *terra incognita.*

The interior of Greenland is even less known than the east coast, and here we encounter a purely scientific problem, whose great importance is apparent from the circumstance that the unestablished theory,—that the interior of the island is one continuous mass of ice,—forms one of the corner stones in glacial science, which again is closely connected with several of the fundamental principles of modern geology. If we except a trip on the inland ice of Greenland in 1751, in lat. 62° 31′ by the Danish merchant Lars Dalager, who penetrated nearly thirteen kilometres across a comparatively even plateau, and the unsuccessful attempt by Whymper in 1867, in lat. 69° 30′, where no progress was possible, in consequence of the difficult nature of the ground, only two serious attempts have been made to explore the interior of Greenland. The first of these was made by Dr. Berggren and myself between July 19 and 26, 1870, in lat. 68° 30′. Favoured by the most magnificent weather, we were able to penetrate nearly fifty kilometres across a country at the outset very difficult and rent by bottomless abysses, but which gradually improved in condition the further we advanced. We had on starting the company of two Eskimo, but they left us after two days' journey. As those who claimed to know the coast glaciers of Greenland had advised me not to waste time and labour on such a hopeless undertaking as that of penetrating over the inland ice, my outfit was very incomplete; we were in want of ropes, tent, suitable sledges, and on the Eskimo leaving us we could not even carry the utensils necessary for cooking. I could not therefore on that occasion get very far, but I certainly came to the conclusion, that I should have been able, with a couple of smart sailors or Arctic hunters and a suitable outfit, easily to have extended my wanderings to 200 or perhaps 300 kilometres. I may also mention here that in the month of June, 1873, I effected with Capt. Palander and nine sailors a journey of more than 190 kilometres over the inland ice of Spitzbergen, which journey was of special interest to me from the circumstance that I here learnt to know the character of inland ice before thaw sets in, as well as the difficulties which are at such a time attendant on journeys on the glaciers of the Polar regions. This experience, I believe, will be of great use to me during the journey now in course of preparation, as I shall have to cross portions of the inland ice which, on account of the altitude, will still be covered with snow at the time of my visit.

The second journey of research on the inland ice of Greenland was made between July 14 and August 4, 1878, in lat. 62° 40′, by the Danes, J. D. Jensen and A. Kornerup. This expedition was carefully equipped, but the country being much fissured, and the weather unfavourable, it did not reach much further inland than the Swedish of 1870.

None of these expeditions saw any limit to the ice desert from their farthest point, but to infer from this that ice covers the whole interior of Greenland appears to me to be entirely unjustifiable. On the contrary, the following reflections seem to demonstrate that it is a *physical impossibility* that the whole of the interior of this extensive continent can be covered with ice, under the climatic conditions which exists on the globe south of the 80th degree of latitude.

The ice masses of the glaciers are commonly termed "permanent," a denomination which was once taken so literally that certain *savants* asserted that the ice in course of time was transformed into mineral crystals, such as those which are so frequently found in the clefts of the Alps. We know now that this term is entirely erroneous. The glacier which seems century after century to fill the same valley is not only in constant, although imperceptible, motion, by the ice masses which slowly advance from higher to lower elevations, but is also subjected to a change in its form by the circumstance that the lower stratum melts away through contact with the mountain on which it rests; while the surface on one side wastes away by thawing in the warm season and evaporation in the cold, and on the other is added to by falling snow, which latter after a time changes from snow dust to granular snow, then to crystals of ice, and eventually to a compact homogeneous mass of ice. And, if the advancing glacier is "fed" by enormous ice-fields, or what I may term "ice-lakes," situated so high that snow always falls there copiously, it can penetrate far below the border of the perpetual snow, yea, even to parts where the snow-fall is far from sufficient to make up for the loss of melting and evaporation. It is therefore clear, that glaciers, or other constant ice-masses, cannot form in places where they cannot be "fed" by descending ice, or where the snow-fall is less than the quantity which appears and disappears yearly; a circumstance which, among others, explains why no glaciers exist in the vicinity of the north polar coasts of the new or old world.

With regard to Greenland it is not difficult to demonstrate that the above-described conditions for the formation of glaciers do not exist there, if the country does not rise gradually both from the eastern and the western shore to the centre, and thus be like a loaf of bread in shape, and with sides slowly and symetrically terminating in the ocean. Such a land-formation is, however, not found in any part of the orography of the known world, and one may therefore safely conclude that neither is it to be found in Greenland. In fact, the geological nature of Greenland, very similar to that of Scandinavia, seems to indicate a similar orographical formation, viz., a formation formed of mountainous ridges alternating with deep valleys and plains; while one may even assume that the culminating line of the land in Greenland runs, as in England and Sweden, and in both American continents, along the west coast.

The winds, therefore, which should produce snow in the interior, must, if coming from the Atlantic, have in the first instance, crossed the broad ice-belt generally encircling the east coast of Greenland, and then the mountains on the coast, some of which we know are very high, and, if coming from Davis Sound, the mountain ridge itself. In both cases the wind would assume the character of the "Föhn" wind, *i.e.*, it must, after passing the mountain-chains on the coasts, be *dry and comparatively warm*. The law of the "Föhn" is, as is generally known, dependent on the circumstances explained below.

If ACB indicates a mountain, and a wind so dry that no deposit of moisture takes place on the top passes from AC to B, the air will certainly be chilled in passing C, in consequence of the lower barometric pressure and consequent expansion of the air; but the same cause which produces the lowering of temperature when the wind ascends has also the effect of liberating its heat, and the air will become warmer as it descends from C to B. The compression and rise of temperature are in the latter case precisely analogous to the expansion and fall of temperature in the former, and the dry air, in passing

the mountain, has, therefore, on reaching B, not suffered any change whatever in temperature or quantity of moisture. Quite different will, however, be the result if the air ascending at A is saturated with moisture, as, for instance, air passing a great expanse of water. In that case the air will expand and become colder, just as it ascends from the water surface to the mountain top, but, at the same time, part of the moisture will be condensed on the top, whereby the latent heat of the hydrogen will be set free and a rise of temperature take place, and this will, to a certain extent, minimise the fall of temperature caused by the expansion of the air. The air will retain the heat thus set free, even after it has reached, in a dry state, the point B, and the air, originally moist, has, when it has passed the mountain, attained a higher degree of heat, but less moisture than at the moment of ascending. *It is in fact dry and warm.*

These causes are not only the reason of the dry warm "Föhn" winds in Switzerland, and the very remarkable circumstance that it is under winds from the snow-covered mountains that the snow disappears in Swedish Lappland, but they play also an important part in the climatic conditions of the whole globe. They are, for example, the cause of the difference in climate and flora of the two sides of the Andes, of the east and west coasts of Tierra del Fuega, and the eastern and western parts of Australia. They are the chief cause of the deserts which cover the interiors of Asia, Australia, the northern portion of Africa, and certain parts of America, while in Sweden they produce the constant western winds, and the consequent prolonged drought which invariably occurs in spring time in the central part of the country. The same laws of the temperature and moisture of the air must also prevail in Greenland. Here too the ocean winds must be moist, and this moisture is usually deposited in the form of snow on the mountains along the coast, whereas all those reaching the interior, whether from east, west, north, or south, must—if the orographical construction of the country is not entirely different from that of others on the globe—be dry and comparatively warm. And in consequence of this circumstance, the snow-falls in the interior of Greenland cannot be sufficient for maintaining a "perpetual" inland ice.

It cannot, however, be asserted that the country should here form a deserted, treeless tundra; one encounters in Siberia forests with giant trees under climatic conditions far more severe than those we may assume are to be found in the interior of Greenland. That the country should prove true to its name has besides been asserted by the celebrated botanist Hooker, from his studies of the flora of Greenland, and even the natives on the west coast themselves have a suspicion that such is the case, from the large herds of reindeer which from time to time are seen to migrate across the inland ice to the west coast.

It is most probable that the interior, if free from ice, is like a North European high plateau, with a flora far more copious than that of the coast.

But this I maintain, that whether the interior of Greenland is richly covered with forests, as the land round the frigid pole of Siberia, or is a treeless, ice-free tundra, or even a desert of perpetual ice, the solution of the problem of its real nature is so important, and of such consequence to science, that there could hardly, at the present moment, be conceived an object more worthy of an Arctic expedition than to ascertain the true conditions of the interior of this particular country.

Besides the object of penetrating to the interior of Greenland, the expedition will have several others in view, of which I may mention the principal:—

To Fix the Limit of the Drift-ice between Iceland and Cape Farewell, and to take Soundings and Dredgings in the Adjacent Seas

This part of the Atlantic has hardly ever been subjected to any other kind of examination than that made on laying the first cables. A knowledge of the same is, however, of great importance, both for completing the missing link in the hydrographic chain of the Ocean dividing Europe and America, and for the discovery of the causes of the change in the ice-conditions in the seas of the east coast which seem to have taken place since Greenland was first discovered. Without much waste of time the expedition may be occupied with these researches during the voyage from Iceland to the Greenland promontory, which will take place in a season when fine weather may be presumed to reign here, while they may, although with less probability of being favoured by this condition, be effected during the return journey.

The Collection of Fresh Specimens of the Flora of the Ice and Snow

Professor Wittrock is at present engaged in publishing a very interesting and important work on the microscopic flora, with several varieties, whose home is in the snow and ice-fields of the Alps and the Polar regions, for which materials have been brought home from the latter by the Swedish Arctic expeditions. My expedition will, during the coming visit to Greenland, be in a position to gather further materials for researches, which have already revealed to us the startling fact, that even snow and ice can form the bed for a flora regular in development, and of many varieties.

New Systematic Researches of the Strata which in Greenland contain Fossil Plants

From the previous Swedish Arctic expeditions, and through the publications in the *Journal* of the Swedish Academy of Sciences of articles by Prof. Oswald Heer, of Zurich, we know that students of science have received during the last twenty years from the sandstones and slates of North Europe an additional abundant material for determining the pre-historic climatic conditions of the earth ; as well as a knowledge of the certainly variable, but even in the last geological era copious, flora which during these ages existed in the present ice-covered regions of the pole. It is well known, that valuable materials for the study of this phenomenon has been collected in Greenland by English, Danish, and Swedish explorers ; but hitherto what has been brought home has been gathered under very difficult conditions, and generally by men who have not been students of the science in question, viz., palæontology. I hope to attach to my coming expedition one of the most celebrated students of this science, and if this be the case, the expedition will undoubtedly bring home novel and circumstantial details relating to this important chapter of the history of the earth. This task is so much easier to accomplish as the richest fields for the discovery of such fossil objects are situated just where it is my intention to invade the inland ice. During the period I shall be absent wandering on the ice, the other members will be occupied in this pursuit.

The Collection of New Data Connected with the Fall of Cosmic Dust

It is clearly demonstrated by the discovery of cobalt containing iron particles in fresh snow in Europe, and the carbon-dust, also containing cobalt-iron, which is found on the ice-fields north of Spitzbergen, as well as by the appearance of metallic iron in "krykonit," a remarkable dust which I brought home from the Greenland ice, that the fall of a small quantity of cosmic dust always takes place, regularly or periodically, most probably in every part of the globe. But that there is a greater variation in the nature of this fall than is generally assumed, is clearly indicated by the discoveries which were made in the *Vega* expedition, viz., of tiny yellow crystals in the snow on an ice-plateau near the Taimur peninsula. This certainly necessitates fresh researches into this phenomenon, in order to settle questions of great moment to geology and cosmology. It is very difficult to investigate this feature of cosmology, in consequence of the small quantities of dust which falls, and when it takes place in closely populated districts, covered by dwellings and factories, and where the ground is perhaps only clad in snow during a short period of the season. The Polar countries, on the other hand, are particularly suited for researches of this kind, both in consequence of the purity of the air and the absence of terrestrial dust, and by the ease with which the dark dust-particles are noticed on pure snow. The coming expedition will, while steaming along the ice-belts between Iceland and Greenland, and during my wanderings on the ice, be able to direct a great deal of attention to these captivating problems.

Should the conditions of the ice in Baffin's Bay be favourable, and the vessel, when in the vicinity of Disco Island, have sufficient coals for a journey northwards, or should it be possible to obtain a sufficient supply from the beds found in these parts, it would be highly desirable that the members remaining on board whilst I am away on the inland ice make an excursion along the west coast as far as Cape York.

There are, according to statements made to the Arctic explorers Ross and Sabine, lying here on a mountain, Savilik, *i.e.*, the iron mountain, lat. 76° 10′, a couple of large, round, solitary iron blocks, from which the natives obtain the little supply of iron which they require for hunting implements and domestic utensils. The metallic constituents of these blocks are, according to an analysis made of one of these utensils brought home, iron and a small percentage of nickel, and, to judge from the descriptions by the Eskimo, these blocks are of the same nature as those which I discovered in 1870 at Ovifak, on Disco Island. It is very strange that the remarkable descriptions by Ross and Sabine should not have been the object of investigation by the many Arctic explorers who have passed this spot. An opportunity is now at hand to ascertain the much-disputed nature and origin of these iron blocks, while a few days' sojourn on this part of the coast, which is so little known, ought to be of considerable scientific value, especially as the geological features here are similar to those on Disco Island,—strata bearing fossil plants.

This expedition will, as has been the case with previous Swedish Arctic expeditions, be accompanied by a specially selected scientific staff, whose members will individually contribute to make the expedition worthy of earlier achievements, and who will lose no opportunity of adding to our knowledge of the Polar regions. They will attempt to solve some of the many problems which await investigation in the far north. It is, however, impossible to detail at length the researches which may be undertaken, as these must depend on the nature of the special studies to which the members have devoted their time.

There is, however, one more object of research to be mentioned, which should not be lost sight of by any expeditions to Greenland, viz., to attempt to solve the question : Where were the former Norse colonies, Eriksfjord, Brattelid, Garda Cathedral, Herjolfsnæs, and others situated ? This question has already been answered by the most eminent students of the early history of Greenland, who maintain that the ancient Österbygd lies west of the southern part of the island, between Cape Farewell and lat. 61°, and the Vesterbygd, a little north of the west coast. But it appears to me that, if the Icelandic Sagas are examined carefully, and without any preconceived opinion, it will become apparent that former investigators of this problem have been led astray, and that the true Eriksfjord, with its cathedral and numerous parishes, has never been found, but must be sought for on the inaccessible east coast north of Cape Farewell.

With the experience I have of the conditions of the ice in other parts of the Polar seas, I believe that this coast may be reached, without much difficulty, by sailing in the autumn from the south in the ice-free channel, which I have every reason to believe forms along this coast. This journey will, however, not be begun until the commencement of September, and this circumstance will exactly fall in with the plan in view, viz., on returning from the inland ice to attempt this voyage of discovery.

With the premises I have here detailed before me, I now have to suggest the following plan for the journey of the expedition :—

The expedition should leave Sweden near the end of May in a suitable, but not too large, steamer built of Swedish iron and constructed with water-tight compartments. And although the journey is intended for the summer months only, it should be provisioned for one year, and be provided with the necessary winter outfit, with the scientific instruments desirable, and accoutrements which would be of service for the journey on the inland ice. The steamer should be navigated by a man accustomed to sailing in the Arctic seas, whilst there ought also to be on board as ice-master a skipper who has hunted in the Arctic. The scientific staff should, besides the commander, consist of four persons, the doctor included.

From Sweden the steamer should steam to a port in Scotland, where more coals are to be taken on board, and the journey continued to Reikiavik in Iceland. There a

stay will be made for a few days, when the edge of the ice in the west will be made for, which will be followed southwards, but no attempt should then be made to penetrate the pack. Only in case there should be found an open lead, which is not expected, this will be entered. The probability of this is, however, very small. After having passed Cape Farewell, the vessel will call at Ivigtuk, where again it will be coaled from the depots which have been laid up here for the use of the steamer. The course will then be shaped for the west coast of Greenland, probably calling at Egedesminde, to the Auleitsivik Fjord, from the bottom of which the ice-journey will be commenced.

This latter will occupy, it is estimated, thirty to forty days, and should be finished thus by the middle of August next. During this time the vessel will steam through the Waigatt to Omenak, where the many deposits of fossil plants will be visited. If the ice and coals should permit, the vessel will steam northwards, if possible as far as Cape York, whereby an excellent opportunity will be offered for geological, mineralogical, botanical, and zoological studies. In the middle of August the vessel will again be due in the Auleitsivik Fjord, and taking the members of the ice-expedition on board, will steam south to Ivigtuk, where a few days' stay will be made for coaling, etc. From here the vessel will steam round Cape Farewell, along the east coast in the open channel, which I expect to find there at that season; and my intention is then, with due reference to the old geographical descriptions of the Icelandic Sagas, carefully to search the fjords which may be accessible.

At the end of September the return journey will be commenced outside the belt of pack-ice to Reikiavik and home.

The distances the vessel will cover are, in round figures, these:—

			Nautical Miles.
GOTHENBURG ...	to	THURSO	500
THURSO	„	REIKIAVIK	700
REIKIAVIK	„	IVIGTUK	870
IVIGTUK	„	AULEITSIVIK FJORD	540
AULEITSIVIK FJORD	„	OMENAK	330
OMENAK	„	CAPE YORK	400

A. E. NORDENSKIÖLD

NOTES

THE Fifty-third Annual Meeting of the British Association will commence on Wednesday, September 19, 1883, at Southport. The President Elect is Arthur Cayley, LL.D., F.R.S., Sadlerian Professor of Mathematics in the University of Cambridge. The Vice-Presidents Elect are the Right Hon. the Earl of Derby, the Right Hon. the Earl of Crawford and Balcarres, the Right Hon. the Earl of Lathom, Prof. J. G. Greenwood, LL.D., Prof. H. E. Roscoe, LL.D., F.R.S. The General Treasurer is Prof. A. W. Williamson, Ph.D., F.R.S., University College, London. The General Secretaries are Capt. Douglas Galton, C.B., F.R.S., and A. G. Vernon Harcourt, F.R.S. The Secretary is Prof. T. G. Bonney, F.R.S. The Local Secretaries are J. H. Ellis, Dr. Vernon, T. W. Willis; and the Local Treasurer the Mayor of Southport. The Sections are the following:—A.—Mathematical and Physical Science.—President: Prof. Henrici, F.R.S. Vice-Presidents: Prof. Balfour Stewart, M.A., LL.D., F.R.S., F.R.A.S.; Prof. Stokes, M.A., D.C.L., LL.D., Sec.R.S. Secretaries: W. M. Hicks, M.A.; Prof. O. J. Lodge, D.Sc.; D. McAlister, M.A., M.B., D.Sc. (Recorder); Prof. Rowe, M.A., B.Sc. B.—Chemical Science.—President: J. H. Gladstone, Ph.D., F.R.S. Vice-Presidents: Hugo Müller, Ph.D., F.R.S., For. Sec. C.S.; Prof. T. E. Thorpe, Ph.D., F.R.S., F.C.S. Secretaries: Prof. P. Phillips Bedson, D.Sc., F.C.S. (Recorder); H. B. Dixon, M.A., F.C.S.; H. Foster Morley, M.A., B.Sc., F.C.S. C.—Geology.—President: Prof. W. C. Williamson, F.R.S. Vice-Presidents: Prof. W. Boyd Dawkins, M.A., F.R.S., F.S.A., F.G.S.; J. W. Hulke, F.R.S., Pres.G.S. Secretaries: R. Betley, F.G.S.; C. E. de Rance, F.G.S.; W. Topley, F.G.S. (Recorder); W. Whitaker, B.A., F.G.S. D.—Biology.—President: Prof. E. Ray Lankester, F.R.S. Vice-Presidents: Prof. Gamgee, M.D., F.R.S.; W. Pengelly, F.R.S., F.G.S.; Prof. Schäfer, F.R.S.; W. T. Thiselton Dyer, M.A., B.Sc., F.R.S., F.L.S. Secretaries: G. J. Haslam, M.D.; W. Heape; Prof. A. M. Marshall, M.A., M.D., D.Sc.; Howard Saunders, F.L.S., F.Z.S. (Recorder); G. A. Woods. Department of Anthropology: W. Pengelly, F.R.S., F.G.S. (Vice-President), will preside. Secretaries: G. W. Bloxam, M.A., F.L.S. (Recorder); Walter Hurst. E.—Geography.—President: Lieut.-Col. H. H. Godwin-Austen, F.R.S., F.G.S., F.R.G.S. Vice-Presidents: Sir Rawson W. Rawson, K.C.M.G., C.B., F.R.G.S.; The Rev. Canon Tristram, M.A., LL.D., F.R.S., F.L.S. Secretaries: John Coles, F.R.A.S., F.R.G.S.; E. G. Ravenstein, F.R.G.S.; E. C. Rye, F.Z.S. (Recorder). F.—Economic Science and Statistics.—President: R. H. Inglis Palgrave, F.R.S., F.S.S. Vice-Presidents: Prof. R. Adamson, M.A., LL.D.,; J. Heywood, F.R.S., F.G.S., F.S.A., F.S.S. Secretaries: Prof. H. S. Foxwell, M.A., F.S.S.; J. N. Keynes, M.A., B.Sc.; Constantine Molloy (Recorder). G.—Mechanical Science.—President: James Brunlees, F.R.S.E., F.G.S., Pres. I.C.E. Vice-Presidents: W. H. Barlow, F.R.S., M.I.C.E.; Prof. Osborne Reynolds, M.A., F.R.S. Secretaries: A. T. Atchison, M.A., C.E.; Edward Rigg, M.A.; H. T. Wood, B.A. (Recorder). The first general meeting will be held on Wednesday, September 19, at 8 p.m., when Sir C. W. Siemens, D.C.L., LL.D., F.R.S., F.C.S., M.I.C.E., will resign the Chair, and Prof. Cayley, M.A., LL.D., F.R.S., V.P.R.A.S., President Elect, will assume the Presidency, and deliver an address. On Thursday evening, September 20, at 8 p.m., there will be a soirée; on Friday evening, September 21, at 8.30 p.m., a Discourse on Recent Researches on the Distance of the Sun, by Prof. R. S. Ball, LL.D., F.R.S., Astronomer Royal for Ireland; on Monday evening, September 24, at 8.30 p.m., a Discourse on Galvani and Animal Electricity, by Prof. J. G. McKendrick, M.D., LL.D., F.R.S.E., Professor of Physiology in the University of Glasgow, and in the Royal Institution of Great Britain; on Tuesday evening, September 25, at 8 p.m., a soirée; on Wednesday, September 26, the concluding general meeting will be held at 2.30 p.m.

THE number of members of the British Association who have announced their intention of being present at the proposed meeting at Montreal in 1884, continues to increase, and now exceeds 410. It is stated that Lord Rayleigh has accepted the presidency for the Canadian meeting.

PROF. HUXLEY has been elected a Foreign Member of the U.S. National Academy.

FROM a list of seven names proposed by the Incorporated Societies throughout the colony, the following gentlemen have been elected honorary members of the New Zealand Institute:—Sir William Thomson, F.R.S., Professor of Natural Philosophy, University of Glasgow; Dr. W. B. Carpenter, F.R.S., C.B., of London; and Mr. R. L. J. Ellery, F.R.S., Government Astronomer at Melbourne.

PROF. TYNDALL has resigned his position as Scientific Adviser to the Board of Trade and the Lighthouse Boards.

WE are glad to see the ample recognition of music during the present week by the conferring of knighthood on Messrs. Sullivan, Grove, and Macfarren in connection with the New College of Music. Let us hope that the science of music as well as the art will receive due cultivation in the new institution; with Sir George Grove's well-known wide sympathies, however, this may be taken for granted.

THE first eclipse news comes from Lima, under date May 7; the sky was overcast on the 6th, preventing any observation of the eclipse. It is to be hoped the conditions were more favourable on the other side of the Pacific.

THE proposal to open museums and galleries on Sundays was, we regret to say, lost in favour of Lord Shaftesbury's compromise to keep them open in the evenings on two or three nights a week, which does not in the least meet the case of the working classes.

M. RICHI, Professor in the Paris School of Medicine, was nominated Member of the Academy of Sciences on Monday, to fill the place vacated by the death of M. Sedillot. Another election will soon take place in the same section of medicine and surgery.

MR. A. H. KEANE proposes, as soon as he obtains a sufficient number of subscribers, to begin the publication of "A Classification of the Races of Mankind," copious materials for which have now been collected. It will form two large octavo volumes of about six hundred pages each. The publication will probably extend over two years, and at least five hundred names will be required to justify the undertaking, although subscribers to the first need not be committed to purchase the second part also. Subscriptions will be received by Mr. Edward Stanford, 55, Charing Cross, or by Mr. Keane, at University College, London. With regard to the scope and contents of the work, it may be briefly stated that its aim will be to place in the hands of the ethnological student a comprehensive treatise on the races of mankind harmonising with the present state of anthropological inquiry. In the general introduction such broad questions will be dealt with as the evolution of man, the antiquity and specific unity of the species, the present varieties of mankind, the physical and moral criteria of race, the fundamental human types, their evolution and dispersion, the peopling of the continents, the origin of articulate speech, the morphological orders and families of speech, the problem of specific linguistic diversity within the same ethnical group. The great physical divisions of the human family will then be dealt with *seriatim*, and here the same arrangement will be adhered to as that observed in Mr. Keane's ethnological appendices to the Stanford Geographical Series. Each of the main sections of mankind will thus be treated in three separate parts. In the first the salient physical and moral characteristics of the type will be discussed. The second will be occupied with the several main branches of each, and here the proper work of classification will be carried out in detail. Lastly, the third part will consist of an alphabetical index, comprising, as far as Mr. Keane has been able to collect them, all the known races, tribes, and languages of each main division, briefly described, and with copious references to authorities. The price of the work will be 16*s*. per volume to subscribers.

THE Dutch Polar ship *Willem Barents* started again on Saturday from Amsterdam, under the command of Capt. Dalen, for the Arctic regions, to try to discover the Dutch expedition in the *Varna*. Mr. Leigh Smith was present, as well as Mr. Clements Markham, to wish the *Willem Barents* "God speed." Sir Allen Young went over to express his best wishes to the crew of the Dutch ship. Mr. Leigh Smith presented to Capt. Dalen two magnificent silver cups, bearing as an inscription the following words, taken from his journal: "August 3d, 1882; Matotchkin Skarr, Nova Zembla, 10.0 a.m. A sail! A sail! The *Willem Barents*."

THE Danish expedition to Greenland left Copenhagen on Wednesday last week. Its purpose is to explore the east coast of Greenland; and it will probably be away for two years.

THE Sanitary Institute of Great Britain held its annual meeting at 9, Conduit Street, on Monday, Prof. de Chaumont, M.D., F.R.S., in the chair. A report was presented by the Council on the progress of the Institute and on the work at the Congress and Exhibition held by the Institute at Newcastle last autumn. The Chairman gave an address, and the officers for the ensuing year were elected, the President being the Duke of Northumberland, and the trustees Sir John Lubbock, Bart., Mr. Thomas Salt, M.P., and Dr. B. W. Richardson.

THE Prince of Wales opened the Indian Institute at Oxford last week.

Two divisions of the pupils of Sainte Barbe left Paris on May 4, one for London, and the other for Germany, where they are to stay for three months under the care of their professors, for the purpose of obtaining practical knowledge of the English and German languages.

REAR-ADMIRAL MOUCHEZ is leaving Paris within a few days. for Algiers, where he will establish an observatory at Coubar. This site is deemed excellent for observations.

WE have received the announcement of an aëronautical exhibition, to be held from June 5 to 15 at the Trocadéro, Paris.

THE Whit Monday excursion of the Geologists' Association will be to Hunstanton; and on May 26 to Ealing and Perivale.

THE well-known explorer of the fauna and depths of Lake Baikal, Dr. Godlevsky, who is now in Kamchatka, writes to the East Siberian Geographical Society, from Petropavlovsk, that last summer he transported on board of a steamer of the company Hutchinson, Kool, and Co., fifteen reindeer, of which eleven were males and four females, to Behring's Island. The reindeer were brought to Petropavlovsk from the west coast of Kamchatka in a flock of 150, and the journey took no less than two and a half months, as it was made across the mountains in order to avoid the mosquitos of the valleys. On board the steamer the reindeer were fed with stems and leaves of birch, as also with fresh grass kept in casks with water. Afterwards they also ate hay, and even bread, contrary to the affirmations of the Lamuts, who said that reindeer never eat grass that has been gathered by man. The steamer-journey was made in two days, and the reindeer arrived in good health.

WHEN leaving Yakutsk for the mouth of the Lena, M. Yurgens distributed a number of thermometers to different persons. The *Izvestia* of the East Siberian branch of the Russian Geographical Society now publish the meteorological observations made at Markha, in the province of Yakutsk, district of Viluisk, during the four months of August to November last, by an exile, M. Pavloff. The rapidity with which the cold weather sets in in those latitudes is very interesting. In August the thermometer rose at 1 p.m. as high as 31° Cels. (on August 1), and reached 14° to 20° during the second half of the month. The first frosts were experienced (at 7 a.m.) in September, and already in the first days of October the thermometer sunk (at 7 a.m.) as low as −11° to −20°; and as low as −30° to −35°, and even −39°, during the second half of October. In November it never rose above −32°, and usually stood at seven o'clock in the morning between −39° to −50°; even at 1 p.m. it did not rise, during the whole of the month, above −31°, and usually stood between −33° to −42°, occasionally sinking to −48° and −50° Cels. The underground thermometer, placed at 1·5 feet below the surface, decreased with a remarkable regularity, from 6°·8, on August 1, to 0° on September 12 to 17, to −2° on October 17, to −6° on November 1, and to −17° on November 30.

A CORRESPONDENT points out that at the conclusion of the address by Prof. Du Bois Reymond on "Darwin and Copernicus"

(NATURE, vol. xxvii. p. 558), it is stated that "Charles Darwin lies buried in Westminster Abbey among his peers, Newton and Faraday," whereas Faraday lies buried in the unconsecrated ground at Highgate, and not in Westminster Abbey.

ACCORDING to information received from the United States, some wonderful telephonic feats are being performed by the new Hopkins arrangement. It seems that two operators—one at New York and the other at Chicago, 1100 miles apart—had no difficulty, not only in talking to each other, but in listening to music, and fulfilling other tests. Even the noise produced by changing the diaphragm at New York was distinctly heard at Chicago.

THE Institute of Agriculture at South Kensington completed the work of its first session on Monday, the 7th inst. The Earl of Aberdeen, Chairman of the Council, presided, and distributed Certificates of Merit to 103 students. Notice was given of largely increased opportunities for study during the next session, which commences on October 1.

THE National Eisteddfod of Wales is not without an appreciation for science; in its list of prizes three are offered in natural history, one of 10*l.* 10*s.*, open to all comers, being for the best model of a skeleton of Plesiosaurus. Length of model not to be less than 4 feet, each bone to be separately modelled and removable except those of the cranium and phalanges. Competitors may obtain information and aid on application to Prof. Sollas, University College, Bristol.

THE statue of Leibnitz, intended for Leipzig, has recently been cast by Herr Lenz of Nuremberg, after the model of Prof. Hähnel, and the casting was a complete success. Herr von Miller of Munich has received an order for a colossal statue of Columbus, destined for the city of Cincinnati.

WE hear of a curious incident occurring on Siemens' electric railway at Portrush. Owing to the fact that as yet only part of the line is furnished with electric conductors, a steam-engine is still used as well as the electric locomotive. A few days ago the steam-engine while drawing its load along the line came to a stand through the accident of bursting a boiler-tube. News of the disaster having been sent to the terminus, the stationary dynamo-electric machine which supplies the current was set into action and the electric locomotive despatched to the rescue. It returned an hour later bringing the disabled steam-engine behind it.

M. FOUSSEREAU has lately measured the electric resistance of glass by charging a condenser from a known battery through a given thickness of the glass, and observing the time required to raise the potential of the condenser to a given degree. Bohemian glass was found to be from 5 to 20 times as good a conductor as ordinary glass, whilst flint glass was from 1000 to 1500 times as good an isolator. M. Foussereau found also that annealing the glass increased its resistance in some cases elevenfold.

AT a special general meeting of the Entomological Society of London, held on May 2, Prof. J. O. Westwood, M.A., F.L.S., was elected titular life-president of the Society by acclamation, the occasion being the fiftieth anniversary of the meeting at which steps were taken to establish the Society.

OUR readers will remember the accounts given last autumn of the electric launch which was successfully tried upon the Thames last autumn. This little craft has been running at intervals all the winter and is still in good trim. We learn that the Electrical Power Storage Company have three other electric boats on hand; one of them for the British Government.

A STRONG earthquake shock, with an undulating motion, was felt on the morning of the 8th, at Biancavilla, Sicily.

REPEATED shocks of earthquake were observed in various places in the Spanish province of Valencia on April 14 and 16. Some were of 2 to 3 seconds' duration.

LITERARY piracy would appear to be one of the institutions of the West to which young Japan has taken rather kindly. According to the *Japan Gazette*, the practice of pirating patents, stamps, and labels, in order to palm off spurious imitations for the genuine article, has been carried on for years, and the evil is extending in every direction. Recently a native company called the "Tokio Bookselling Company" was established in the capital, and its chief business appears to be the pirating of English and American schoolbooks. Todhunter's well known Elementary Algebra, Euclid, and other mathematical books have already been reproduced, as well as several American books, Mill's "Liberty," and other volumes. These are published as much like the originals in size, covers, &c., as possible. The Company affixes its imprint to the titlepages, but offers no explanation as to the publication in Japan, and indeed they have no hesitation in reprinting (probably through ignorance) the foreign publishers' notices, such as "Entered according to the Act of Congress in the year, &c., &c." An examination of the reprint of Todhunter's Algebra shows letters upside down, wrong fount letters, letters misplaced, and words improperly spelt, testifying to the slovenly way in which the books have been printed. There is said to be scarcely a page in the book which does not contain one or more errors in orthography, and the mathematical formulæ, which always require such care at the printers' hands, must be in a bad state when ordinary words are so neglected. The direct damage to foreign authors, patentees, and manufacturers by these petty thefts cannot be very great; the real injury is to the Japanese people themselves. Among well-known English labels, that of Bass and Co. has for many years been the subject of innumerable depredations. The striking red diamond on white ground lends itself easily to imitation, while to people who cannot read English letters at all, any strange marks below the diamond are sufficient to represent the names of the eminent brewers. Large quantities of some decoction brewed in Tokio are thus passed off in the interior as Burton ale. A patent law is wanted in Japan, not so much for the protection of foreign inventors (the Japanese Government is far too advanced to think of such a minor consideration as this) as for the protection of the pockets and stomachs of the Japanese themselves.

WE are pleased to observe that the *Chrysanthemum* magazine of Yokohama has commenced its third year with a more ambitious flight. The size is considerably enlarged, as is also the number of subjects treated and the staff of writers. The theological discussions appear to be wholly eliminated, to the increase of the general interest of the journal. The first two numbers of the new issue are now before us, and exhibit no lack of mental power. The literary treatment of the varied subjects discussed is in most cases excellent. The most powerful recruit appears to be Capt. Brinkley, R.A., who writes a serial tale with Japanese characters and scenes, as well as a serial history of Japanese keramics, which deserves more attention in this country than it is likely to get. Higher education in Japan is elaborately treated by Dr. Groth, while Capt. Blakiston, probably the best authority on the ornithology of Japan, writes on that subject. Mr. A. H. Cole is responsible for a popular paper on the Darwinian theory, and Prof. Summers describes some ancient caves near Osaka. These are but a few of the papers in the first numbers of the new magazine, the editor of which appears determined at least to deserve success. We may, however, draw his attention to one defect, surely a very grave one in a scholarly publication such as this, viz. the notices of current literature, which so far have been quite unequal to the other departments.

THE additions to the Zoological Society's Gardens during the past week include a Macaque Monkey (*Macacus cynomolgus*) from India, presented by Mrs. Florence A. Hill ; a Common Rhea (*Rhea americana*) from Uruguay, presented by Mr. F. R. S. Balfour ; a Common Kestrel (*Tinnunculus alaudarius*), British, presented by Mr A. Lidbury ; a Wood Owl (*Syrnium aluco*), British, presented by Mrs. W. Duncan ; two Horned Lizards (*Phrynosoma cornutum*) from Texas, presented by Mr. John G. Witte ; two Marbled Newts (*Triton marmorata*) from France, presented by Mr. G. H. King ; two Viverrine Cats (*Felis viverrina*), an Indian Otter (*Lutra nair*), an Indian Darter (*Plotus melanogaster*), a Hamilton's Terrapin (*Clemmys hamiltoni*), three Indian Gazelles (*Gazella bennetti*) from India, deposited ; two Natterjack Toads (*Bufo calamita*), four Marbled Newts (*Triton marmorata*), four Short-nosed Sea Horses (*Hippocampus antiquorum*), from France, purchased ; a Black Wolf (*Canis niger*) from India, received in exchange ; an Eland (*Oreas canna* ♀), born in the Gardens.

OUR ASTRONOMICAL COLUMN

D'ARREST'S COMET.—Although M. Leveau's elements of this comet for the approaching return to perihelion were communicated to the Academy of Sciences of Paris on January 22, they were unaccompanied by predicted places, and it would appear that the ephemeris has had only a very limited circulation, being confined, if we are rightly informed, to those observers who are in possession of the larger instruments. Hence comparatively few persons may have become acquainted with the circumstances under which this return of the comet to perihelion takes place, and it may not be without interest if we briefly examine the conditions as compared with those of former appearances.

Assuming as usual the intensity of the comet's light (I) to be represented by the reciprocal of the product of the squares of the distances from the earth and sun, we find the following values :—

		I.
1851.	Last observation at Berlin, Oct. 6	0·590
1858.	Last observation at the Cape, Jan. 18 ...	0·151
1870.	Last observation at Athens, Dec. 20 ...	0.154
1877.	Last observation at Athens, Sept. 10 ...	0·127

The greatest distance from the earth at any of these dates was 1·93 on January 18, 1858.

M. Leveau's elements for the approaching return apply to 1883, June 12·0 M.T. at Paris ; neglecting, of course, the small effect of perturbation in the interval, the perihelion passage is found to take place 1884, January 13·5765 M.T. at Greenwich. The coordinate constants for apparent eqt inox of 1883, May 1, are :—

$$x = r\,.\,[9{\cdot}99502] \sin (v + 50^\circ\ 13'{\cdot}7).$$
$$y = r\,.\,[9{\cdot}99308] \sin (v + 321\ 48{\cdot}0).$$
$$z = r\,.\,[9{\cdot}36631] \sin (v + 280\ 38{\cdot}1).$$

Hence we have the following approximate positions and distances of the comet, with the corresponding values of the theoretical intensity of light, taking dates near the time of new moon :—

12h. G.M.T.	R.A. h. m.	Decl. ° ′	Distance from Earth.	Distance from Sun.	I.
June 6 ...	13 9·6 ...	-12 48 ...	2·037 ...	2·615 ...	0·035
July 4 ...	13 13·7 ...	+10 12 ...	2·185 ...	2·415 ...	0·036
Aug. 2 ...	13 37·0 ...	+ 5 42 ...	2·321 ...	2·204 ...	0·038
Nov. 29 ...	17 58·6 ...	−16 55 ...	2·281 ...	1·427 ...	0·094
Dec. 29 ...	19 45·7 ...	−18 8 ...	2·244 ...	1·338 ...	0·111

Whence it will be seen that even when most favourably circumstanced, towards the end of the year, the intensity of light will be less than the lowest value at which the comet has hitherto been observed, viz. 0·127. On November 29 the comet sets about 2h. 8m. after the sun. It was missed at the return in 1864, and the chances of observation at its present visit are by no means encouraging.

Mr. Common informs us that he has made a thorough search or the comet with his large reflector, but without success up to May 7. He remarks that the number of faint nebulæ about its track is surprising.

The orbit of this comet almost intersects that of the lost comet of De Vico, 1844 ; in heliocentric longitude 339°·37′, with the elements of 1851, the distance between the orbits was only 0·0055 or 507,000 miles.

TEMPEL'S COMET, 1873 II.—The corrected elements of this body by M. Schulhof, from observations at its last appearance in 1878, indicate that, neglecting perturbations, it may be again in perihelion about November 20. The positions calculated on this assumption show that the comet will be very unfavourably placed for observation, and it may escape detection at this return.

RULES AND REGULATIONS FOR THE PREVENTION OF FIRE RISKS ARISING FROM ELECTRIC LIGHTING[1]

THESE rules and regulations are drawn up for the reduction to a minimum, in the case of electric lighting, of those risks of fire which are inherent in every system of artificial illumination, and also for the guidance and instruction of those who have, or who contemplate having, electric lighting apparatus installed on their premises.

The difficulties that beset the electrical engineer are chiefly internal and invisible, and they can only be effectually guarded against by "testing," or probing with electric currents. They depend chiefly on leakage, undue resistance in the conductor, and bad joints, which lead to waste of energy and the dangerous production of heat. These defects can only be detected by measuring, by means of special apparatus, the currents that are either ordinarily or for the purpose of testing, passed through the circuit. Should wires become perceptibly warmed by the ordinary current, it is an indication that they are too small for the work they have to do, and that they should be replaced by larger wires. Bare or exposed conductors should always be within visual inspection and as far out of reach as possible, since the accidental falling on to, or the thoughtless placing of other conducting bodies upon such conductors, would lead to "short circuiting," and the consequent sudden generation of heat due to an increased current in conductors not adapted to carry it with safety.

The necessity cannot be too strongly urged for guarding against the presence of moisture and the use of "earth" as part of the circuit. Moisture leads to loss of current and to the destruction of the conductor by electrolytic corrosion, and the injudicious use of "earth" as a part of the circuit tends to magnify every other source of difficulty and danger.

The chief dangers of every new application of electricity arise from ignorance and in:xperience on the part of those who supply and fit up the requisite plant.

The greatest element of safety is therefore the employment of skilled and experienced electricians to supervise the work.

I. THE DYNAMO MACHINE

1. The dynamo machine should be fixed in a dry place.
2. It should not be exposed to dust or flyings.
3. It should be kept perfectly clean and its bearings well oiled.
4. The insulation of its coils and conductors should be practically perfect.
5. All conductors in the dynamo room should be firmly supported, well insulated, conveniently arranged for inspection, and marked or numbered.

II. THE WIRES

6. Every switch or commutator used for turning the current on or off should be constructed so that when it is moved and left it cannot permit of a permanent arc or of heating.

7 Every part of the circuit should be so determined, that the gauge of wire to be used is properly proportioned to the currents it will have to carry, and all junctions with a smaller conductor should be fitted with a suitable safety fuse or protector, so that

[1] Recommended by the Council of the Society of Telegraph Engineers and of Electricians in accordance with the Report of the Committee appointed by them on May 11, 1882, to consider the subject. Members of the Committee:—Prof. W. G. Adams, F.R.S., Sir Charles T Bright, T. Russell Crampton, R. E. Crompton, W. Crookes, F.R.S., Warren De La Rue, D.C L., F.R.S., Prof. G C Foster, F.R.S., Edward Graves, J. E. H. Gord)n, Dr. J. Hopkinson, F.R.S., Prof. D. E. Hughes, F.R.S., W. H. Preece, F.R.S., Alexander Siemens, C. E. Spagnoletti, James N. Shoolbred, Augustus Stroh, Sir William Thomson, F.R.S., Lieut.-Col. C. E. Webber, R.E.

no portion of the conductor should ever be allowed to attain a temperature exceeding 150° F.

8. Under ordinary circumstances complete metallic circuits should be used; the employment of gas or water pipes as conductors for the purpose of completing the circuit should not in any case be allowed.

9. Bare wires passing over the tops of houses should never be less than seven feet clear of any part of the roof, and all wires crossing thoroughfares should invariably be high enough to allow fire-escapes to pass under them.

10. It is most essential that joints should be electrically and mechanically perfect and united by solder.

11. The position of wires when underground should be clearly indicated, and they should be laid down so as to be easily inspected and repaired.

12. All wires used for indoor purposes should be efficiently insulated, either by being covered throughout with some insulating medium, or, if bare, by resting on insulated supports.

13. When these wires pass through roofs, floors, walls, or partitions, or where they cross or are liable to touch metallic masses, like iron girders or pipes, they should be thoroughly protected by suitable additional covering; and where they are liable to abrasion from any cause, or to the depredations of rats or mice, they should be efficiently incased in some hard material.

14. Where indoor wires are put out of sight, as beneath flooring, they should be thoroughly protected from mechanical injury, and their position should be indicated.

N.B.—The value of frequently testing the apparatus and circuits cannot be too strongly urged. The escape of electricity cannot be detected by the sense of smell, as can gas, but it can be detected by apparatus far more certain and delicate. Leakage not only means waste, but in the presence of moisture it means destruction of the conductor and its insulating covering, by electric action.

III. Lamps

15. Arc lamps should always be guarded by proper lanterns to prevent danger from falling incandescent pieces of carbon, and from ascending sparks. Their globes should be protected with wire netting.

16. The lanterns, and all parts which are to be handled, should be insulated from the circuit.

IV. Danger to Person

17. Where bare wire out of doors rests on insulating supports, it should be coated with insulating material, such as india-rubber tape or tube, for at least two feet on each side of the support.

18. To secure persons from danger inside buildings, it is essential so to arrange and protect the conductors and fittings that no one can be exposed to the shocks of alternating currents of a mean electromotive force exceeding 100 volts, or to continuous currents of 200 volts.

19. If the difference of potential within any house exceeds 200 volts, the house should be provided with a "switch," so arranged that the supply of electricity can be at once cut off.

UNIVERSITY AND EDUCATIONAL INTELLIGENCE

Cambridge.—Dr. Humphry has formally resigned the Professorship of Anatomy, after having taught anatomy in the University for thirty-six years, at first as assistant to Prof. Clark, and since 1866 as Professor. The Electoral Board for the Professorship consists of Professors Huxley, Allen, Thompson, Flower, Paget, Newton, and Liveing, Dr. Michael Foster, and Mr. J. W. Clark.

The Honorary Degree of LL.D. will be conferred in June upon Count Menabrea, formerly Italian Ambassador to England, Prof. Pasteur, Prof. Michaelis (Strasburg), Sir A. Grant, Bart., Principal of Edinburgh University, Sir John Lubbock, Bart., Sir J. A. G. Ouseley, Bart., Professor of Music at Oxford, Sir Richard Temple, Bart., Lieut.-Gen. Walker, Surveyor-General of India, Mr. Matthew Arnold, Prof. W. W. Goodwin (Harvard, U.S.), Mr. Reginald S. Poole (British Museum), Prof. H. E. Roscoe (Owens College), and Mr. G. F. Watts, R.A.

The Graces creating a Professorship of Surgery without stipend, and authorising the immediate appointment of a Professor of Physiology, are to be voted on to-day (Thursday).

The City and Guilds of London Institute has decided to make a grant of 300*l*. a year for five years, for the purpose of supporting a Chair of Mechanical Engineering in connection with Firth College, Sheffield.

SOCIETIES AND ACADEMIES

London

Royal Society, March 15.—"Atmospheric Absorption in the Infra-Red of the Solar Spectrum." By Capt. Abney, R.E., F.R.S., and Lieut.-Col. Festing, R.E.

Any investigations on the subject of atmospheric absorption are of such importance in the study of meteorology, that we have deemed it advisable to present a preliminary notice of certain results obtained by us, without waiting to present a more detailed account which will be communicated at a future date. From 1874, when one of us commenced photographing the spectrum in the above region, till more than a year ago, the extremely various manners in which the absorption took place caused considerable perplexity as to their origin, and it was only after we had completed our paper on the absorption of certain liquids [1] that a clue to the phenomena was apparently found. Since that time we have carefully watched the spectrum in relation to atmospheric moisture, and we think that more than a year's observations in London, when taken in connection with a month's work at an altitude of 8500 feet on the Riffel, justify the conclusions we now lay before the Society.

A study of the map of the infra-red region of the solar spectrum,[2] and more especially a new and much more complete one, which is being prepared for presentation to the Royal Society by one of us, shows that the spectrum in this part is traversed by absorption lines of various intensities. Besides these linear absorptions, photographs taken on days of different atmospheric condition show banded absorptions superposed over them. These latter are step by step absorptions increasing in intensity as they approach the limit of the spectrum at the least refrangible end. In the annexed diagram [3] Fig. 4 shows the general appearance of this region up to λ 10,000 on a fairly dry day: the banded absorption is small, taking place principally between λ 9420 and λ 9800: a trace of absorption is also visible between λ 8330 and λ 9420. On a cold day, with a north-easterly wind blowing, and also at a high altitude on a dry day, these absorptions nearly if not quite disappear. If we examine photographs taken when the air is nearly saturated with moisture (in some form or another) we have a spectrum like Fig. 1. Except with very prolonged exposure no trace of a spectrum below λ 8330 can be photographed. Fig. 2 shows the absorption-bands, where there is a difference of about 3° between the wet and dry bulbs, the latter standing at about 50°. It will be noticed that the spectrum extends to the limit of about λ 9420, when total absorption steps in and blocks out the rest of the spectrum. Fig. 3 shows the spectrum where the difference between the wet and the dry bulbs is about 6°. Figs. 5 and 6 show the absorption of thicknesses of 1 foot and 3 inches of water respectively, where the source of light gives a continuous spectrum. With ½-inch of water all absorption-bands except that commencing at λ 9420 are absent. It will be seen that there is an accurate coincidence between these "water-bands" and the absorption-bands seen in the solar spectrum, and hence we cannot but assume that there is a connection one with the other. In fact, on a dry day it is only necessary to place varying thicknesses of water before the slit of the spectroscope and to photograph the solar spectrum through them, in order to reproduce the phenomena observed on days in which there is more or less moisture present in the atmosphere. It is quite easy to deduce the moisture present in atmosphere at certain temperatures by a study of the photographs. There does appear a difference, however, in the intensity of the banded absorptions in hot weather and in cold about up to 50°. In the former they are less marked when the degree of saturation and the length of atmosphere traversed are the same as in the latter.

The accepted view, we believe, of absorption of vapours is that they give linear absorptions in certain thicknesses, and as the thickness increases or the density becomes greater, the lines

[1] "The Influence of the Atomic Groupings of the Molecules of Organic Bodies on their Absorption in the Infra-Red Region of the Spectrum." *Phil. Trans.*, Part III., 1881.

[2] *Phil. Trans.*, 1880.

[3] The black lines given in the diagram are merely lines of reference, and do not represent the aqueous absorption under consideration.

blacken, new lines appear, and gradually total absorption sets in in the region where the lines are most numerous and close. It is in the range of possibility that the presence of a small quantity of vapour might show itself as a haze over some region of the spectrum; if, however, the quantity was gradually increased, the haze would give place to lines, and the phenomena just described would be repeated. Suppose several localities of absorption to exist, the absorptive power of the vapour increasing the further down in the infra-red the locality was situated, it might happen that, whilst one locality showed only a haze of absorption, one further down might show total absorption, and some locality between these two should show linear absorption.

In the case of the absorptions in the solar spectrum we find a very different state of things existing. A comparison of the photographs taken in London on days of different dryness, and with those taken at the Riffel, shows that the linear absorptions are not increased in number or intensity; except so far that the blackness of the lines is increased by the blackness of the banded absorptions, and the same blackness can be induced by placing a certain thickness of water before the slit of the spectroscope: another point is that the Fraunhofer lines in certain regions (say λ 9420 to λ 9800) are so irregularly distributed as to preclude the idea that they all belong to the absorption of aqueous vapour, yet all are equally darkened by the band, and they do not spread out as the blackness of the band increases. This is against the view of the bands being formed by aqueous vapour, as we know it.

The question then arises as to what these "water-bands" can be due—if not due to vapour. This we consider an open question, and one which should be discussed. All we can state is that the absorptions shown are similar to those of water (liquid) and they do not seem to point to the watery stuff existing as vapour,[1] if we take the visible spectrum as a guide. An intense blue sky at sea-level is often indicative of moisture in the atmosphere, and it also seems to be indicative of finely suspended matter of some kind. If this be the case, can this suspended matter be suspended water stuff? for if it be not, there is no reason why the sky should be bluer on a moist day than on a dry day. We would remark that the deep blue sky at sea-level is of a different colour to the black-blue of high altitudes where, if they exist, the fine suspended particles would be largely diminished in number, and the coarser particles which cause white haze would also be fewer. The great difference of

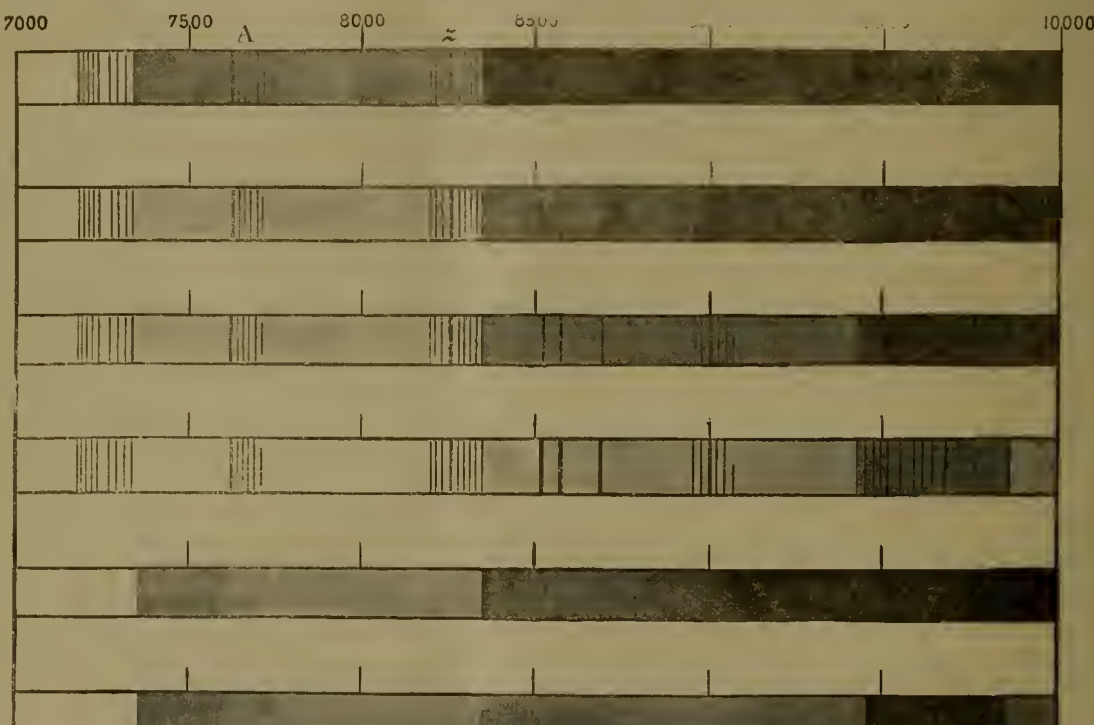


FIG. 1.

FIG. 2.

FIG. 3.

FIG. 4.

FIG. 5.

FIG. 6.

the intensities of the light from the blue sky in England and at 10,000 feet was determined by one of us and communicated to the British Association at Southampton, and the enormous disparity between the two has some bearing on the question we have been discussing.

In the above paper we have described the absorption due to "water stuff" in the atmosphere to λ 9800, as it is only to that wave length to which the normal spectrum has been as yet published. We wish however to add that there are bands in the solar spectrum commencing at[1] λ 9800, λ 12,200, and λ 15,200, and giving step by step absorptions from one wave-length to the next, as in the diagram, which also corresponds with *cold* water bands. The absorption in the locality from 12,200 downwards is usually total, and it is only on dry cold days or at high altitudes that rays of sufficient amplitude can penetrate to cause photographic impression to be made.

April 5.—"Observations on the Colouring-matters of the so-called Bile of Invertebrates, on those of the Bile of Vertebrates, and on some unusual Urine Pigments, &c.," by Charles A. MacMunn, B.A., M.D. Communicated by Dr. M. Foster, Sec.R.S.

In this paper the result of a systematic examination of the bile and various extracts of the so-called liver of Mollusca and Arthropoda, and of the pyloric or radial cœca and other appendages of the digestive system of Echinodermata is described. The universal distribution of one colouring-matter, which by appropriate experiments is shown to be a chlorophyll pigment, is proved. It occurs in the above organs, and can be detected in the bile of specimens of *Helix* after a six months' fast; for this colouring-matter, since it is found in the appendages of the enteron, the name enterochlorophyll is proposed. The slight differences observable in different cases are shown to be due to the probable greater or less amount of the usual chlorophyll constituents, blue chlorophyll, yellow chlorophyll, and chlorofucine, and the presence of xanthophyll, lutein, or tetronerythrin. Enterochlorophyll is shown to be much more abundant in the liver of Mollusca and in Echinodermata than in Crustacea, as the livers

[1] Unless it be held that the water itself holds vapour in solution.

[1] These wave-lengths are taken from the map from the *Phil. Trans.*, 1880, and are only to be considered approximate.

of the last generally contain more lutein, or sometimes tetronerythrin may be present.

The presence of reduced hæmatin is also demonstrated in the bile of the crayfish and in several pulmonate Mollusca, and its respiratory and other uses discussed.

The conclusions which these observations and others led to are summed up as follows :—

(1.) The existence of enterochlorophyll in the so-called liver, or other appendages of the enteron in Invertebrates is definitely established.

(2.) This pigment occurs in greatest abundance in Mollusca, it occurs less frequently in Arthropoda, and its presence in Vermes is not proved.

(3.) The pyloric cœca of starfishes contain it in great abundance, also the intestinal appendages of *Echinus*, which fact shows that the former function like the so-called liver of other Invertebrates.

(4.) The bile of the crayfish and that of most pulmonate Mollusca contains hæmochromogen ; in the latter it is generally accompanied by enterochlorophyll, and appears to be concerned more in aërial than aquatic respiration.

(5.) The so-called liver of Invertebrates is a pigment-producing and storing organ, as well as being concerned in the preparation of a digestive ferment.

(6.) The presence of hæmochromogen in the bile of Invertebrates is apparently determined by their mode of living, and it does not appear to be distributed according to purely morphological considerations.

The remainder of the paper deals with vertebrate bile pigments, and contains some observations on abnormal urinary colouring matters, mainly with regard to their spectroscopy. The various bile pigments of Städeler are first dealt with, and some remarks on the bile spectra of animals follow ; here it is shown that urobilin can be extracted from the liver of *Salamandra maculata* by means of alcohol, that it is absent from reptilian bile during hibernation, and that the liver of some fishes may contain tetronerythrin which can be extracted from them by suitable solvents. The latter fact suggests an analogous function to that of the liver of Invertebrata.

The results of the examination of a green hydrocele liquid are detailed, which showed beyond doubt that biliverdin was present, and since in that case its origin could be traced to blood pigment, the origin of biliverdin from blood pigment is demonstrated.

The identity of stercobilin and hydrobilirubin got by the action of nascent hydrogen on bilirubin is proved, and a difference between them and febrile urobilin shown to exist.

The statement that the absorption-bands of sheep bile are the same as those which occur in Gmelin's reaction is shown to be erroneous, and a brief description of the method of isolating the colouring-matter of sheep bile and the wave-lengths of its different solutions given. Chlorophyll is shown to be absent.

Under the head of urinary pigments it is shown that the feeble bands described by me in a former paper in the spectrum of febrile urobilin are not due to impurities, but are as much part of the spectrum as the band at F. Urohæmatin and its difference from hæmatoporphyrin and its pathological significance are discussed. A simple method for the detection of indican in urine, some remarks on uroerythrin, and on a peculiar red colouring-matter in pale urine, somewhat like urrhodin, follow. The deductions from this part of the paper cannot be very well given in the form of conclusions, and are therefore scattered throughout the paper.

A drawing of the microscopic structure of the liver of Limax, showing the enterochlorophyll within the liver cells, and maps of the most important absorption spectra described, accompany the paper. All readings are reduced to wave-lengths.

Physical Society, April 28.—Prof. Clifton in the chair.—A paper on colour-sensation, by Mr. H. R. Troop, was read by Mr. Walter Baily, secretary. The author showed that more than three colour-sensations were consistent with the theories of Maxwell and Helmholtz, and explained that four primary separate colour-sensations, in couples, served the theory as well as three. The author gave reasons for the existence of a fifth sensation—that of white. Mr. Stanley mentioned that his father was colour-blind to green, and saw it as a brown. He considered partial colour-blindness very common. Mr. Lewis Wright stated that he found in optical experiments a partial colour-blindness from time to time, and between one of his eyes and the other. He recommended the study of this partial and variable blindness to colours. Prof. Clifton stated that he had found similar variations amongst his students, and considered that one in three was unfit for delicate optical experiments.—Sir John Conroy exhibited a new photometer, which is a modification of Ritchie's, the white screens not meeting at an angle, but almost meeting, and one projecting a little beyond the other, so that the eye could see the outer side of one and a little of the inner side of the other, both visible surfaces being illuminated by the lights. The screens were inclosed in a blackened box.—Mr. Walter Browne then read a paper on the causes and consequences of glacier motion. After reviewing the various theories of glacier motion and criticising each, the author gave reasons for preferring that of Mr. Moseley, namely, expansion by heat. He showed that the regelation theory now commonly accepted appears inadequate, inasmuch as it does not explain the state of flow at low temperatures. Mr. Stanley pointed out that Forbes in his work on Norway gives expansion as a cause of glacier motion. Prof. Perry referred to the experiment of Mr. Bottomley (in which a wire, weighted at the ends, cuts its way through a block of ice) in support of the regelation theory ; and Prof. Guthrie described an experiment he had made of the same kind, using a copper wire and a silk cord of the same thickness, equally weighted, on the same block of ice. The wire cut through, but the silk did not. Prof. Ayrton explained this on the assumption that the wire conveyed heat from the air, and enabled the weighted wire to lower the temperature of the ice to the melting point, whereas the silk could not do so without more pressure, that is weight. Mr. W. Coffin referred to the ice-houses of Lake Superior, where he has seen heavy iron implements lying on blocks of ice at a low temperature, without sinking in. Sinking took place when the sun shone on the ice. Prof. G. Forbes said that below forty feet in rock variations of temperature were imperceptible, and probably it was the same with ice. Prof. Macleod, Mr. G. R. Griffiths, Mr. W. Baily, and the President also took part in the discussion.—Prof. Fuller then took the chair, and Prof. Clifton exhibited a new spectrometer of his design. In the spectrometer it is important that the axis round which the prism turns and the axis round which the telescope turns should not be inclined, and in the new instrument these axes are coincident. A single cone, turned very true, has the telescope piece, the circle, and prism plate fixed upon it.

Institution of Civil Engineers, April 24.—Mr. Brunlees, president, in the chair.—The paper read was "Resistance on Railway Curves as an Element of Danger," by Mr. John Mackenzie, Assoc. M. Inst. C.E.

Berlin

Physiological Society, March 30.—Prof. Du Bois Reymond in the chair.—Instead of the condensed milk, which, owing to its large percentage of sugar, has not kept its place as a food for children, a preparation of milk has lately been introduced into the market from Switzerland, which is protected against fermentation and decomposition by previous cooking. Dr. A. Baginski has chosen the relation of this new infants' food to the digestive ferments as the subject of a comparative investigation, which is not as yet concluded, but which has elicited some physiologically interesting facts about the action and occurrence of these ferments. The rennet ferment is well known to act upon milk both when it is sour and when its reaction is neutral or alkaline, but the rapidity of the curdling when acted upon by the ferment is different for different temperatures. At the temperature of the room the milk curdled only after twenty or thirty minutes ; at a temperature of 20° to 25° C. the curdling was already completed in five minutes ; at from 30° to 35° C. the curdling lasted about one minute, and it took place at still higher temperatures up to 55° C. in still shorter time. On the other hand, at 60° C. and at higher temperatures the action of the rennet was delayed, and soon ceased altogether. In previously boiled milk rennet also failed to bring about curdling. The rennet ferment was found not alone in the stomach, but also in the small intestines and in several plants, *e.g.* in *Carica papaya*, in artichokes, and in figs. In other plants it was sought for in vain. Decomposition ferments had various actions upon rennet ; sometimes they destroyed its action, at other times they did not ; the former was particularly the case when the fluid was strongly alkaline. Pepsin had no disturbing influence upon the activity of the rennet, although trypsin had to a marked degree. Dr. Baginski made similar observations upon the effect of decomposition ferment, pepsin, and trypsin upon each other.—Prof.

Brieger reported upon the results of his attempts at a more accurate study and determination of the violent poisons which develop out of animal substances, and which not unfrequently bring about sudden death after eating stale cheese, stale sausage, or stale fish. Prof. Selmi made the first step towards the knowledge of these poisons by preparing powerfully acting bodies out of corpses and decomposing animal matter. These he described under the name of "ptomäin." Prof. Brieger has now investigated their occurrence in analogous substances by means of the reactions that Prof. Selmi gave for these ptomäins, and he has found, among other things, that neurin when exposed to the air very soon develops such a ptomäin, which, like the rest, kills frogs and rabbits with the symptoms of coma. If the neurin remains a long time exposed to the oxidising influence of the air, the violent poison disappears. Further, a poisonous body belonging o the same group was found in a number of artificial peptones, but not in all, and here also this substance was soon destroyed by further alterations taking place in the peptone. This fact could be quite universally established, viz. that a further progressing decomposition in dead bodies destroyed all poisonous substances. Prof. Brieger next approached the task of isolating these easily decomposable and violently poisonous bodies. He succeeded, by working on large masses of dead animal bodies with a series of chemical processes, in preserving the poison in beautiful, large, needle-shaped crystals. He has not as yet determined whether these consist of the poisonous body pure, or whether they also contain other bodies. Therefore the chemical composition of the ptomäins has not yet been certainly determined.

PARIS

Academy of Sciences, April 30.—M. Blanchard in the chair.—The following papers were read:—On the reduction of the barometer and the pendulum to the sea-level, by M. Faye. While Poisson's correction for attraction of a continental mass may be suppressed, the attraction of a hill or mountain on which observations are made must not be neglected. At a station in mid-ocean, such as a volcanic or coral island, the attraction of the island must be considered. Correction for masses like the Alps or Himalayas is much more difficult.—On the pyro-electricity of quartz, by MM. Friedel and Curie. They dissent from Herr Hankel's views as to the distribution of electric tensions in crystals.—On a quaternary base derived from oxyquinoleine, by M. Wurtz.—Prolonged anæsthesia obtained by protoxide of nitrogen at normal pressure, by M. Bert. The new method he has tried on animals is to cause anæsthesia first with the pure protoxide, then give a mixture of the protoxide and oxygen (the blood then recovering the oxygen necessary to it); then the pure protoxide may be given again. Thus both asphyxia and return to sensibility are obviated. A dog was kept insensible half an hour. A mask and two caoutchouc bags are all that is necessary.—On the project of the African interior sea, by M. de Lesseps. He replies to M. Cosson.—On a theorem of partitions of complex numbers contained in a theorem of Jacobi, by Prof. Sylvester.—*Résumé* of meteorological observations made during the year 1882, at four points of Haut Rhin and the Vosges, by M. Hirn. *Inter alia*, the greatest actinometric differences do not always correspond to the most limpid skies. The slight mist or dust, which stops part of the luminous rays, does not absorb the calorific rays.—A new general formula for the development of the perturbative function, by M. Baillaud.—Observations of solar spots and faculæ at the Royal Observatory of the Roman College during the fourth quarter of 1882, by M. Tacchini. After the secondary minimum in August, the spots increased to a considerable maximum (relatively) in November, then decreased suddenly to a minimum in December. During 1882 there appears a greater activity than in 1881. The mean of faculæ is slightly greater in 1881 than in 1882.—Observations of solar protuberances, faculæ, and spots at the Royal Observatory of the Roman College, during the third and fourth quarters of 1882, by the same. The number of protuberances *per diem* was nearly the same as in the first half-year, but the height and extension were somewhat greater. The minimum was in September and October. Spots, faculæ, and protuberances were more numerous near the equator than in the previous half-year.—Observation of the transit of Venus at St. Thomas in the Antilles by the Brazilian Commission, by M. de Teffé. The third and fourth contacts were observed.—On the use of a birefringent glass in certain observations of spectrum analysis, by M. Cruls. By giving the crystal an alternative motion of rotation the extraordinary spectrum is displaced, and the eye is thus helped to see peculiarities better than if the spectrum were at rest. Again, the two spectra of a faint star may be so juxtaposed that the bright parts of the one correspond to the dark channellings of the other, so that vision is aided.—Determination of a particular class of surfaces, &c. (continued), by M. Darboux.—On continuous periodic fractions whose numerators differ from unity, by M. de Jonquières.—On the generalisation of the theorem of Fermat, by M. Lucas.—On the same, by M. Pallet.—On the groups of linear equations, by M. Poincaré.—On some double integrals, by M. Goursat.—On the Eulerian functions, by M. Bourguet.—On the cycle of motors with explosive gases, by M. Witz.—On the transmission of sound by gases, by M. Neyreneuf. With a sensitive flame arrangement he proves that carbonic oxide has about the same transmitting power as air; carbonic acid much greater. He finds Hauksbee's law inexact.—On the analogy that exists between the allotropic states of phosphorus and of arsenic, by M. Engel.—Research on the metallic derivatives of amides; means of distinguishing a monoamide from a diamide, by M. Gal.—On a process of hardening soft calcareous stones by means of fluosilicates with a base of insoluble oxides, by M. Kessler.—On a means of foreseeing liberations of firedamp, by M. de Chancourtois. On the supposition that movements of the earth's crust often cause such liberation, he proposes the use of delicate seismographic apparatus.—New histological researches on the termination of the biliary conduits in the lobules of the liver, by M. Kanellis.—On the structure of the nervous system of Hirudineæ, by M. Saint Loup.—On the incubation of eggs of a hen affected by cholera of fowls, by M. Barthélemy. The eggs contained germs of the microbes; these were only developed with aërial respiration when the allantoid yielded to the blood the oxygen necessary. (The development of the embryo stopped between the eighth and the tenth day.)—Comparison between the bacilli of tuberculosis and of leprosy (continued), by M. Babes.—M. Common presented a negative photograph of the nebula in Orion (taken January 30). An equatorial reflector of three feet aperture was used, and the dry plate was exposed 39 minutes.

CONTENTS

THURSDAY, MAY 17, 1883

THE FISHERIES EXHIBITION

"Do you know me, my lord?
Excellent well; you are a fishmonger."—*Hamlet.*

THE exhibition which was opened last Saturday by the Prince of Wales on behalf of Her Majesty the Queen is the latest of a series of such shows of matters relating to fish and fishing apparatus which was initiated by the French at Arcachon, other exhibitions having followed in subsequent years at Amsterdam, at Norwich, at Berlin, and at Edinburgh. Though in this country the accumulated knowledge and experience of scientific zoologists is not made use of either by the Government, or by local authorities, or by private capitalists in order to render our fisheries more productive, or to prevent the total destruction of some branches of them (except in the case of the salmon fisheries), yet the Fisheries Exhibition will have some interest for scientific men and for the readers of NATURE.

It is true that the present exhibition differs from its continental predecessors in the fact that it is a private undertaking entirely organised by practical men who would disclaim the title of "scientific" for themselves, and who have not largely availed themselves of the services of the professional zoologists of the country in carrying out their enterprise. Nevertheless the exhibition will have a scientific character and importance in consequence of the fact that almost without exception every foreign country which takes part in the exhibition is represented by distinguished zoologists, who have been delegated by the governments of the countries to which they belong, to take charge of and to organise the exhibition of such objects as in their judgment may best serve to illustrate the vast variety of matters of interest and instruction connected with their fisheries. From the republican United States of America, from democratic Norway, from Holland, from Sweden, and from Italy skilled zoologists have been sent by their respective Governments and are at this moment in London in order both to teach and to learn at the fisheries show.

It will be interesting to compare the results which these skilled officials and men of science can produce with those offered by the crowd of independent English exhibitors, manufacturers, fishery owners, fishmongers, and naturalists. Hitherto in the great fishery exhibitions England has been represented at a great disadvantage, for although the Governmental departments of fisheries control and inspection of foreign states have cordially responded to the invitation sent by the committee of the present London exhibition, yet on no occasion has the English Government assisted to place before the public in other countries any of the methods or products of English fishing. On the present occasion, though as hitherto the English Government has no official machinery for representing or dealing with British fisheries generally, and practically takes no part in the affair, yet in consequence of the activity of the large committee of gentlemen who have organised the exhibition, we shall no doubt see a much fuller representation of British fishing enterprise than at any former exhibition.

It is too soon at this moment, when many cases of objects are still unopened and sufficient time for a careful inspection of the exhibits has not elapsed, to offer any detailed remarks on the teaching to be derived from the fisheries show. On Saturday the special exhibit, which cannot be retained permanently during the whole period for which the show is open, was that of the fisherfolk themselves. Amongst the men the East Anglian herring fishers of Yarmouth and Lowestoft carried off the palm by their fine physique, intelligent faces, and sturdy bearing, wonderfully like to their brother Norsemen from the other side of the North Sea. These and the bright fearless faces of the Newhaven fisher-girls as they sat side by side with the strangely capped women from Boulogne and from the Dutch and Belgian coast, who good naturedly took part in the ceremony of Saturday last, were sufficient to demonstrate that whether British fisheries need or do not need to be improved and developed by that scientific supervision which is applied to the harvest of the sea on foreign shores, the race of men and women occupied in carrying on those fisheries bring to their business the fullest measure of intelligence and physical capacity. It is due to the courage, skill, and vigour of these fisherfolk that British fisheries continue to flourish, though their enterprise is unaided by the science of a Government department and their market is systematically injured by the devices of "middle-men."

Possibly the London Fisheries Exhibition of 1883 may have a result in regard to the British interests there represented similar to that which the Great Exhibition of 1851 effected in regard to the various art manufactures of the country. Just as the public demonstration of British inferiority in the matter of artistic workmanship led to the action of the Government in promoting a remedy in the foundation of schools of art and design, so the extraordinary contrast afforded by the British and Foreign exhibits on the present occasion in all that relates to a reasonable use of accurate knowledge (otherwise called science) in dealing with fish, oysters, lobsters, &c., may lead to an effort on the part of the constituted authority to imitate in some way the action of foreign governments (whether popular or paternal) in retaining the services of competent zoologists for the purpose of continually acquiring new knowledge in regard to fishes, and in particular of devising new ways of increasing and protecting the annual yield of fishes in the market.

It is a remarkable fact that for the purpose of dealing with questions and effecting practical objects connected with the economic aspects of the vegetable kingdom, the British Government supports the most efficiently organised botanical institution in the world. The Royal Gardens at Kew are the source of a ceaseless stream of scientific information and advice which is poured by every mail into all parts of the globe where our colonies extend, and it may be truly said that the pecuniary value of the scientific knowledge to British commercial enterprise, which has thus been furnished, is gigantic.

It does not admit of any question, that a parallel, though not in the first instance so vast a service, might be rendered to British industrial and commercial interests by a governmental zoological institution, to the scientific staff of which might be intrusted for study and control, not only matters relating to the sea- and river-fisheries of these

islands, our oyster, lobster, and shell-fish fisheries, but also matters concerning the pearl fisheries of India, the sponge-fisheries of the Bahamas, and the possible coral fisheries of the Australian coast. Further, the duties which even among the self-helping inhabitants of the United States are assigned to a State entomologist, might here also be discharged. From the duly established officials of such a state zoological laboratory or institute, the Foreign Office and the Colonial Office could at once obtain full and decisive information enabling them to act intelligently in relation to the importation of the Phylloxera pest, whilst the Home Office might gain courage in the presence of the Colorado beetle. It seems strange that the creation of an official laboratory of economic zoology has been so long delayed.

We shall be able to judge in the case of the present exhibition whether the cooperation of scientific men would have rendered the English department more instructive than it is under the present conditions, as compared with the scientifically organised exhibits of foreign countries. The comparison of the official catalogue of the London Exhibition with that of the Berlin Exhibition will be important in the same direction. With regard to the essays for which the committee has offered prizes, it may at once be stated that unfortunately no steps have been taken to bring the questions concerning which treatises are desired under the notice of the persons most likely to be able to deal with them satisfactorily either in this country or abroad. A series of valuable reports might have been obtained and circulated in connection with the exhibition by a sufficiently public appeal to the zoological world made in due time. It may yet be not too late to take some steps in this matter.

SCIENCE AND ART

NO one will be surprised that Mr. Huxley took advantage of the opportunity afforded him at the Academy dinner to reply to some remarks made by Mr. Matthew Arnold on a like occasion two years ago. Mr. Arnold, we presume, does not claim to possess that amount of knowledge either of art or of science which would render him a prejudiced witness, and, being unprejudiced, he drew a terrible picture of the future of art, not only in this, but in all other countries, unless some very decided steps were taken. Time out of mind, according to Mr. Arnold, art and literature had divided the sweets and beauties of this world between them, but now, in these latter days, that terrible thing science—

"Monstrum horrendum, informe, ingens, cui lumen ademptum,"

was about to bar their future progress, and invade and destroy the fair kingdoms of thought and work gained from the unknown by the labours of both. Hence the necessity of an alliance offensive and defensive against the common enemy; hence the artist and the man of letters were to band themselves together to stamp this new hydra from out the land.

It was not to be expected that such a view as this would be allowed to pass unchallenged by Mr. Huxley. He declined to regard science as an invading and aggressive power, eager to banish all other pursuits from the universe. Putting Mr. Matthew Arnold's view in a more concrete form, he represented it as picturing science rising as a monster from out the troubled waters of the sea of modern thought, intent upon devouring the unprotected Andromeda of Art. For him Literature was Perseus equipped with the swift shoes of the ready writer, and the cap of invisibility of the editorial article, while the death-dealing quality of Medusa's head had a fitting representative in the sting of vituperation. Mr. Huxley's remarks dealt less with Andromeda than with Perseus, to whom he suggested the advisability of thinking twice before trying conclusions with the risen monster. He ended by showing how necessary Art and Science were to each other, how each was strong in the other's strength, and how they were never likely to be sundered, but were certain to twine round each other more closely, and to help each other more as time went on. Agreeing as we do altogether with Mr. Huxley, we think, however, that another view is worthy of consideration. For ourselves, although likening art to fair and chained Andromeda, we cannot admit that science is correctly represented in the form of the monster. Without further considering of whom or of what the monster may be typical, it seems to us perfectly certain that the Perseus of whom the Andromeda of Art stands so much in need is not Literature, but Science, because this Perseus alone can give the help and render the assistance which the maiden needs so sorely at the present moment.

Occasion has been before taken in these columns to point out how one of the greatest revivals of art in the history of the world was contemporaneous with the dawn of one of those sciences which must for ever lie at the base of much work in art: we refer to the science of anatomy; and when one looks round this year's Academy and compares the work based upon this branch of knowledge, the anatomy of form, with that connected with the other branch of knowledge which has to do with the anatomy of light and colour, one cannot but feel that the Andromeda of Art is being sacrificed indeed. Landscape painting has as close a connection with physical science as figure painting has with anatomy, and we cannot help thinking it is because physical science has not been sufficiently taught in our public schools, that our landscape painting is, if we are to judge by this year's pictures, not advancing, but almost retrograding. The man who finds anatomy too difficult for him and rushes into landscape soon discovers that there is something there which he has not learned, but which has to be learned ere he can achieve distinction; and like too many others he has to give up the battle ingloriously. Not for many years has there been such an absence of landscapes of the highest order as in the present Academy; and in order to show, on the one hand, how those artists who have some knowledge of the branches of science which bear upon their work in art have succeeded in filling their canvases with worthy representations of natural effects, and, on the other hand, how those who lacking this knowledge are only successful in producing misrepresentations and distortions of nature, we shall on a subsequent occasion give a series of notes upon those pictures which fall within the reach of our remarks. In some pictures the ignorance of one part of nature has been as great as if a portrait painter had painted a face in which the mouth was represented between the eyes and

the nose, or again as if he had painted feet instead of hands.

There is one instance so much in point that we may at once refer to it. One artist, who shall be nameless, has attempted to grace his picture by introducing into it a rainbow. Now if the rainbow had been part of the human form it would have been studied, there would have been books about it, and the artist would have made it as much his own as the student of physical science, since some artists study anatomy as closely as does the man of medicine, but, because the rainbow happens to lie outside that branch of scientific knowledge which is generally supposed to be the only branch to which artists need turn their attention, the painter thinks that he may treat it anyhow. Thus we have had rainbows with the colours—which in nature are absolutely definite in their order and arrangement—painted in reverse order; again, we have had a rainbow, which must always appear to form part of a circle, painted in perspective; but the rainbow fancier of this year has almost transcended the want of observation shown by his predecessors. Possibly ignorant of the fact that all primary rainbows are alike; that the order of colours, from red through orange, yellow, green, blue, indigo, to violet, is dominated by a most rigid law, to which there is, and can be, no exception; the artist has chosen to paint his rainbow with the violet in the middle This seems to indicate either such looseness of observation or such contempt for nature—and the painter may take his choice between these two alternatives—that we doubt whether side by side with either there can exist that sympathy with nature which must lie at the root of all good work in art. We shall show on a subsequent occasion that this picture is only typical of a good deal of artistic work, which must in the nature of things act like a discord, and put the eye and the heart of the painter out of tune.

Those branches of science to which we have to make reference in these columns have to do of course with the forms and colours of clouds and sky and natural objects generally, and the laws of reflection, and if an artist will paint suns and moons, then with those elementary astronomical principles which have to deal with the appearances of these bodies, and which are not beyond the comprehension of a child in the Fourth Standard of a public elementary school. It is not therefore imposing too much upon an artist that he should know these things, and it is not too much to suppose that one who paints work on which he wishes to build his fortune or his reputation as the case may be, should wish to appeal to a more or less cultured audience. At present, perhaps, it is only a select few who notice and deplore this want of harmony with nature which marks the productions of so many of our artists; but the love of physical science among the great mass of mankind grows stronger and more strong, and the circle of those who can discriminate between fact and fancy as displayed in the works which grace the walls of our picture galleries is daily becoming a wider one. We would therefore utter a word of warning to the artist who allows blunders to creep into his picture because he thinks nobody will find them out. Somebody is sure to find them out.

The opportunities which artists in following their profession have of studying nature in very varied moods enable them to see the actual phenomena, where *a priori* considerations leave a student who lacks such opportunities entirely in the dark. Several very interesting questions are raised by some of the pictures in this year's Academy, and the candid critic must acknowledge that many of them give much food for thought and suggestions for future inquiry and study on his own part.

THE TRANSIT INSTRUMENT

A Treatise on the Transit Instrument as Applied to the Determination of Time; for the Use of Country Gentlemen. By Latimer Clark, M.I.C.E., &c. (Published by the Author.)

IT is something new to have a book on the transit instrument for the use of country gentlemen. It is something still newer to find that book brought out by an eminent engineer. In fact we may regard the publication of such a book, under such conditions, as a sign of the times, and as an indication of the slow but sure way in which science, and even the methods of science, are interesting a gradually increasing number of our educated classes. Mr. Latimer Clark has done his work in a most admirable manner, and no country gentleman who wishes to know a little more than he does at present about the practical working of a most fascinating branch of science, could do better than invest, not only in the book, but in the very satisfactory and handy little instrument which Mr. Clark has been wise enough to produce side by side with it. This transit instrument to which we refer, and which can be obtained of Mr. Coppock of Bond Street, is an excellent one of its kind. It is cheap—costing only about 10*l.*—and it is simple. The many parts of the instrument which form necessary adjuncts to it when used in an observatory are of course suppressed, but nothing is wanting which is really of importance to that public which Mr. Latimer Clark wishes to educate in its use The author is quite wise in the way he goes to work We naturally have a description of the instrument, and reference to the way in which it can be most conveniently and satisfactorily employed, nor are those necessary adjustments omitted without which of course the simplest instrument would be of very little use. Full instructions are then given for putting it in position, and Mr. Clark's form of instrument has a cover, by means of which, when once placed in position, say, out on a stone pillar on a lawn with a good north and south line, it can be left out with very little chance of its taking any harm in all weathers. The actual taking of transits, both of the sun and stars, are then dealt with, and we should add here that the transit eyepiece is armed with a system of seven vertical wires, so that the means of several transits over the wires can be taken in the ordinary way. The only objection we have so far found to Mr. Latimer Clark's form, is that there are no means of adjustment for the verticality of the wires. We regard this as a point which should be looked to, in case our author should be fortunate enough to induce a great many people to employ this cheap and simple form.

The corrections for longitude and latitude are next given, and we are glad to see that the book deals with these matters in a way not only far from dry, but so as to introduce a considerable quantity of very useful astro-

nomical knowledge put in a very simple and taking form. Thus, for instance, when Mr. Clark urges, and rightly urges, that the latitude of any particular house where it may be suggested to put up one of these instruments is more likely to be accurately determined by a reference to the Ordnance Map than in any other way, we are not only told that the Ordnance Map may be got for 1*s*., but the ignoramus is not even forgotten, and the way in which longitude is marked on the map is clearly stated. Mr. Clark it will be seen has spared no pains to make everything as clear as possible to everybody. Here, for instance, is what he says on counting time to a beginner in astronomical work:—

"In taking the time of transit by a watch some difficulty will at first be experienced, owing to the fact that watches tick various numbers of beats in a minute, but rarely any direct multiple of the second. Having carefully ascertained the number, the watch is placed to the ear at any even ten or twenty seconds, and the counting is continued by sound, thus: One and, one and, two and, two and, three and, three and, fifteen, fifteen, &c. In recording the transit, the minutes and seconds at the time of starting are noted, and the number of additional ticks is counted by ear, and these are afterwards converted into seconds and added. When a single observation is considered sufficiently accurate for practical purposes, it may be conveniently recorded by a chronograph, which is started in exact accordance with your clock a few minutes before it is required, and is stopped at the exact instant of transit. At night this dispenses with the necessity of referring to a lantern for time. These chronographs may now be obtained at a very moderate price, and when they beat quarter-seconds are very useful for counting, and for carrying the time of a clock or chronometer into the open air, and might be advantageously used on shipboard. The portable American clocks, which are sold everywhere for a few shillings, sometimes beat quarter-seconds, and serve well for counting time. They may be easily converted into rough chronographs by adding a stop movement in the form of a light spring pressing against the balance wheel; the second hands are, however, loose and inaccurate. A servant may be taught to record transits with the assistance of one of these chronographs very correctly.

"The writer, however, employs an arrangement which he considers still preferable. A rough pendulum about ten inches long is constructed out of a wire suspended by a flat spring or by a double loop of wire, and screwed at the bottom through a flat leaden plumb-bob; it is adjusted so as to beat half-seconds correctly. A very small glass bead or button is suspended loosely at the lower end of the rod, and strikes against a thin metal plate at each oscillation; it is started by a trigger of wood or wire, supporting the plumb-bob at a definite angle, and insuring uniformity of swing. This is fixed on a board and hung within reach of the instrument. It may be used in several ways; the trigger may be pulled at the instant of transit, the first tick being counted as one, and the counting being continued while the observer rises from his position, and obtains at leisure a coincidence with a given second on his watch, the number of beats being converted into seconds and deducted from the time noted. The observation is noted thus, 46m. 28-21, and is afterwards converted into

$$\begin{array}{rrr} 8^{\circ} & 46' & 28'' \\ & - & 10{\cdot}5 \\ \hline 8 & 46 & 17{\cdot}5 \end{array}$$

"Or the observer may start the pendulum just before the transit occurs, and having obtained a coincidence with his watch may continue to count by ear. The pendulum may, if preferred, be made to tick seconds by muffling one side of the metal strip with a piece of felt, and it may be removed after an observation, leaving the support and striking plate attached to the pillars of the transit. It oscillates on a rod or knife edge, and will continue in motion for two or three minutes."

As an appendix to the book we find transit tables giving the Greenwich mean time transit of the sun and certain stars for every day in the year, computed from the *Nautical Almanac*, and there is also a table for converting intervals of sidereal time into equivalent intervals of mean solar time. More recently the author has published a set of transit tables separately, and we would recommend those who use his book on the transit instrument to obtain them. To all who are not familiar with the use of the tables, clear and simple instructions are given, and we think they will prove of great use to those for whom they are intended.

Mr. Latimer Clark has certainly well deserved the thanks of all interested in astronomy, for the pains he has taken in thus endeavouring to popularise an instrument which, although it is the most important instrument used in astronomy for the determination of position, is at the same time one from which an immense amount of pleasure can be obtained by the merest tyro in science, whilst the great advantage of using such an instrument as this is, that no one can use it without rendering himself thoroughly familiar with some of the most important problems which lie at the root of any useful knowledge touching the stars, or the planet on which we dwell.

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications.*

[*The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to insure the appearance even of communications containing interesting and novel facts.*]

Fossil Algæ

In a review of Saporta's work on "Fossil Algæ" in NATURE (vol. xxvii. p. 514) there are certain opinions brought forward which ought not to be passed by without some remarks. At first it should be stated that Saporta, while still insisting upon the vegetable nature of his so-called "algæ," does not only defend his views about those doubtful bodies which have been the objects of my criticisms, but also "defends" true algæ, concerning which I have never expressed any doubt. Such are, for example, the Florideæ represented on the first three plates in his work, and by "defending" them he puts me in a somewhat false position, at least in the eyes of the readers who have not studied my work.[1] The real fact is that I have only questioned—and still do so—the vegetable nature of almost all those objects which Schimper in Zittel's "Handbuch der Paleontologie" comprises under the head of "*Algæ incertæ sedis.*" There is consequently some exaggeration in Saporta's statement of my opinion.

Now it is quite clear that fossil trails of animals must occur in most cases in relief on the under sides of the slabs, the tracks in such cases being impressions in the soft sand or mud, which have since been filled by sediment. And it is also quite natural that the trails of animals should especially be found where there are alternating beds of sandstones and shales.

Now it is a fact that the Bilobites, as well as Eophyton, *always* occur in this way, projecting as convex bodies on the under sides

[1] A. G. Nathorst, "Om spår af några evertebrerade djur arbderas paleontologiska betydelse." With a French *résumé*, "Mémoire sur quelques traces d'animaux sans vertèbres et de leur portée paléontologique." (*Svenska Vetenskaps Akademiens Handlingar*, Bd. xviii., No. 7. Stockholm: Norsted och Söner, 1880.)

of the sandstones. Both Dr. Dawson and Dr. Linnarsson therefore long ago expressed the opinion that the Bilobites of Sweden and America must have been trails of some animals. In order to explain this mode of occurrence so that it might not appear as proof against the vegetable nature of the bodies, Saporta takes refuge in a somewhat curious manner of fossilisation described and illustrated by woodcuts in the review referred to. As I feel sure that every one who has made himself acquainted with true modes of fossilisation will immediately be aware that the process adopted by Saporta is indeed most improbable, it will be superfluous to dwell any longer on that question. But even granted that the plants *sometimes* should occur in this way—which statement I, however, think must be due to some confusion as to the real facts—such an occurrence could never be regarded but as a very rare exception to the general rule. And it therefore does not explain why the Bilobites should *only* occur in this, for true plants, exceptional way, (on the under surfaces of the slabs), *never* as true fossils embedded *in* the rock. This mode of occurrence harmonises, on the other hand, perfectly with the view that the Bilobites are trails of some animals, while it *cannot* be explained on the supposition that they are true organic bodies.

One arrives precisely at the same conclusions on studying their external structure, which possesses pretty great analogies as well with the trails of Limulus, long ago described by Dawson, as with those of other Crustaceans, described by myself. It is true that Saporta lays great stress on some superficial markings which are to be seen on some of the French specimens; but those who have studied not only the French Bilobites, but also those from Sweden or America, will soon be aware that the markings referred to are quite accidental. It is indeed surprising that Saporta, while adopting my views concerning Crossochorda, does not see that the Bilobites are somewhat analogous forms, though much larger. There is consequently no reason why they should be regarded as other than the trails of Crustaceans.

As for Eophyton, it is a pity that this should still be mentioned as possibly of organic origin. It occurs precisely as true trails on the under surface of the slabs; it is found in every system from the Cambrian to the present time, where it can still be studied on the seashores; all the different forms, under which it presents itself are also still to be seen there. Although it thus has been *proved* that it cannot be any organism, Saporta still adheres to the opposite opinion. Now, if he had read through my work, he would have learnt that I by experiment have demonstrated that Eophyton can not only be produced by drifting plants, but also by the tentacles of Medusæ or other soft bodies. Now there are casts of Medusæ associated with Cambrian Eophytons of Sweden, and their habits were probably—as I have elsewhere [1] tried to show—similar to those of the existing *Polyclonia frondosa*, which creeps on the mud by means of its tentacles, and it is therefore likely that the *Cambrian* Eophytons are of this origin.

It is further stated that "the Chondrites of the Flysch, strongly impregnated as they are with carbonaceous matter, are admitted on all hands to be Algæ, and the author asks how the same origin can be denied to casts of specifically identical Chondrites of the Cretaceous and so on to the Liassic forms." This argument is, however, a real "*petitio principii*," for it is so far from the actual state of things that the Chondrites of the Flysch are on all hands admitted to be Algæ, that many authors, and among them Dr. Th. Fuchs, of Vienna, whose excellent and exhaustive studies of the Flysch are everywhere known, hold a quite opposite opinion. And as for the supposed carbonaceous matter, it is not much better with this, as will be shown from a communication from Dr. Fuchs published in my work referred to: "The supposed carbonaceous nature of the Chondrites of the Flysch is in my opinion *a perfect mistake*. They are certainly very often quite black, but even in such cases they consist only of dark marl, *not* of coal."

Much more might be said on the fossil Algæ, but as I am about to combat the views held by Saporta more fully in a special work, I will here only add that I have found no statement whatever in his work referred to which would tend to alter my opinion, that almost all the "*Algæ incertæ sedis*" in Schimper-Zittel's "Handbuch der Paleontologie" are not vegetable fossils.

A. G. NATHORST

Stockholm, April 9

[1] A. G. Nathorst, "Om aftryck af Medusar i Sveriges Kambriska layer." (*Svenska Vetenskaps Akademiens Handlinger*, Bd. xix. No. 1, Stockholm: Norstedt och Söner, 1881.)

DR. NATHORST has certainly shown that many of the markings referred to Algæ by some authors might be tracks left by moving animals on a soft mud, but is there reason to suppose that there are conditions under which submarine surfaces of very soft mud with minute tracks have, or could ever have been preserved. On the other hand there is no question about seaweed having existed in Palæozoic and Mesozoic times, and either some of the markings in question are their prints, or no traces of them are preserved. Now it is an uncontroverted fact that even the most indestructible of all vegetable tissue, that of the Coniferæ, has been met with in the same condition of fossilisation, *i.e.* a projecting cast in sandstone on the under side of a slab, and without any internal trace of tissue or even of colouring due to carbon or iron, and Saporta has offered a satisfactory explanation of the origin of such casts. From the relative rarity with which terrestrial plants have been thus preserved, Nathorst almost derides Saporta's application of this explanation to fossil Algæ, yet it is by no means improbable that this may be their normal mode of preservation. The decay of dead olive-green seaweeds in water must be very rapid. The decomposition of some among them sets in almost immediately under water, and a colourless mucilaginous fluid is given off copiously. I have not watched the whole process of decay, but my impression is that the entire substance in some species would eventually pass away in a structureless glairy mass, and therefore that nothing but a hollow impression could ever be preserved. Casts of these would be more likely to be preserved in sand or mud than mere tracks, because the substance of the weed would occupy them, and prevent them from being immediately filled with the same quality of matrix as the surrounding rock, and until what would afterwards be a line of cleavage had been produced. So far therefore from its being exceptional for fossil seaweeds to appear as casts projecting from the under surface of the overlying mud, this is likely to be the normal condition in which fossil algæ are preserved. This is apart altogether from the question whether any of the Palæozoic markings are Algæ, for, these differ so considerably from any existing forms, that in the absence of internal structure it is quite unlikely that there will be any general agreement respecting them. These observations do not apply to the Rhodospermeæ, which scarcely enter into the question. Some simple experiments on the decay of seaweeds in fine sand under water, which any one at the seaside could make, would help to throw light on the subject.

J. S. GARDNER

The Weather and Sunspots

IN NATURE (vol. xxvii. p. 551) Mr. Williams ascribes the great cold of March, 1883, at the Riviera, to the absence of sunspots. There is the less reason for ascribing this cold to sunspots, as till now much more evidence goes the other way. And may it not be contended that this evidence is in favour of warm weather, with minimum sunspots in the tropics or in summer alone. The months of November to March, 1877–78, especially February and March, were so warm over an extensive area, especially in the interior of North America and Western Siberia, that the mean temperatures were nearly without precedent, while in no extensive country of the world the temperature was much below the average.

I give some data for March, 1874 (a season with a considerable number of sunspots), at Suchum-Kale, on the east coast of the Black Sea, a place in the same latitude as Cannes, and similarly situated in respect to sea and mountains; it is sheltered from cold winds, and much warmer than the surrounding country.

The observations in Russia being made at 7 a.m. and 1 and 9 p.m., and no minimum-thermometer used, the minima cannot be strictly compared.

The mean temperatures for a long average at Nizza (which are about the same as at Cannes) are January 47°·1, March 51°·8; at Suchum-Kale, January 43°·0, March 47°·8, being at both about 4° colder. Taking the mean of minimum and maximum as the daily mean at Cannes, and that of 7 a.m. and 1 and 9 p.m. as the daily mean at Suchum, we have: Coldest days of March, 1883, at Cannes, 10th, mean 35°·5, or 16°·3 below average; 11th, 34°·5, or 17°·3 below average; lowest minimum on the 11th, 24°·1, or 27°·7 below monthly mean temperature. Coldest days of March, 1874, at Suchum, 3rd, 19°·9, or 27°·9 below average; 4th, 20°·5, 27°·3 below average; 5th, 20°·8, 27°·0 below average. The lowest temperature at 7 a.m. was, on the 6th, 16°·4, or 31°·4 *below average monthly temperature*. Thus it is seen that at

Suchum, in the same latitude and in a very similar situation as Cannes, in March, 1874, a year with a considerable number of sunspots, there were three days which were more than 27° colder than the average, while in March, 1883, with little or no sunspots, the coldest days mentioned by Mr. Williams at Cannes was only 17°·3 colder than the average.

I want only to show by this example that if it is wished to prove anything as to the varying intensity of the sun's rays, a large number of observations in distant countries should be given, especially in middle latitudes, the work of Dové having well proved that there is always a compensation to a certain extent between cold and warm areas, and a very great number of these deviations being certainly due to causes which have nothing to do with anything beyond the earth's atmosphere.

St. Petersburg, April 17 A. WOEIKOF

Sheet Lightning

LOOKING to the south and south-east from the Bel Alp, the play of silent lightning among the clouds and mountains is sometimes very wonderful. It may be seen palpitating for hours, with a barely appreciable interval between the thrills. Most of those who see it regard it as lightning without thunder—*Blitz ohne Donner*, *Wetterleuchten*, I have heard it named by German visitors.

The Monte Generoso, overlooking the Lake of Lugano, is about fifty miles from the Bel Alp as the crow flies. The two points are connected by telegraph; and frequently when the *Wetterleuchten*, as seen from the Bel Alp, was in full play I have telegraphed to the proprietor of the Monte Generoso Hotel, and learnt in every instance that our silent lightning coexisted in time with a thunderstorm more or less "terrific" in Upper Italy. JOHN TYNDALL

May 12

I AM glad to find that M. Antoine d'Abbadie's remarks confirm in the main those which I have made on the above subject in NATURE (vol. xxviii. p. 4), especially as to the occurrence of lightning at a great altitude as observed in low latitudes.

In stating that he has frequently observed "thunder without lightning, and lightning without thunder," does M. d'Abbadie mean that, like every one else, he has observed thunder without observing lightning, and lightning without observing thunder? Or have we here a living advocate not only of the dumb lightning, but of the dark (lightningless) thunder?

The thin and local fogs which are not uncommon in thundery weather readily transmit the illumination of a distant flash of lightning. It seems highly probable that in such cases the lightning may be occasionally supposed to be an electric discharge occurring in the fog itself, just as a flickering aurora seen above thin clouds has often been supposed to have its habitat in the clouds themselves.

The suggestion of M. d'Abbadie is a fair one, and I for my part shall be glad to undertake observations of "sheet lightning" this summer in conjunction with any one resident about forty miles from this place, the observers interchanging reports by the earliest post after the hour of observation.

W. CLEMENT LEY

Ashby Parva, Lutterworth, Leicestershire

Hydrogen Whistles

IN his interesting communication on the above topic (NATURE, vol. xxvii. p. 491) Dr. Francis Galton has inadvertently fallen into a mistake which quite seriously affects the numerical deductions which follow. He erroneously assumes that "the number of vibrations per second caused by whistles is inversely proportional to the specific gravity of the gas that is blown through them."

It is well known that the number of vibrations is inversely proportional to the *square root* of the density or specific gravity of the gas. Hence for hydrogen, as compared with air, the number of vibrations per second produced by a given whistle would be increased only about 3·6-fold instead of 13-fold, as he estimates it. Similarly the number of vibrations by the use of hydrogen in the little whistle when set at 0·14 inches would be only about 86,533, instead of 312,000. JOHN LE CONTE

Berkeley, Cal., April 12

[THE objection of your correspondent is valid. I am informed independently and by high authority that the velocity of sound in hydrogen must be considered as barely fourfold greater than in air, the number of vibrations per second emitted by a hydrogen whistle being increased in the same proportion.

In making my earlier estimate I had been misled by an erroneous statement in a work that is still of much general credit and authority, to which I referred for ascertaining the velocity of sound in different gases, as it happened to be the book then nearest at hand, and as I have no special knowledge of the subject. It was the first edition of the *Penny Cyclopædia*, where in the article "Acoustics," p. 95, I lit upon the following passage, which professed to give the precise information I wanted:—"Thus air being about thirteen times as heavy as hydrogen, the velocity of propagation in the latter is about thirteen times that in the former." I need not take up your space by quoting the paragraphs before and after this, which emphasise and corroborate the statement and make it clear that it was no slip of the pen. Possessors of this Cyclopædia (I know nothing of subsequent editions) would do well to look out the passage and put a note of warning by the side of it.

The fourfold gain, or nearly so, of the hydrogen whistle is not to be despised. It is sufficient to establish its rank as the emitter of the largest number of aërial vibrations per second of any instrument yet contrived. My little whistle, of about 1 mm. bore, requires a very small supply of air, a bag that I fill with a single expiration containing enough to keep it in continuous sound for many minutes. As yet I have not got a portable holder for pure, dry hydrogen, but a well-known chemist is kindly making an experiment of one for me.

FRANCIS GALTON]

The Pillar of Light

I HAVE frequently observed this phenomenon. The first time I saw it was on April 8, 1852, when I saw it here at sunset, and on April 11 I saw it at sunrise when I was in the Irish Channel, near to Port Patrick, where I was laying a submarine cable.

In the *Monthly Notices* of the R.A.S. vol. xii. p. 185, there are several notices of its having been seen at that time in various places. I saw it last on April 6 this year, when it had the same appearance as previously, which is well represented by Mr. Symond's drawing on p. 7, except that the lower part is too bright, and it looks more correct when shaded with a pencil. The pillar is always perpendicular to the horizon and to the sun's position. I saw the zodiacal light several times in February, extending as far as the Pleiades, and at an angle of about 45°. I think it is highly probable that the pillar of light is caused by reflection from ice crystals, as we had very cold weather early in April, and have still. These atmospheric phenomena are often best seen reflected from a plate glass window.

Gateshead, May 9 R. S. NEWALL

Remarkable Lunar Phenomenon observed at Weston-super-Mare, August 21, 1861

AT about 8.30 p.m. a band of silvery light appeared proceeding from the lower margin of the moon, in a line perpendicular to the horizon. The width of this band was equal to the exact apparent diameter of the moon's disk. Slowly the band lengthened, until its upper portion reached beyond the moon to the extent of about two diameters, while the lower limb extended itself to about the length of four diameters, where its foot rested apparently on a light fleecy cloud. In a few minutes a similar band traversed the other at right angles, forming a perfect Latin cross, the brilliant face of the moon occupying the place of intersection. The arms of the cross were respectively about two diameters of the moon's face. The portion of the sky in which this occurred was clear, but clouds were slowly drifting from the west, and in ten minutes began to obscure this beautiful and unusual phenomenon.

The only record of any similar phenomenon which I can meet with is to be found in Lowe's treatise on atmospheric phenomena, wherein two instances are described. The observer of one was Dr. Armstrong, and the appearance was seen by him at South Lambeth on February 25, 1842. The other observer was Mr. Lowe himself, who was at Derby railway station when the phenomenon occurred. In both these instances, however, the crossbeam was absent. Although no hypothesis has been suggested to account for this appearance, it may be interesting to note that in the case recorded by Mr. Lowe, the very

hour of its occurrence is identical with that of the appearance of the phenomenon seen by me, and the day of the month so closely approximates as to be only one day later. That which Dr. Armstrong saw in 1842 was at the time of the full moon in February.

C. POOLEY

Curious Habit of a Brazilian Moth

AT the last meeting of the Literary and Philosophical Society of Liverpool (April 30) I read the following note on a remarkable habit of a Brazilian moth; and as it is a habit that has perhaps not been observed before, it may be of sufficient interest for insertion in NATURE.

The moth (of which I inclose a sketch) is a species of Panthera (*P. Apardalaria*).

When rambling about the rocky beds of small streams on the Serra da Contareira, near San Paulo, I have often been struck by the great numbers of yellow and black moths that flew up from the water as I disturbed them by my presence. On careful examination I found that these moths were resting upon the wet stones, in many cases even with a film of water flowing over the spot on which they had settled, and were all engaged in sucking up the water through the proboscis (I can hardly call it drinking, for no imaginable thirst could account for the enormous amount of water sucked up), and this water was passing through the moths, minute drops forming on the tip of the abdomen, and falling off as formed. I timed several specimens, and found that the average rate was fifty drops per minute. I have observed the same individual remain in the same position with the action going on unceasingly for three hours; and in all probability it had been there some time before I observed it, and remained after I went away. But even in this length of time the quantity of water passing through the moth was enormous in proportion to its size. The drops I did not actually measure, but they are probably between 1 and 2 millimetres in diameter. Taking them to be 1·5 millimetres in diameter, the total amount of water in the three hours was 15·84 cubic centimetres, or almost exactly a cubic inch. This quantity is equal to about 200 times the volume of the body of the moth!

The tibiæ of the hindlegs are very thick and are armed with long hairs, that by their capillary action prevent the moth being immersed in the water. I have often seen one of them knocked down by a little spurt of water splashing over the stone on which it was standing, and it recovered itself immediately without being wetted in the least.

Upon my return to Brazil I shall try to measure exactly the amount of water passing through one of these moths. And it would be most interesting to find out what is the object of this excessive drinking. Can it be that the moth extracts nourishment from minute particles of organic matter contained in the water?

I may remark that the water of the streams where I have observed the moth is very clear and pure.

E. DUKINFIELD JONES

Acrefield, Woolton, Liverpool, May 5

Leaves and their Environment

I TAKE the following from an experiment which I made two years ago. I think it throws some light on the point under discussion:—

On May 8 six young pea plants, similar in size, &c., were transplanted from the garden into three large flowerpots, a pair in each, and were covered with bell glasses. On next day an apparatus for generating a constant stream of carbonic acid gas was connected to No. 1 bell glass. No. 2 was left normal. No. 3 inclosed a small disk of caustic potash solution. They all had as nearly as possible the same amount of sunlight, and the same measured quantity of water was given to each.

Taking the notes referring to the leaves only I find on May 21: "No. 1, vigorous large leaves. No. 2, much smaller leaves. No. 3, leaves smaller than No. 2, with edges serrated as if the veins were growing on, but could not find food for fleshy part of leaf—really a starved plant."

On May 27 the plants were taken up and washed, when No. 1 weighed 148 grains; No. 2, 115·5 grains; and No. 3, 87 grains. After drying, the weights of Nos. 1 and 3 were as 19 to 13. The longest leaf on No. 1 measured $1\frac{5}{8}$ in., and on No. 3 $1\frac{1}{8}$ in.

J. BROWN

Belfast, May 3

Foam Balls

IN NATURE, vol. xxvii. p. 531, there is a mention by Mr. J. Rand Capron of foam balls. These are common on the coast of the Northern United States, especially of a cold dry day, when, if there be much wind, these huge foam balls, which may reach a diameter of two feet or more, are rolled up the beach. Their weight soon changes their form, so that at last they present the appearance of long white rolls of sparkling foam. This singular appearance was first described in verse, so far as I know, by Dr. S. Weir Mitchell, of Philadelphia. The verse, as I recall it—I quote from memory—is this:—

> "And wilder yet when of a winter day
> The cold dry norther rolls athwart the beach
> The gleaming foam balls into serpents white,
> And all the sand is starred with rainbow light."

Philadelphia, U.S. AN AMERICAN SUBSCRIBER

ANTHROPOLOGY[1]

II.

IN considering the claims of anthropology as a practical means of understanding ourselves, our own thoughts and ways, we have to form an opinion how the ideas and arts of any people are to be accounted for as developed from preceding stages. To work out the lines along which the process of organisation has actually moved, is a task needing caution and reserve. A tribe may have some art which plainly shows progress from a ruder state of things, and yet it may be wrong to suppose this development to have taken place among themselves—it may be an item of higher culture that they have learnt from sight of a more advanced nation. Our own history shows to how small an extent we have been the developers of our own arts and sciences, how largely we have embodied the culture of other nations. It is essential in studying even savage and barbaric culture, to allow for borrowing, so as to clear the lines of real development. When the savage comes into contact with the civilised man, he does not see his way to copy all the high contrivances of this mysterious higher being, but where he thinks he can imitate, he is apt to try, and sometimes succeeds, though oftener fails. After a time of friendly intercourse, the wild man generally learns such substantial secrets of culture as he is in a position to assimilate. Ethnologists have been inclined to look on the wandering Esquimaux of the polar regions as "nature-men," and perhaps no harm has arisen from reasoning on them as such, for they are in many ways fair representatives of the rude nomad hunter and fisher. But I suspect that in some respects they do not show the mere result of the primitive savage working out by slow degrees a somewhat higher culture. Looking at them not as they are now, Europeanised under missionary training, but as they were when Egede and Cranz went out to them from Denmark in the eighteenth century, it seems that their way of life even then had some incidents above the savage level. Their clothing was artistically contrived to resist the intense cold. Its material is sometimes strange to our notions; an undershirt of birds' skins with the feathers inside requires an effort to realise even in our bleakest season. But a leather tunic with sleeves and a hood to pull over the head, a pair of sealskin breeches with leather stockings and boots, form a defence against the cold, at once like that familiar to Europeans, and unlike any unquestionable savage costume, such as the furs which in the Antarctic regions the shivering Fuegians throw over their shoulders. Moreover, all across the polar coast region of the Esquimaux their houses of earth or moulded snow, with compartments like ship-cabins, are warmed and lighted with blubber, burnt in lamps shaped out of potstone with moss to serve as wick, and over these are hung the potstone kettles for their slight cookery. Now,

[1] Two lectures on "Anthropology," delivered on February 15 and 21 at the University Museum, Oxford, by E. B. Tylor, D.C.L., F.R.S. Continued from p. 11.

the kettle carved out of potstone (lapis ollaris) is ancient in Scandinavia, and the plain open dish lamp occurs widely in Northern Europe (it lingered till lately in the Scotch *crusie*). But the lowest races know nothing of so cultured an invention as a lamp. It is of course within the wide bounds of possibility that under the stress of a climate so cold for loose-clad, half-naked men, and where the scanty supply of wood drifted to the shore was too precious for fuel, the Esquimaux, driven by the warlike American Indian tribes of Algonquins and Athabascas, may have discovered how to improve their clothing, and to warm and feed themselves by the aid of lamps, so that they could hold their own against the rigour of polar nature. But if so, how curious that they should have done this by inventing just what the Norsemen could have taught them. Independent Greenland invention, if possible, is hardly probable, and I think a strong case may be made for an easier explanation. We know that the ancestors of the Esquimaux had been in contact with Scandinavians since before our Norman Conquest, when in 1004 the small, sallow, broad-cheeked Skrällings in their skin canoes slew Thorvald with their spears thrown with throwing-sticks, and he was buried with a cross at head and feet at Crossness, which may have been about where long afterwards the Puritan emigrants landed from the *Mayflower*. It seems clear that the Esquimaux had to go north from these delightful regions of New England, but they lived for ages within reach of the Norse settlers in Greenland, whose last survivors in the fourteenth century are thought even to have merged their race in some tribe of the despised Skrällings. Thus it is not surprising that the Scandinavians returning to Greenland after four hundred years more should have found the Esquimaux shaping their skins and furs into semi-European garb, and by the aid of these and the stone lamps and kettles maintaining a polar existence which, without these, would have been difficult indeed. Even that curious Scandinavian institution, the scurrilous nith-songs with which the Norse champions drove one another wild with fury, so that they had to be prohibited by law under heavy penalties, had become a regular Esquimaux custom, and Rink calls them simply *nid-vise*, just as he would have called them among his own Danish forefathers. His first specimen is a Greenland song sung at festive winter gatherings, made to ridicule one Kukouk, who was a poor hunter and fisher, but loved to make friends with the white men; it begins—

> "Wretched little Kukouk
> He takes care of himself;" &c.

If this view of a Scandinavian element in the culture of the Greenlanders is sound, we have the curious spectacle of modern Danes going to civilise the wild men and describing their manners and customs as those of savages, without a thought that some of the most curious of them were relics of forgotten life of their own old Norse-land.

That families can go down in the world is only too well known, nay, that whole tribes and nations can in evil days fall off from their old prosperity, intelligence, and virtue. The question asked by the anthropologist is whether a civilised race can sink to the barbaric level, and thence to the lowest or savage level. The answer is as yet in a confused state, but certain elements of truth may already be got at. It appears at any rate that when civilised men take to a wild life, and mix with the people of the wilderness, they may give rise to a race in whom knowledge and comfort and morality are lowered from the ancestral level. This is the familiar case of the Gauchos of the Pampas, Spaniards in language and partly in race, but leading a life which to the soldiers of Pizarro would have seemed gross and brutal. In the forests of Ceylon there still roam families of wild men, the so-called Veddas or hunters who, as names of places show, once lived widely over the country till dispossessed by the invading nations from India. The wildest Veddas are to be found in the park-like hunting-grounds of Nilgala, and in "the land of Bintan, all covered with mighty woods and filled with abundance of deer," as Robert Knox described it two centuries ago. These wild shy people, of stature averaging under five feet, and in skin duskier than the Singhalese around them, with tiny heads covered with a mass of shaggy hair, and showing in their dull and melancholy faces that uniformity of feature and expression so characteristic of low grades of culture, may seem at first sight to lead a life comparable with that of the forest savages of Brazil or New Guinea. Yet their language is a Singhalese dialect; they are in fact the one known race who may be called savages, and yet speak a language of our own Aryan stock. The following is one of their charms, intended to subdue an elephant in the forest, whom it describes in terms which show a curious transition between the charm and the riddle; indeed, every one who remembers our own nursery riddle about the cow will be struck with its close resemblance to the Vedda charm:—

> "Ichchata vallay
> Pachchata vallay
> Déli devallay
> Situ appá situ."

> "In front a tail,
> Behind a tail,
> On the two sides two tails,
> Stay, beast, stay!"

This is almost Sanskrit, and it is obvious that with so distinctly Aryan language there must needs come some strain of Aryan blood, for it is almost outside the possibilities of social life that a tribe should adopt a language, without such intermarriage with those who speak it that thenceforth the people will in part have ancestry corresponding to the new tongue. The Singhalese indeed hold the Veddas to be of Aryan descent, and in the Mahawanso they stand as offspring of no less an Aryan ancestor than King Vijayo, who married the native princess Kuvéni, and by her means conquered the Island of Ceylon; afterwards, when he ungratefully divorced her and took a daughter of King Pandavo of Madura, the native queen wandered away, and her children married in-and-in, as continued till lately to be Vedda custom. This, says the poem, was the origin of these Pulindá or barbarians; and thus it is that they still claim royal descent, and look down on the Singhalese. Combining the evidence of the Vedda skulls and features with that of their language, we may so far agree with poetic legend as to consider them really outcasts of mixed Aryan and Dravidian or indigenous race. If so, it must be granted that descendants of the Aryan stock, "heirs of all the ages in the foremost files of time," may be found among tiny, shy, wild men of the woods, with sad dull features peering out of their matted locks, who dwell in huts of boughs when they cannot get the preferred shelter of a cave, who live on venison and wild honey and fish drugged by putting poisonous fruit in the pools, and who in their intercourse with the more cultured Singhalese bring themselves into contempt by their simple truthfulness, that utter incapability of cheating and lying which is as characteristic of the savage state as it is rare at higher levels of culture. Truly the condition of these poor relations of ours is of interest. But they are not Aryans on their way upward from primitive rudeness. Their kinsfolk actually till patches of ground and are a settled if a rude people, and these wildest Veddas are evidently a few dwindling clans of outcasts sunk from a higher stage. There is not among them any evidence that they have been in the Stone Age; a story is told of them like our own legend of Wayland Smith's cave, that in old times when they wanted arrows they would carry loads of meat to the smith's shop in the night, and hang it there with a leaf cut to the pattern of the arrow-heads they expected him to leave out in exchange; at any rate it is certain that they have always

had iron by barter from more civilised neighbours, and their occupations, especially the taking of wild honey, are such as belong to the Singhalese.

In the presence of such examples as these, anthropologists admit that civilisation has always had its ups and downs. A nation may themselves develop some thought or art, or borrow it from abroad, and then ages afterwards lose this knowledge or skill because they have no longer the power or leisure to keep it up. It is only after taking such cautionary examples of the migration and degeneration of culture, that it becomes safe to trace the lines along which civilisation has developed in the world.

"When will hearing be like seeing?" says the Persian proverb. Words of description will never give the grasp that the mind takes through actual sight and handling of objects, and this is why in fixing and forming ideas of civilisation, a museum is so necessary. One understands the function of such a museum the better for knowing how the remarkable collection formed by General Pitt-Rivers came into existence. About 1851 its collector, then Colonel Lane Fox, was serving on a military subcommittee to examine improvements in small arms. In those days the British army was still armed (except special riflemen) with the old smooth-bore percussion musket, the well-known "Brown Bess." The improved weapons of Continental armies had brought on the question of reform, but the task of this committee of juniors to press changes on the heads of the service was not an easy one, even when the Duke of Wellington, at last convinced by actual trial at the butts, decreed that he would have every man in the army armed with a rifle musket. Colonel Fox was no mere theorist, but a practical man who knew what to do and how to do it, and his place in the history of the destructive machinery of war is marked by his having been the originator and first instructor of the School of Musketry at Hythe. While engaged in this work of improving weapons, his experience led his thoughts into a new channel. It was forced upon him that stubbornly fixed military habit could not accept progress by leaps and bounds, only by small partial changes, an alteration of the form of the bullet here, then a slight change in the grooving of the barrel, and so on, till a succession of these small changes gradually transformed a weapon of low organisation into a higher one, while the disappearance of the intermediate steps as they were superseded left apparent gaps in the stages of the invention, gaps which those who had followed its actual course knew to have been really filled up by a series of intermediate stages. These stages Colonel Lane Fox collected and arranged in their actual order of development, and thereupon there grew up in his mind the idea that such had been the general course of development of arts among mankind. He set himself to collect weapons and other implements till the walls of his house were covered from cellar to attic with series of spears, boomerangs, bows, and other instruments so grouped as to show the probable history of their development. After a while this expanded far beyond the limits of a private collection, and grew into his Museum. There the student may observe in the actual specimens the transitions by which the parrying-stick used in Australia and elsewhere to ward off spears must have passed into the shield. It is remarkable that one of the forms of shield which lasted on latest into modern times had not passed into a mere screen, but was still, so to speak, fenced with: this was the target carried by the Highland regiments in the Low Countries in 1747. In this museum, again, are shown the series of changes through which the rudest protection of the warrior by the hides of animals led on to elaborate suits of plate and chain armour. The principles which are true of the development of weapons are not less applicable to peaceful instruments, whose history is illustrated in this collection. It is seen how (as was pointed out by the late Carl Engel) the primitive stringed instrument was the hunter's bow, furnished afterwards with a gourd to strengthen the tone by resonance, till at last the hollow resonator came to be formed in the body of the instrument, as in the harp or violin. Thus the hookah or nargileh still keeps something of the shape of the coco-nut shell from which it was originally made and is still called after (Persian, *nárjíl* = coco-nut). But why describe more of these lines of development when the very point of the argument is that verbal description fails to do them justice, and that really to understand them they ought to be followed in the series of actual specimens. All who have been initiated into the principle of development or modified sequence know how admirable a training the study of these tangible things is for the study of other branches of human history, where intermediate stages have more often disappeared, and therefore trained skill and judgment are the more needed to guide the imagination of the student in reconstructing the course along which art and science, morals and government, have moved since they began, and will continue to move in the future.

It is convenient in illustrating intellectual development to choose a branch where every one, so to speak, carries his specimens about with him. Some eighteen years since I made an attempt to describe and analyse the gesture-language, in order to show the consistency of principle with which men debarred from spoken language, whether deaf-mutes or men unacquainted with one another's languages, contrive to utter their own thoughts and understand the thoughts of others through expressive gestures. In these gestures we have a direct and universal outcome of the human mind, a system by which a deaf and dumb scholar from an English asylum can hold converse at first sight with Laplanders or Iroquois or Chinese. They understand each other because they use signs for the most part self-expressive, and conveying their own meaning to those who never saw them before. Now any idea can be thus conveyed by self-expressive signs, not in one way alone but many. A hunter of the prairie, for example, has to express the idea "horse": this he can do by various signs, as by the hand so held as to imitate a horse's head, or by the act of straddling a pair of forked fingers across the edge of the other hand, or by the imitated motion of the gallop; different as these signs are, each tells its own tale. When, however, people have been long used to converse together in gestures, they are apt to cut them down into abbreviated forms which do not show their meaning at first sight, and might even seem to outsiders to be artificial. Thus, a white man, seeing a Cheyenne Indian hold his bent arm forward with the hand closed knuckles upwards, was puzzled as to what this might mean; the Indian, seeing his look of perplexity, took a stick, and bending his head and back, completed the picture into that of a bent old man leaning on a staff, thus showing that the sign meant "old man." Traditional signs may even go on after their reason has passed away, as the sign for "stone," made by hammering with the closed fist on the other hand, a gesture dating from the Stone Age, in which the Indians lived within a few generations, when their only hammer was a stone. These two examples are taken from the recent careful collection of North American gesture-signs by Col. Mallery, published by the Smithsonian Institution. The labour and expense which anthropologists in the United States are now bestowing on the study of the indigenous tribes contrasts, I am sorry to say, with the indifference shown to such observations in Canada, where the habits of yet more interesting native tribes are allowed to die out without even a record. But to return to the gesture-language. This passage of self-expressive signs into what seem arbitrary signs throws strong light on the principles of spoken language, where we find a few self-expressive sounds, such as interjections and names of animals imitated from their cries, while the great majority of words are not even traceable back to the self-expressive

stage from which the analogy of gesture-language leads us to suppose that they originally sprang. Moreover, the sequence or collocation of gesture-signs conforms to fixed rules, which display the action of the thinking mind. The subject must precede the attribute: for instance, such a sequence as a "heavy stick" would have no sense to the sign-maker, who necessarily introduces the stick before he can clothe it with an attribute. Phrases, so to speak, out of an American gesture-story illustrate the gesture-syntax. When the finger-tips of the two hands are brought together to show a hut or wigwam, then pointing to one's own breast does the work of the pronoun, "hut-mine." The sequence "buffalo-one-shot-killed" starts with the idea of buffalo, adds that there was one, and then the sign-maker, having placed the idea of that one buffalo before his interlocutor, can imitatively shoot at it, and it falls dead. He can even imply the idea of causation in the sharp following of the shot by the animal's fall, which makes one the instantaneous consequence of the other. In spoken language the theory of syntax or combining-order is a subject of great complexity and difficulty. Of the few philologists who have attempted it, mention may be made of Steinthal, von der Gabelentz, and Max Müller, whose early dissertation is published as an appendix to Bunsen's "Philosophy of Universal History." But while the age-long shifting history of speech has brought the order of sequence of its elements into an entanglement hardly possible to unravel, we have still before us the first clue in the sequence by which man has arranged his gestures, and will do so anew when he is put to pantomime as a means of converse. Thus the philologist, engaged in studying the formation and combination of speech-sounds or words, may have from the anthropologist the natural rules framed by the human mind dealing in the simplest of known ways with the problem how to express thought.

Scarcely less light is thrown on the working of the human mind by the history of that special development of error which since the remotest ages has taken the form of magic. Of late certain events in France have revived popular interest in that curious old-fashioned instrument, the divining-rod, and as I happened to be staying at a friend's house in the Mendip district, where it is still used by well-sinkers and miners, at my request a regular practitioner was sent for. I show the instrument—a forked hazel twig, which is held loosely by its outward-bent ends in the closed upturned hands, so that it can rise or fall easily. On approaching a spring, vein of ore, &c., the rod dips toward it, but when replaced horizontally and passing over the place, it rises toward the bearer's face. That the spring or other object sought has really no effect on the instrument, but that its dipping has to do with the seeker, is sufficiently shown by its being considered to act with the most dissimilar objects—a spring of water, a vein of ore, a piece of metal, a dead body—which have, however, this in common, that they are what the "dowser" is in search of. It does not appear that he fraudulently moves the rod, but my sensations led me to agree with Chevreul that the slight movements of the hands are unconsciously guided to accumulate into impulses sufficient to cause the twig to dip or rise. I noticed that when I could allow my attention to stray, the rod would from time to time move in my hands in a way so lifelike that an uneducated person might well suppose the movement to be spontaneous. It is hardly necessary to say that the rod always moves where the bearer's mind suggests an object. In the present case the special business of the dowser was to find springs of water, and his difficulty was to distinguish between the mere *top springs*, which though acting on the rod were of course practically worthless, and the valuable *main springs* which would repay the sinking of a well. In the trial an incident occurred which threw light on the nature of the whole operation. The rod when brought over my watch, dipped strongly, and the dowser looking up at me with innocent archness said: "You see, sir, it's just over the *mainspring* of your watch." The remark showed how his mind was so simply controlled by association of ideas, that he expected the same action from a *main spring* of water and of a watch, their likeness of name quite overriding their unlikeness of nature. Nothing could have better shown at once the man's sincerity and the purely ideal character of his craft, nor does one often meet with a more perfect illustration of the state of mind where magic has its origin in delusive analogy, whether of things or of their names. Magic has often passed as mystery, but to the anthropologist few arts are less mysterious; he reads by childishly simple association of ideas the open secret of half the magical rules which prevail in savage and barbaric life, and even last on into the midst of civilisation. In the wild north-west of Ireland I learnt not long since the use of the "worm-knot" for curing ailments of cattle; it is a bit of cord in which a peculiar slip-knot is made, and if this knot when pulled over the creature's back comes away clear (as shown), the disease will come away too. On the same principle, the purpose of the pig's heart stuck full of thorns and bricked up in an old chimney (produced) was sympathetically to pierce with pain and shrivel with disease some hated person, probably a reputed witch suspected of "overlooking." It is a curious exercise to read from this point of view the precepts of the modern astrologer, which still show their quasi-reasons, futile but quite intelligible. Suppose one's self seeking for lost property, the significator of the thing missing will be the moon, apparently because herself so often lost and found again. According to her position, east or west, the object must be looked for; if the Moon is in a human sign, as the Twins, it will be in a human habitation, but the sign of the Bull indicates its being in a cow-house. Even the thief's clothes are denoted by the governing planets; under Saturn he will be found in a black suit, or if Mars is in it, his presence will be shown by some red article, perhaps a neck-handkerchief. Folly as this is, it at any rate shows the working of uneducated men's minds, where the argument from analogy appears in its early crude state, not yet cleared by observation, but still on its way to become, under proper checks and reservations, the explorer of the universe and the guide to science.

This is by no means the only example of a delusive theory being, when honestly worked out, productive of scientific truth. In times when the study of races for mere knowledge sake had little hold on scholars' minds, anthropology was much indebted to the fancy that any people whose presence in an outlying region seemed hard to account for must be the "lost tribes of Israel." One nation answered the conditions of this theory about as well as another: the remnants of the ten tribes were found marauding in the Afghan passes, wandering with the reindeer in Lapland, chasing buffaloes on the American prairies, or slaughtering human victims on the teocallis of Mexico. The manners and customs of these countries being studied, showed distinct analogies with Jewish customs, as indeed they would have with German or Chinese, or any other. Enthusiasts such as Rudbeck, or Garcia, or Adair, of course did not see this, but the practical result was that, especially in North and South America, evidence of great value in the history of civilisation was recorded which would have perished had it not been thus caught before it was swept away by European influence. This is a good instance of its being better to have a bad hypothesis than none at all. The ten tribes delusion has now, however, sunk to a lower level than when Lord Kingsborough spent his fortune in publishing the Mexican pictures and chronicles. But in spite of all the new real knowledge as to races, it has even in this country more

votaries than ever. On opening, the other day, a book of the curious "Anglo-Israelite" sect, I met with the following passage, written in evident seriousness by a seeker after proofs that the British nation are the Lost Tribes of Israel:—"I am even now acquainted with many words in current use in some parts of the West of England that were in common use by Israel of old, and that I have not found in use in any other country—such as goad, gourd, barm, leaven, comrade, lattice, chambering, flay, score, gallon, cruse, lintel, latchet, girdle, pitcher, platter, glean, &c., &c." It takes a little thought to understand the full depth of ignorance of a man who, finding these words in the English Bible, thought they were used by the ancient Israelites. Why I ask you to notice it is that the author of the volume it is printed in says that 100,000 copies of his work have been sold; there is, indeed, no doubt but that this abject nonsense has a far larger circulation than all the rational ethnology published in England. It opens a window by which we can see into the state of education of its readers, who mainly belong to the lower middle class, and whose thousands of schools are as yet unvisited by the University Delegates on the one hand or the Education Department on the other. Even our Public Elementary education, good as it is in many respects, passes some questionable anthropology. Happening to look a few days ago at a Third Standard book on English History, I was surprised to find a picture of a South Sea Islander, tattooed all over, standing to represent the condition of the ancient Britons, who are described as savages. Now this is hardly an appropriate designation for a pastoral and agricultural people, who had a gold coinage, and whose war-chariots even the Roman legionaries found troublesome to deal with.

Having now attempted to support the claims of the problems of human nature to fuller recognition in our system of advanced education, it may be well to observe by way of caution that anthropology, while contributing materials to other sciences, does not dictate the conclusions which each science is to draw from them. It has not a rule of morals, a system of politics, or a doctrine of religion to teach, only a series of facts showing the stages through which each of these has been developed, and with these the counsel that the anthropological way of studying human conduct is to trace its principles along the historical line of their change and progress. Anthropology, though acknowledging degeneration as an important factor in human life, gives no encouragement to pessimist theories of society. The clinging to life by savage and civilised alike is a measure of their judgment that with all its ills it is a substantial good, to be valued and defended. That the tendency of mankind is toward industrial progress need not be proved, for it is not denied. That moral progress is on the same footing rests on the main fact that man obtains the happiness he seeks, not only through his own sensations but by sympathy with the enjoyments of others; now beings whose interests are thus consonant with the prosperity of those around them are plainly on the road to good rather than evil. At the same time facts constantly presenting themselves in anthropology guard the student from a prejudiced optimism. He has the picture constantly before him of low-cultured but kindly and truthful tribes of favoured climates, into whose midst the march of civilisation is bringing the beginnings of trade and wealth, and with them temptations to selfishness and dishonesty. At every step in the advance towards prosperity he sees, accompanying the growth of knowledge and the raising of the social standard, a series of concomitant evils, the break-up of the old stage, the failure to assimilate the new. Often a dispiriting lesson, this is yet of the highest practical value, for it elucidates what the statesman should be ever striving to learn, how, in the remodelling of institutions, to gain the utmost advantage while minimising the accompanying loss.

To conclude: my explanation of the unsymmetrical way in which I have here put forward the cause of anthropology must be that the necessity of the case compelled me to a certain scrappiness of treatment. For presenting my subject thus in shreds and patches I am tempted to apologise in that well-worn lecturer's jest, the story of the man who had a house to sell and carried about a brick as a specimen. Perhaps, however, there may be more of a moral in this story than is commonly supposed. I cannot help fancying that the flippant Greek who first told it had actually seen something of the kind done in sober earnest. He may have watched some grave Roman going down to the prætor's court carrying a tile in his hands, which in the lawsuit was to be the legal symbol of the house itself, just as a farm would be represented by a sod of its turf, or as one of our Teutonic forefathers, living in a wooden house, would transfer it by handing over a chip from the doorpost. This indeed is the very position in which I find myself placed in undertaking to treat of anthropology in two lectures. Because the whole structure is too extensive and too massive to bring into court, I have been obliged to symbolise it by fragments taken here and there, and can only ask that these be accepted as symbols, placing the edifice they represent under the guardianship of the University of Oxford.

THE ARCTIC METEOROLOGICAL STATION ON THE LENA

THE last number of the *Izvestia* of the East-Siberian branch of the Russian Geographical Society gives further news of the Lena Arctic Meteorological Station, dated October 24, 1882. This news was brought by the American officers, Messrs. Garber and Schütze, who left the station on October 25, and reached Yakutsk on November 29. Mr. Schütze made a sketch of the station, which appeared in the *Izvestia*, and which we reproduce. The house brought from Yakutsk proved to be comfortable and warm. It has been erected at the Sagastyr arm of the Lena, on Sagastyr Island (in 73° 22′ 30″ N. lat., and 144° 14′ 46″ E. long.); the name of this island is very significant: it means "it blows away." Galleries of planks have been erected behind the house to connect it with four pavilions for instruments. Besides the coal that has been taken from Yakutsk, the station has a good supply of fuel in the driftwood scattered around the station. The Sagastyr arm of the Lena supplies the inhabitants of the station with fresh fish. The health of all the members is satisfactory. Dr. Bunge received a contusion to a rib during the journey, but he is now well, and is besieged by indigenes, who come to him for medical help. Several Tungus families stay at one and two miles' distance from the station, and they are on the best terms with the meteorologists. The temperature is very low and, as there is no snow, the prospects are not very brilliant. The soil is frozen to a great depth, and cracks; the rivers and lakes are covered with a thick sheet of ice, so that the water beneath the ice is shallow, and the fish are in want of air to breathe. The food for the reindeer is frozen. Even at Yakutsk there was but one inch of snow on December 16, and a great inundation is expected for the spring, as well as epidemics, which are said usually to follow inundations.

As to the journey from Bulun to Sagastyr, it was performed not without difficulties. On August 6 a fresh west wind compelled the boats to stop on unfavourable ground, and the wind blowing with increasing force, it soon turned out a strong storm, blowing from north-west on August 9. The boats were thrown aground close by the banks of the river, and filled with water. The waves rolled above their decks. The chief instruments were, however, safe, as they were landed in time. On August 19 the expedition reached the Ketakh settlement, seven

miles south of the signal usually drawn on the maps of the delta of the Lena. Three days later the expedition chose Sagastyr Island as the place for the station, and began to erect the building and arrange the instruments.

Russian Meteorological Station at Sagastyr, at the mouth of the Lena, 73° 22′ 30″ N. lat., 144° 14′ 46″ E. long.

The house of the station is obviously very small; and when looking at Mr. Schütze's sketch of this small building, lost amidst the snow-plain, far from any communication with the civilised world, one cannot but admire the devotion of those who have willingly submitted to remain in these inhospitable latitudes for scientific purposes.

THE AURORA BOREALIS

AMONG the numerous varieties of the aurora borealis or northern light, there is one of particular interest as regards the determination of the origin of this phenomenon. This variety, which was observed and reported upon in 1868 by the members of the Swedish Polar Expedition, takes the form of tiny flames or a phosphorescent luminosity, appearing during the winter months in the Polar regions, around projecting objects, viz. mountain cones and ridges. This phenomenon is so prominent that one need not be a scientist to discover it, and it was observed by our well-known philologist, Herr M. A. Castrén, during his journeys in Siberia. Herr Castrén's descriptions of the phenomenon are very minute, and exactly in accordance with its usual appearances, but his observations were, however, not known to me in 1868, and it was only on the return of the expedition that I heard of them. The observations made by the Swedish expedition at Spitzbergen led the Finnish Society of Science in 1871 to despatch an expedition, of which I was a member, to Lapland to ascertain if such a phenomenon could not be called forth, or at all events magnified, by mere mechanical appliances. And assuming that the aurora borealis in general, and the variety of the same just mentioned in particular, is caused by electric currents in the atmosphere, an apparatus of the following nature was erected on Luosmavaara, a mountain-top about 520 feet above the surface of the Lake Enare, in Lapland. It consisted of a number of fine points of copper wire laid out in the shape of a wreath two square metres in area, and connected by a circular wire of the same metal. This wreath was attached to a long pole, from the top of which a single copper wire (0·4 mm. in diameter) led to a galvanometer fixed in a room in the Enare vicarage, some two miles distant east, and from the galvanometer another copper wire led to a disk of platina in the earth.

When this circuit was closed, the galvanometer gave a deflexion, although faint. But on the very same night the apparatus was erected, viz. November 22, 1871, *there appeared an aurora*, which began with a single perpendicular column of light *above the top of the Luosmavaara!* This column was analysed with the spectroscope, and gave the usual yellow-green line, but whether the column was *on* or *behind* the mountain-top could unfortunately not be clearly ascertained. That it, however, had its origin from the apparatus described above appears to me, after the researches were made which I am about to detail, to be beyond a doubt. At the same time, on November 22, 1871, it was observed, when studying the spectrum of the flames which, on that day appeared around the mountain-tops more distinctly than usual, that the characteristic yellow-green line in the spectroscope was returned from nearly every object, as, for instance, the ice of a pond, the roof of a shed, and even, though faintly, from the snow in the immediate vicinity of the observatory. These observations led me to believe that I was within a sphere of electric discharge, whose radius extended considerably around the station.

This interpretation of mine has, however, not been generally accepted by students of the phenomenon, who, on the other hand, have explained the appearance as being an aurora reflected from the earth; but that this theory is erroneous will be clearly demonstrated by the researches detailed below.

In Baron Nordenskjöld's exhaustive investigations of the aurora borealis during the *Vega* expedition, he states that he was unable to discover this phosphorescent phenomenon which I have observed, and that he had noticed in the spectroscope, in connection with the same, a faint band near the line D; but this has nothing whatever to do with the auroral line. In order, however, to make it clear that I have not confused these two lines, I may state that already in 1871 I observed the absorption-band in question, as will be seen from my work at the time on the aurora borealis and the auroral spectrum. In these researches I determined the wave-length of this line, and as the latter is only apparent in daylight or moonshine, while my observations were without exception made in the dark, it is perfectly clear that this line or band has nothing in common with the auroral one. The two lines are, in fact, of such a different character that they cannot

be confused for a moment by any one who has had an opportunity of comparing them simultaneously.

During the period which has elapsed since 1871 my efforts have been bent on the closest study of the aurora borealis, and an accidental discovery that a Geissler tube may produce certain forms of light without being in direct connection with either pole of an electric battery further stimulated my attention. This led to the result that the electric current emanating from the pole of an electric apparatus, while the other is connected with the earth, can be made to traverse a layer of air of *ordinary* density without producing any light at all; but when, on the other hand, it encounters a layer of *very thin* air, the luminous phenomenon will at once appear. These researches led me to construct an auroral apparatus for demonstrating this phenomenon as it appears in nature. The knowledge I have gained of the aurora during my continued labours and the above-mentioned observations in particular, made me conclude that an attempt to produce the auroral phenomenon in the very lap of nature by aiding the action of her own forces, ought to give important results and also originate a method for the future study of the phenomenon. In consequence of this idea I made the proposition to the International Polar Congress which I attended at St. Petersburg in 1881, to erect a station at Sodankylä to follow up my researches, although the results might perhaps be of negative value.

The Experiments on Oratunturi.—For the purpose of carrying out the experiment suggested above, the Finnish Polar Expedition was comparatively well equipped, the Physical Faculty of the University at Helsingfors having contributed instruments as well as wire; but the former had to be slightly altered for the research in view. Circumstances, however, occurred which prevented my work being commenced until the end of November, 1882; and after having examined the country and made some preliminary experiments from the steeple of Sodankylä Church, situated close to our observatory, I determined to erect the apparatus on the summit of the Oratunturi Mountain, some twelve miles from the observatory. The top was well suited for the purpose, although surrounded by rising copses of wood, which should, according to theory, be rather detrimental to such experiments. The summit was determined by barometrical measurements to be 1070 feet above the town of Sodankylä. To lead the wire from Oratunturi to Sodankylä was, in consequence of the great distance, impossible, as there were neither wires nor insulators sufficient, and I had therefore to carry out the experiments as follows:—

On the summit of Oratunturi (lat. 67° 21′, long. 27° 17′·3 E. of G.), about 540 metres above sea-level, I laid out the instrument which I have named an "utströmnings" apparatus, *i.e.* a "discharging" apparatus. It consisted of a bare copper wire 2 mm. in diameter, fitted at every half metre with points soldered thereon. The copper wire was laid out in entwined squares, while the points were raised on poles 2½ metres high, provided with insulators, so that each inner coil was about 1·5 m. from the outer one. The apparatus covered a surface area of 900 square metres. From the inner end of this wire an insulated copper wire on poles with telegraph insulators led to the foot of the mountain, where a hut of pine branches was erected; here the wire was connected with a galvanometer, and from this another wire led to a zinc disk in the "earth," *i.e.* in a flowing spring. The elevation of the apparatus above the zinc disk was 180 metres, and the direction of the conducting wire from the mountain north-west to south-east.

From the first day the apparatus was finished, viz. December 5, there appeared almost *every* night a yellow-white luminosity around the summit of the mountain, *while no such luminosity was seen around any one of the others!* The flames were very variable in intensity, and in constant oscillation as those of a liquid fire. Three times it was tested, 2½ miles off in south-east, by a Wrede spectroscope (small size with two prisms), and it returned a faintly continuous spectrum from D to F, in which the auroral line $\lambda = 5569$ with soft variable intensity was observed. The galvanometer steadily gave the deflexions of a positive current from the "utströmnings" apparatus to the earth. The deflexion was so exceedingly variable that the needles were in constant motion when the circuit was closed. A Leclanché's element of ordinary size gave a deflexion which varied according as the positive pole was turned against the mountain or against the earth, but it was always measurable.

It was, however, impossible to continue the observations for any length of time, as hoar-frost quickly developed on the wires and the poles in large quantities, whereby the insulation became affected. The conducting wire from the mountain was of copper insulated with waxed cotton, and 0·8 mm. in diameter. This became so covered with hoar-frost that it broke under the weight in spite of the short distance between the supporting poles, viz. 25 metres. It was, therefore, necessary to examine its entire length before the experiments could be commenced, and as the temperature, as a rule, was under −30° C., our work was greatly impeded.

Although the deflexions, by their great variation in the electric current and perhaps, from changes in the electric forces or the imperfect insulation, cannot be of great scientific value, I subjoin them:—

December 13, 1882.—After several alterations of the galvanometer, and the right sensitiveness having been obtained, the result was, with open circuit: scale-reading[1] of position of equilibrium (*a*) 361·5; (*b*) 362·7; with closed circuit (*c*) 457·8 [or 95·8 parts of the metre = 3°·5, every part of the metre being 2′·2]. This deflexion constantly varied, and often descended to 30 parts of the metre, to rise suddenly again. The temperature was that day comparatively high, viz. −10° to −12° C. The air was hazy.

December 19, 1882.—Plane of equilibrium, 468·0; with closed circuit, 471·6; deflexion = 3·6 parts of the metre.

A Leclanché's element placed in the circuit gave—

1. Plane of equilibrium with open circuit ... ... 469·4
2. With the carbon pole against the earth ... ... 476·4
3. With the carbon pole against the mountain ... ... 467·9

Or the current from the atmosphere = 2·75 parts of the meter.
And the current from the element = 4·25 ,, ,,

The insulation was here worse than in the previous case, and the temperature very low, viz. −35° C.

From December 27 to 29 several experiments were also made, under which the deflexions of the current were greater or less than under the above.

With regard to the construction of the galvanometer it may be mentioned that the instrument consisted of an ordinary wooden frame constructed for the coiling of the wire, while a pair of astatic needles with a mirror could freely swing within the frame thus: one within and the other above the coil. At first the frame was coiled with copper wire 0·5 mm., insulated with indiarubber, but during the experiments this wire was replaced by another 0·4 mm., and insulated with silk impregnated with stearine. The arrangement of the needles was altered several times, but under the above-recorded experiments they were hung on two fine threads of cocoon silk about 20 cm. long and about 1·5 mm. apart. The deflexions were read with a tube and scale from a distance of 0·8 metre.

The Experiments at Pietarintunturi.—On this mountain (lat. 68° 32′·5, long. 27° 17′·3 E. of G.), 950 metres above the sea, a smaller "utströmnings" apparatus was erected in two parts, so that the inner one covered about 80 and the outer 320 square metres. In other respects it

[1] These figures refer to the reading of a divided metre-scale viewed by reflexion in the mirror of the galvanometer by the "subjective" method through a reading-telescope.—[Ed.]

was similar to the one at Oratunturi, but the galvanometer here was not so sensitive, as the needles had lost some of their astatic quality through the journey.

On December 29 there appeared above this apparatus *a single column of aurora some* 120 *metres in height!* Its plane was determined from several points, viz., S.S.W. to E.S.E., within an angle of 90° with the plane of the horizon, and there was not the slightest doubt of the aurora appearing just *above* the apparatus. No spectrum analysis was, however, made, as the question of the greatest moment on this occasion was to determine the plane of the column in relation to the apparatus. A faint aurora in the sky at the back of the mountain might perhaps have influenced the analysis; besides which another difficulty arose from the temperature being − 35°, and the instrument in consequence difficult to handle. But that a spectrum similar to the one at Oratunturi and the one of 1871 on the Luosmavaara would have been received I have not the slightest doubt.

The galvanic measurements were effected as follows:—First, the galvanometer was connected with the inner coil of the "utströmnings" apparatus, 80 square metres in area, when deflexion was observed; secondly, it was connected with the outer coil, 320 square metres in area; and thirdly, with both apparatuses combined, or 400 square metres in area.

The deflexions were, in consequence of the small sensitiveness of the galvanometer, weak; but it was clearly noticed that the current increased with the area of the apparatus. As the deflexions varied considerably, it was impossible to ascertain exactly their natural law, but it seemed apparent that the strength of the current was *proportionate to the surface area of the apparatus laid out.* The galvanometer used in this case was similar to the other, but not so sensitive. If I designate the inclinations at Oratunturi with O and those of Pietarintunturi with P, the relative proportion of the sensitiveness of the galvanometers would be

$$P = 0{\cdot}370,$$

with very nearly the resistance of 20 Siemens units. The metre was here divided into 1''·2. The following shows the results of the experiments:—

December 29, 1882.—With the inner coil, or an area of 80 square metres:—

First deflexion,	2·1	parts of the metre.
Second „	0·9	„ „
Third „	1·0	„ „
Fourth „	2·4	„ „
Fifth „	2·0	„ „

The temperature was on that day, as almost during the whole period of experimenting here, − 30°, or under. The air was clear.

December 31, 1882.—With the whole surface of the apparatus, or an area of 400 square metres—

First deflexion,	1·6	parts of the metre.
Second „	2·8	„ „
Third „	3·8	„ „
Fourth „	3·1	„ „
Fifth „	3·1	„ „

With a Leclanché's element the result was:—

1. With the carbon pole against the mountain deflexion = − 0·2
   With the zinc „ „ „ + 7·5
   Of which deflexion by the current from the atmosphere = 3·65
   And „ „ „ Leclanché's element = 3·85
2. With the carbon pole against the mountain deflexion = − 1·5
   With the zinc „ „ „ = 11·9
   Of which deflexion by the current from the atmosphere = 5·2
   And „ „ „ Leclanché's element = 6·7

The experiments with the different parts of the apparatus resulted as follows:—

1. Plane of equilibrium (*a*) 271·6, (*b*) 271·4 } = 271·5 open circuit.

Inner apparatus A = 80 square metres.

(*a'*) 271·5, (*b'*) 271·7 } = 271·6 closed circuit.

Plane of equilibrium (*c*) = 270·4 open circuit.

Deflexion from A = 0·6.

Outer apparatus B = 320 square metres.

(*a*) Plane of equilibrium = 270·4 open circuit.

(*a'*) „ „ 271·8, (*b'*) „ „ 271·1 } = 271·5 closed circuit.

(*b*) „ „ = 270·1 closed circuit.

Deflexion from B = 1·2.

Entire apparatus C = 400 square metres.

(*a*) Plane of equilibrium = 270·1 open circuit.

(*a'*) „ „ 270·9, (*b'*) „ „ 270·7 } = 270·8 closed circuit.

(*b*) „ „ = 269·7 open circuit.

Deflexion for C = 0·9.

During these experiments it was noticed that the first deflexion was always far greater than the subsequent double, constant ones, and I have therefore noted this one only. When the circuit was closed it was observed that the needle pair always was at rest in the plane of equilibrium.

2. A. Plane of equilibrium 269·7 open circuit.
   (*a'*) „ „ 270·8 closed circuit, 1st defl. 277·0
   (*b*) „ „ 269·8 open circuit.
   Deflexion for A = 1·0 ... ... 1st defl. = 7·2
   B. (*a*) Plane of equilibrium 269·8 open circuit.
   (*a'*) „ „ 271·3 closed circuit, 1st defl 281·0
   (*b*) „ „ 270·4 open circuit.
   Deflexion for B = 1·2 ... ... 1st defl. = 11·8
   C. (*a*) Plane of equilibrium 270·4 open circuit.
   (*a'*) „ „ 271·8 closed circuit, 1st defl. 284·9
   (*b*) „ „ 270·4 open circuit.
   Deflexion for C = 1·4 ... ... 1st defl. = 14·5
3. A. (*a*) Plane of equilibrium 270·4 open circuit.
   (*a'*) „ „ 271·7 closed circuit, 1st defl. 278·2
   (*b*) „ „ 270·8 open circuit.
   Deflexion for A = 1·1 ... ... 1st defl. = 7·6
   B. (*a*) Plane of equilibrium 270·8 open circuit.
   (*a'*) „ „ 274·0 closed circuit, 1st defl. 282·0
   (*b*) „ „ 273·6 open circuit.
   Deflexion for B = 1·8 ... ... 1st defl. = 10·8
   C. (*a*) Plane of equilibrium 274·5 open circuit.
   (*a'*) „ „ 279·0 closed circuit, 1st defl. 292·3
   (*b*) „ „ 275·6 open circuit.
   Deflexion for C = 3·9 ... ... 1st defl. = 17·2

If these observations are connected we obtain:—

A = 80 square metres
B = 320 „
C = 400 „

	Deflexion.	1st. defl.	Deflexion.	1st defl.	Deflexion.	1st defl.
1.	0·6	—	1·2	—	0·9	—
2.	1·0	7·2	1·2	11·8	1·4	14·5
3.	1·1	7·6	1·8	10·8	3·9	17·2

If it now be considered that the current from the atmosphere was very variable, besides that the insulation was rather imperfect, the results of these experiments are not so reliable as might be desired. Still, this inference can safely be drawn from them, that the strength of the current *increases* with the area of the "utströmnings" apparatus or with the number of points, but the natural law of the increase I have not yet been able to determine. As will be seen from the subjoined figures the conductive power of the wire was that day very small, although the temperature was comparatively high, viz. at 1 a.m. − 18° C., but falling.

The entire apparatus (C) with one of Leclanché's elements:—

(*a*) Plane of equilibrium 273·1 open circuit.
(*a'*) „ „ 275·3 the zinc pole against the mountain.
(*b'*) „ „ 270·4 carbon „ „
(*b*) „ „ 270·1 closed circuit.

From this we obtain: the current from the atmosphere = 1·5, and from the element = 2·45, which figures show

the small conducting power of the wire, as well as the weak current from the atmosphere.

Another inference may be drawn from these observations. If we take the average of the results on December 31 on Pietarintunturi, when the whole apparatus was used, it will be 3·2 parts of the metre, and comparing this with that of December 19 on Oratunturi, when the atmospheric conditions were similar, which was 3·6 parts of the metre, and transform these into minutes, the result will be as follows: 3·6 parts of the metre at 2′·2 = 7·92; 3·2 parts of the metre at 1′·2 = 3′·84, but the sensitiveness of the galvanometer at Pietarintunturi was only 0·36 of that at Oratunturi = 0·37, and the area of the apparatus in the former 200 square metres against 900 in the latter, and further, assuming that the current increases in proportion to the area of the apparatus, we shall have:

$$\frac{3'{\cdot}84}{0{\cdot}36} \; \frac{900}{400} = 24'{\cdot}0.$$

And the deflexion 3′·84 being reduced to the same galvanometer sensitiveness and the same area of apparatus, the actual result is that the experiments at Oratunturi showed a deflexion *three times* greater than those at Pietarintunturi. The latter place is certainly situated a little higher than the former, but in my opinion the increase of the electric force lies in the fact that Oratunturi is in a higher latitude than Pietarintunturi, *i.e.* nearer the plane of the aurora borealis. Although the experiments recorded above suffer from inaccuracy on account of the imperfect insulation, I have come to the conclusion *that the electric current from the atmosphere increases rapidly with the latitude.*

The great deflexion which I obtained at Oratunturi on December 13, 1882, I do not consider refutes this inference, as the atmospheric conditions on this occasion were exceptional, viz. the temperature high and the air hazy.

The experiments in both places have, however, unfortunately been of a somewhat provisional character, which is due to the external impediments in our way. Thus, when experimenting at Oratunturi, the writer had to make a journey in the snow of 20 kilometres, viz. of four hours' duration, then to examine the apparatus on the summit, clean it from hoar-frost, and often repair it, with the thermometer at − 30° C. Then only could the experiments be commenced. It was only possible to work for five to eight minutes at a time, as it was necessary to thaw one's benumbed hands before a bonfire lit on the snow. At Pietarintunturi the road was certainly shorter, but, nevertheless, very fatiguing, as it was necessary first to climb a ridge about 1000 feet, and then journey about 3 kilometres.

These difficulties, and chiefly the imperfect insulations and the weakness of the wires at my disposal, compelled me to abandon experiments of this character.

SELIM LEMSTRÖM
Professor of the Helsingfors University

(*To be continued*)

NOTES

BESIDES Prof. Huxley the following English men of science have been elected Foreign Associates of the U.S. National Academy of Sciences:—Prof. J. C. Adams, Prof. Cayley, Prof. Sylvester, Prof. Stokes, Sir William Thomson, and Sir J. D. Hooker.

MR. HERBERT SPENCER has been elected a Corresponding Member of the Paris Academy of Moral and Political Sciences.

THE remarkable enthusiasm with which the project of the memorial to Charles Darwin was received in Sweden has already been noticed in our columns. The amount of the subscriptions collected, as was said, from all ranks, has just been received by the treasurer of the Darwin Memorial Fund. It is a sum of 382*l.* 12*s.* 6*d.*, the largest, we believe, that has been contributed by any foreign country, and a proof of the zeal on behalf of science that exists in the land of Linnæus.

WE are glad to learn that America has at length subscribed for a table at the Zoological Station at Naples. In view of the very considerable number of American students in European biological laboratories some surprise has naturally been felt that America has not hitherto been represented at Naples. President Carter and the trustees of Williams College are to be congratulated on having taken the lead in a matter the importance of which must be apparent to all who are interested in the progress of morphological study.

DR. WILLIAM CHAMBERS, the head of the eminent publishing firm, well deserves the honour of a baronetcy which he has just received, on account of the public services rendered by him to education and to social improvement throughout a long life; he is just the age of the century, we believe.

The public sale of the late Prof. J. Decaisne's library will take place in Paris from June 4 to 23 next. The catalogue of 480 pages, published by Labitte, of Paris, contains more than 5000 entries, classified according to subject by M. Vesque, assistant to the late M. Decaisne. It is probably one of the finest libraries in botany, horticulture, and general natural history which has been sold since the death of Jussieu. The catalogue contains a portrait of Decaisne and a biography by Dr. E. Bornet.

THE death is announced, at the age of seventy-one years, of Mr. James Young of Kellie, the "Sir Paraffin" of his old friend Livingstone. Mr. Young is best known in connection with his process of distillation of paraffin oil from bituminous coal, which attained great dimensions, and from which he realised a fortune. Mr. Young took a real and active interest in chemical research, and founded the Chair of Economic Chemistry in Anderson's University, Glasgow; he was a Fellow of the Royal Society.

THE departure of the Swedish Expedition to Greenland has been postponed to the 22nd, and Baron Nordenskjöld will join the *Sofia* at Gothenburg, instead of coming to Scotland.

A HYGIENIC exhibition was opened at Berlin on Saturday.

THE Society of Arts *conversazione* will this year be held in the buildings of the International Fisheries Exhibition; the Prince of Wales, the President of the Society, has intimated his intention of being present. The date is not yet announced.

IN connection with the recent discussion on the opening of picture galleries and museums on Sundays, the following facts relating to the Whitechapel Fine Art Exhibition are full of interest. This exhibition, which as may not be known to all our readers, is one which is open for thirteen days at Easter in one of the most desolate parts of this great metropolis. It consists of about two hundred pictures of the highest order of merit, which are placed at the disposal of a responsible committee by the artists or those who are fortunate enough to possess them. It is open gratuitously from ten in the morning until ten at night, except on Sundays, when the opening takes place at two o'clock, after morning service. This year, it will be seen from the numbers we give below, that no less than 34,644 of the poorest of the poor visited the pictures; and as they were to a very large extent "personally conducted" round the rooms by ladies and gentlemen who freely gave up their time to the work, the way in which they appreciated the pictures is thoroughly well known. The same men and women came again and again, bringing their friends to show them the pictures in which they themselves had taken the greatest interest. One of the most important points that we wish to urge now is, that on the last day the exhibition was open, which

was Sunday, between the hours of two and ten more than 3000 working men and women visited the collection, and we are informed that when the rooms were most crowded, there was always not only absolute order and good temper, but a reverence for the spirit of the place. This, we think, is a sufficient reply to those who say that if picture galleries and museums were opened on Sunday, they would not be visited. Seeing that a love of science and nature must be at the bottom of all true love for art, we feel ourselves bound to thank Mr. Barnett and those who have helped him in this humanising work; and as it is known with what sympathy artists and possessors of pictures placed them at the disposal of the committee, we think it a pity that the Whitechapel example is not more generally followed. It is not necessary to give the numbers for 1882, but we may just say that very nearly 10,000 more people visited the exhibition this year, which clearly shows that the interest taken in it is not a transient one, but one which increases from year to year. And the figures do not do justice to the success of the exhibition, for they mean something more than they would at an exhibition in the West End; the Whitechapel people went to see, and they made a business of seeing. The attendances were as follows:—

Tuesday	March 20	933
Wednesday	,, 21	2,094
Thursday	,, 22	1,431
Good Friday	,, 23	2,722
Saturday	,, 24	2,581
Easter Sunday	,, 25	1,632
,, Monday	,, 26	3 369
,, Tuesday	,, 27	3,304
Wednesday	,, 28	3,523
Thursday	,, 29	3,212
Friday	,, 30	2,681
Saturday	,, 31	4,045
Sunday	April 1	3,117
	Total for 13 days	34,644

AT the first meeting of the Sociological Section of the Birmingham Natural History and Microscopical Society for the study of Mr. Herbert Spencer's "System of Philosophy" held last week at the Mason College, the President (Mr. W. R. Hughes) explained that the new Section had originated in a wish to unite, for the purposes of mutual help, those who were already students of Mr. Herbert Spencer's system, but were unknown to each other; and to introduce to the synthetic philosophy those already engaged in some special biological study, but as yet unfamiliar with the principles common to all departments of natural history. He read a letter from Mr. Herbert Spencer expressing cordial sympathy with the objects of the Section, and adding some valuable suggestions as to the course of work to be undertaken by the Section. Whether we admitted or rejected Mr. Spencer's principles, the President said, there was no doubt of the wonderful influence they were exercising in this country, on the Continent, and in America. He enumerated many reasons why Birmingham was peculiarly adapted for the study of sociology, saying it was central, healthy, industrious, and earnest in all it undertook, active in reform, versatile in its trades, and therefore free from commercial panics, many-sided in religion, untiring in political activity. During the present century no town had exhibited a more remarkable social development, and therefore there was no town more fit for the study of sociology. Its development was of a type peculiar to a large industrial organisation, and was in marked contrast to that kind of development which would obtain under a military or ecclesiastical or agricultural organisation. Sociological generalisations made there might therefore be regarded as typical and unique. The President's address was followed by a discussion upon the first two chapters of the "Essay on Education."

MR. CLEMENT L. WRAGGE has undertaken to reorganise he meteorological work at the Ben Nevis Observatory, which he first commenced about two years ago, under the auspices of the Scottish Meteorological Society, and hopes to have the observing system reopened and in order by June 1. Mr. William Whyte, of Fort William (formerly assistant), will then receive further instruction from Mr. Wragge, and will take charge, having been appointed by the Society to carry on the work during the summer of the present year, in consequence of Mr. Wragge's intention to resume his travels in the course of a few months, and to revisit Australia. The voyage will be made a scientific one, and Mr. Wragge hopes to add largely to his natural history and ethnographical collections now at Stafford. He is arranging to carry on ocean meteorological observations on a large scale, following mainly the plan adopted by the *Challenger* expedition. Negretti and Zambra's new deep-sea thermometers are to be employed.

THE German gunboat *Hyäne* visited Easter Island last autumn, and determined its exact position, which was found to be 27° 10′ S. lat., and 109° 26′ W. long. The commander of the *Hyäne*, Capt. Geiseler, has reported minutely to the German Admiralty Office on the ethnology of the island, and this report is accompanied by numerous drawings of old colossal statues, stone houses, monuments, tombs of chiefs, &c. At the same time Capt. Geiseler made a collection of ethnological specimens which has been forwarded to Germany by way of Apia. The report is now printed and published by Mittler and Sohn (Berlin).

PROF. BASTIAN has been nominated honorary president of the Berlin Geographical Society. The following gentlemen have been elected as honorary members: Prof. von Richthofen, Dr. Gustav Nachtigal, Prof. Neumayer, Dr. Pogge, Dr. Buchner, and Lieut. Wissmann. The latter has also received the Society's Silver Knights-Medal.

AT Berlin an aurora borealis was observed on April 29 at 9 p.m. The phenomenon brightened up the whole sky, across which numerous bright red cloud-streaks seemed to shoot.

MR. ERNEST GILES, the explorer, contemplates organising a grand final expedition to traverse the remaining unexplored portions of the Australian continent, and to endeavour to discover some more trustworthy traces of Leichhardt.

IN the "Publications of the Massachusetts Society for the Promotion of Agriculture," Mr. S. H. Scudder has given an interesting account of the habits of a small moth (*Retinia frustrana*), and of the ravages caused by it on the pitch pine of Nantucket Island (*Pinus rigida*). Of late it has become so abundant as to threaten the total destruction of the pines. Like its European congeners its larvæ bore into the interior of the healthy young shoots and destroy them. The remedy recommended is the radical one of taking off from every tree those shoots that show themselves to be infested, but the author is fully alive to the difficulties attendant upon such a recommendation, especially those of expense. The insect has not yet made its appearance on the adjoining mainland, but it seems to have been observed in other more distant parts of the Eastern States. In Europe (and indeed in Britain) much damage is done to conifers (especially Scotch fir) by allied species, and they more especially infest quite young trees. Some of them principally affect the lateral shoots, and these, if not too numerous, cause no lasting injury to healthy young trees; but one especially (*R. turionella*) attacks the leading shoot, and is far more serious; in this case, if the tree be strong and healthy, a lateral shoot takes the place of the destroyed "leader," and recovery is effected by this means.

NEWS has at last been received from Dr. Pogge, the companion of Lieut. Wissmann, on his journey across Africa, and who remained in Africa after Wissmann left. It appears that

Dr. Pogge reached the Mukenge safely in September last, bringing large collections with him. He had written and sent to Malange for means for his return journey.

A REPORT on the Peter Redpath Museum, Montreal, the foundation of which was laid by the Marquis of Lorne in September, 1880, describes the opening ceremony in August, during the meeting of the American Association. Mr. Redpath in a very few words handed over the Museum to the University, and speeches were made by the Chancellor, Dr. Carpenter, Prof. Hall, and Dr. Dawson. Already collections have been placed in the Museum, which promises to become one of the first rank.

THE current number of the *Agricultural Students' Gazette*, edited by students at the Royal Agricultural College, Cirencester, contains an instructive article on Devonshire Orchards and Cider-making, by C. B. Northcote, a member of the College. Miss Ormerod contributes a paper on the Coffee Grub in Ceylon, embodying our information on this pest up to the present time, from information largely derived from a pamphlet by Mr. Haldane on the subject. Mr. Rutherford gives a concise paper on the Agriculture of the Cotswolds; Prof. Garside one on Glanders, adducing evidence that it is a germ disease due to a bacillus. There is also an interesting and instructive collection of chemical curiosities, answers to examination questions; and in addition reports on the experimental field plots, on the weather, on the amount of chlorine in the rain water of the district, and on many other more purely college matters. This magazine fully keeps up to its advanced standard, and has a value in a circle far wider than its immediate connection with the Agricultural College.

WE have received the *Proceedings of the Medical Society* of the Kazan University, which contains, besides purely medical papers, several valuable papers of general interest. We notice among them a lecture, by Prof. Scherbakoff, on carbonic and azulmic acids in the soil as a measure of the oxidation of its organic constituents. It is known that since more attention has been given to the sanitary conditions of different soils, Herr Pettenkofer has proposed to measure the amount of putrefied organic matter in the soil by the amount of carbonic acid it contains. Prof. Scherbakoff makes a complete analysis of the chemical and putrefactive processes that are going on in the soil, and comes to the conclusion that, unhappily, the carbonic acid does not give a measure either of the oxidating capacity of the soil or of the decomposition of the organic matter. The same conclusion is arrived at with regard to azulmic acid, which is formed only under the action of special ferments, as appears from the classical researches of MM. Schlesing, Müntz, and Pasteur, so that oxidation of the organic elements of the soil may go on on a large scale without azulmic acid appearing as a result of the process. We notice also a paper, by M. Orloff, on the influence of wet and dry chlorine upon different materials when used for disinfection, the author giving the results of a series of experiments on various linen, cotton, silk, and woollen stuffs. The tables of diseases at Kazan and in several districts of the province are also of great interest; they show, for instance, that the number of cases of malarial fever is really enormous, as it has reached, in the town of Kazan, the figure of 23,000 cases during five years. As to cattle and horse diseases, their number is still more striking, as every year the province loses no less than 4300 to 4600 head of horned cattle, to which must be added sometimes—as in 1877—3250 cattle and horses exterminated by the Siberian plague.

THE additions to the Zoological Society's Gardens during the past week include a Bonnet Monkey (*Macacus radiatus*) from India, presented by Mr. F. J. Wicks; a Ring-tailed Coati (*Nasua rufa*), a Kinkajou (*Cercoleptes caudivolvulus*) from Demerara, presented by Mr. Ernest Francis; a Herring Gull (*Larus argentatus*), British, presented by Mrs. Andrews; a Smooth Snake (*Coronella lævis*), European, presented by Mr. W. A. B. Pain; a Bateleur Eagle (*Helotarsus ecaudatus*) from Africa, two Germain's Peacock Pheasants (*Polyplectron germaini*) from Cochin China, purchased; a Bennett's Wallaby (*Halmaturus bennetti*), four Brown-tailed Gerbillus (*Gerbillus erythrurus*), born in the Gardens.

OUR ASTRONOMICAL COLUMN

D'ARREST'S COMET.—The following approximate positions of this comet are deduced from M. Leveau's elements:—

At Greenwich Midnight

	R.A. h. m. s.	Decl. ° ′	Log. Distance from Earth.	Log. Distance from Sun.
May 25	13 13 47	+13 8·9	0·2983	0·4312
27	— 12 51	13 6·9		
29	— 12 0	13 4·9	0·3015	0·4267
31	— 11 15	13 1·9		
June 2	— 10 35	12 58·0	0·3051	0·4221
4	— 10 1	12 53·2		
6	— 9 34	12 47·6	0·3090	0·4175
8	— 9 13	12 41·1		
10	13 8 57	+12 33·9	0·3132	0·4128

THE OBSERVATORY OF RIO JANEIRO.—We have received the *Bulletin Astronomique et Météorologique de l'Observatoire Impérial de Rio de Janeiro* for January and February. In the first number are observations of the nucleus of the great comet of 1882 made by M. Lacaille. While stationed at Olinda (Pernambuco) for the observation of the transit of Venus, he remarked on November 16 a small nebulosity 6° south of the nucleus of the great comet: it was circular, and had a slight central condensation. On November 20 he saw it again; its aspect was the same as on the previous day, it had the same right ascension, but its declination was 1° further south. On November 22 and 26 it was observed in the same position as on the 20th. M. Lacaille believes that this small nebulosity was no other than a fragment of the nucleus of the great comet. On returning to Rio, he found on January 8, on examining this nucleus with the 10-inch equatorial and power of 500, that it was highly elongated and subdivided into four small nebulosities, the centres of which had the appearance of stars of the twelfth magnitude. The aspect of the fourth as compared with the others, was less condensed, but rather more lengthened out. On the following night he was surprised to find that the first nebulosity was no longer in the position that he had seen it on the 8th, but that it was situate outside the elongated nucleus, and its centre had lost the appearance of a star of the twelfth magnitude. The second nebulosity was precisely in the position of the day preceding. The fourth had sensibly approached the third. On January 10 the four nebulosities retained the same relative positions. Several days of cloudy weather followed, but on January 15 he found that there was a fifth nebulosity in the elongated nucleus. These changes are well shown in a lithograph accompanying M. Lacaille's observations. In the February number of the *Bulletin* are observations of the same comet, made at Athens by Dr. Julius Schmidt, as detailed in a letter addressed by him to the Emperor of Brazil. It relates chiefly to the nebulosities which were remarked by Dr. Schmidt in the vicinity of the nucleus of the great comet on October 9, 10, and 11, his drawings showing the fantastic forms presented by the nebulosities being lithographed.

THE OBSERVATORY OF MOSCOW.—Volume IX. (livraison i.) of *Annales de l'Observatoire de Moscou*, has been issued. Amongst the contents are a short paper by M. Bredichin on the resisting medium; Researches on the first comet of 1882 (Wells), and observations of the minor planet Victoria, taken in connection with others to be made at the Cape and other southern as well as northern observatories, as part of a plan organised by Dr. Gill, for the determination of a new value of the solar parallax. M. Bredichin compares the observed phenomena of the tail of the first comet of 1882 with the indications of theory.

KIELL ON TYCHO BRAHE'S NOVA 1572.—It has often been stated in our astronomical text-books, that John Kiell, Professor of Astronomy at Oxford, considered that the period of the celebrated star in Cassiopeia in 1572, was "about 150 years," or only half that which had been more generally assigned to it.

We suspect that this statement has arisen from a misconception of Kiell's words, while referring to the star in his *Introduction to the true Astronomy or Astronomical Lectures*, &c., the first English edition of which appears to have been published in 1721. In the third edition, 1739 (which is before us), at p. 56, we read, after his reference to the phenomena in 1572, "Leovitius, from the history of those times, tells us that in the time of the Emperor Otho, about the year 945, a new star appeared in Cassiopeia, just such a one as was seen in his time in the year 1572. And he brings us another ancient observation—that there was likewise seen in the *northern* region of the heavens, near the constellation Cassiopeia, in the year 1264, an eminently bright *star*, which kept itself in the same place, and had no proper motion. It is probable that these two stars might have been the same with that which was seen by Tycho, and that in about 150 years the same star may again make its appearance."

It will be remarked that Kiell makes no reference to any star seen midway between 945 and 1264, nor between 1264 and 1556, and it seems his meaning is clear, that Tycho's star, with a period of some 300 years, might make its appearance again "in about 150 years" from the time at which he wrote, as it might do were its changes accomplished in about three centuries. The misinterpretation of Kiell's words has led to his being credited with the opinion that the period is about 150 years, an idea which he probably never intended to express.

ELECTRICITY APPLIED TO EXPLOSIVE PURPOSES[1]

IN introducing the subject the lecturer indicated the principal advantages which it had been early observed would result from a certain mode of firing explosive charges by electric current instead of by the ordinary fuzes, the best of which had inherent defects, greatly limiting their use for any but the simplest operations. He traced the history and development of electric firing from the crude experiments of Benjamin Franklin, about the year 1751, through the various stages in which frictional electricity, volta-induction apparatus, and magnetoelectric machines had supplied the means of generating the current, the tendency of late years being to revert to a modified form of voltaic battery for one class of work, and to employ dynamo-electric machines for another class. The history and development of the low-tension or wire fuze, and of the various fuzes employed with electric currents of high tension, were also discussed, and their relative advantages, defects, and performances were described.

The only sources of electricity which at present thoroughly fulfilled the conditions essential in the exploding-agent for submarine mines were constant voltaic batteries. They were simple of construction, comparatively inexpensive, required but little skill or labour in their production and repair, and very little attention to keep them in constant good working order for long periods, and their action might be made quite independent of any operation to be performed at the last moment.

When first arrangements were devised for the application of electricity in the naval service to the firing of guns and so-called outrigger charges, the voltaic pile recommended itself for its simplicity, the readiness with which it could be put together and kept in order by sailors, and the considerable power presented and maintained by it for a number of hours. Different forms of pile were devised at Woolwich for boat and ship use, the latter being of sufficient power to fire heavy broadsides by branch circuits, and to continue in a serviceable condition for twenty-four hours, when they could be replaced by fresh batteries, which had in the meantime been cleaned and built up by sailors.

The Daniell and sand batteries first used in conjunction with the high-tension fuze for submarine mining service were speedily replaced by a modification of the battery known as Walker's, which was after some time converted into a modified form of the Leclanché battery.

The importance of being able to ascertain by tests that the circuits leading to a mine, as well as the fuzes introduced into that circuit, were in proper order, very soon became manifest; and many instances were on record in the earlier days of submarine mining of the disappointing results attending the accidental disturbance of electric firing arrangements, when proper means had not been known or provided for ascertaining whether the circuit was complete, or for localising any defect when discovered.

The testing of the Abel fuze, in which the bridge, or igniting and conducting composition, was a mixture of the copper phosphide and sulphide with potassium chlorate, was easy of accomplishment (by means of feeble currents of high tension), in proportion as the sulphide of copper predominated over the phosphide. Even the most sensitive might be thus tested with safety; but when the necessity for repeated testing, or even for the passing of a signal through the fuze, arose, as in a permanent system of submarine mines, the case was different, this fuze being susceptible of considerable alterations in conductivity on being frequently submitted to even very feeble test-currents, and its accidental ignition by such comparatively powerful test- or signal-currents as might have to be employed, became so far possible as to create an uncertainty which was most undesirable.

Hence, and also because the priming in these fuzes was liable to some chemical change detrimental to its sensitiveness, unless thoroughly protected from excess of moisture, another form of high-tension fuze, specially adapted for submarine mining service, was devised at Woolwich. This, though much less sensitive than the original Abel fuze, was sufficiently so for service requirements, while it presented great superiority over the latter in stability and uniformity of electric resistance; and, though not altogether unaffected by the long-continued transmission of test-currents through it, the efficiency of the fuze was not affected thereby.

Although high-tension fuzes presented decided advantages in point of convenience and efficiency over the earlier form of platinum wire fuze, the requirements which arose, in elaborating thoroughly efficient permanent systems of defence by submarine mines, and the demand for a battery for use in ships which would remain practically constant for long periods, caused a very careful consideration of the relative advantages of the high- and low-tension systems of firing to result in favour of the employment of wire fuzes for these services. In addition to the disadvantages pointed out there was an element of uncertainty, or possible danger, in the employment of high-tension fuzes, which, though partly eliminated by the adoption of voltaic batteries, in place of generators of high-tension electricity, might still occasionally constitute a source of danger, namely, the possibility of high-tension fuzes being accidentally exploded by currents induced in cables, with which they were connected, during the occurrence of thunderstorms, or of less violent atmospheric electrical disturbances.

Experiment, and the results obtained in military service-operations, had demonstrated that if insulated wires, immersed in water, buried in the earth, or even extended on the ground, were in sufficient proximity to one another, each cable being in circuit with a high-tension fuze and the earth, the explosion of any of the fuzes by a charge from a Leyden jar, or from a dynamo-electric machine of considerable power, might be attended by the simultaneous ignition of fuzes attached to adjacent cables, which were not connected with the source of electricity, but which become sufficiently charged by the inductive action of the transmitted current. It therefore appeared very possible that insulated cables extending to land- or submarine-mines, in which high-tension fuzes were inclosed, might become charged inductively during violent atmospheric electrical disturbances to such an extent as to lead to the accidental explosion of mines with which they were connected. In a Report by von Ebner on the defence of Venice, Pola, and Lissa, by submarine mines, in 1866, he refers to the accidental explosion of one of a group of sixteen mines during a heavy thunderstorm, as well as to the explosion of some mines, by the direct charging of the cables, through the firing station having been struck by lightning. Two instances of the accidental explosion of tension fuzes by the direct charging of overhead wires during lightning discharges occurred in 1873 at Woolwich.

Subsequently an electric cable was laid out at Woolwich along the river bank below low-water mark, and a tension fuze was attached to one extremity, the other being buried. About eleven months afterwards the fuze was exploded by a charge induced in the conductor during a very heavy thunderstorm.

In consequence of such difficulties as these experienced in the special application of the high-tension fuzes to submarine purposes, the production of comparatively sensitive low-tension fuzes, of much greater uniformity of resistance than those employed in former years, was made the subject of an elaborate

[1] The fifth of the series of Six Lectures on the Applications of Electricity, delivered on Thursday evening, April 19, at the Institution of Civil Engineers, by Sir F. A. Abel, F.R.S., Hon.M.Inst.C.E.

experimental investigation by the lecturer. Different samples of comparatively thin wires, made from commercial platinum, showed very great variations in electrical conductivity. Very considerable differences in the amount of forging to which the metal, in the form of sponge, had been subjected, did not importantly affect either its specific gravity or its conductivity, and the fused metal had only a very slightly higher degree of conductivity than the same metal forged from the sponge. The conductivity of very fine wires could therefore be but slightly affected by physical peculiarities of the metal, and the considerable differences in conductivity observed in different samples of platinum were therefore chiefly ascribable to variations in the degree of its purity. It appeared likely that definite alloys might furnish more uniform results than commercial platinum; experiments were therefore made with fine wires of German silver and of the alloy of sixty-six of silver with thirty-three of platinum employed by Matthiessen for the reproduction of B.A. standards of electrical resistance. Both were greatly superior to ordinary platinum in regard to the resistance opposed to the passage of a current; German silver was in its turn superior to the platinum-silver alloy; although the difference was only trifling in the small lengths of fine wire used in a fuze (0·25 inch), while the comparatively ready fusibility of platinum-silver contributed, with other physical peculiarities of the two alloys, to reduce German silver to about a level with it. Moreover, the latter did not resist the tendency to corrosive action exhibited by gunpowder and other more readily explosive agents which had to be placed in close contact with the wire bridge in the construction of a fuze, while the platinum-silver alloy was found to remain unaltered under corresponding conditions. Experiments having also been made with alloys of platinum with definite proportions of iridium, the metal with which it is chiefly associated, very fine wires of an alloy containing 10 per cent. of iridium were eventually selected as eminently the best materials for the production of wire fuzes of comparatively high resistance and uniformity, this alloy being found decidedly superior in the latter respect as well as in point of strength (and therefore of manageableness in the state of very fine wire 0·001 inch in diameter) to the platinum-silver wire. The fuzes now used in military and submarine services were made with bridges of iridio-platinum wire containing 10 per cent. of the first-named metal.

The electrical gun-tubes in the navy were fired by means of a specially arranged Leclanché battery, and branch circuits worked to the different guns; in broadside firing it was important that the wire bridge of any one of the gun-tubes which was first fired should be instantaneously fused on the passage of the current, so as to cut this branch out of circuit; in this respect the comparatively fusible platinum-silver alloy appeared to present an advantage, hence the naval electrical fuzes were made with bridges of that alloy. Uniformity of electrical resistance had become a matter of such high importance in the delicate arrangements connected with the system of submarine mines, as now perfected, that the very greatest care was bestowed upon the manufacture of service electric fuzes and detonators, which were in fact made in all their details with almost the precision bestowed upon delicate scientific instruments, and the successful production of which involved an attention to minutiæ which would surprise a superficial observer.

One of the earliest applications of electricity to the explosion of gunpowder was the firing of guns upon proof at Woolwich by means of a Grove battery and a gun-tube, which was fired by a platinum wire bridge, a shunt arrangement being used for directing the current successively into the distinct circuits connected with the guns to be proved. When the high-tension fuze had been devised, gun-tubes were made to which it was applied, and an exploder was arranged by Wheatstone, having a large number of shunts, so that as many as twenty-four guns might be brought into connection with the instrument, and fired in rapid succession by the depression of separate keys connected with each.

The firing of cannon as time-signals was an ancient practice in garrison towns, but the regulation of the time of firing the gun by electrical agency from a distance appears first to have been accomplished in Edinburgh, where, since 1861, the time-gun had been fired by a mechanical arrangement actuated by a clock, the time of which is controlled electrically by the mean-time clock at the Royal Observatory on Calton Hill.

Shortly after the establishment of the Edinburgh time-gun, others were introduced at Newcastle, Sunderland, Shields, Glasgow, and Greenock. The firing of the gun was arranged for in various ways; in some instances it was effected either direct from the Observatory at Edinburgh, or from shorter distances, by means of Wheatstone's magnetoelectric exploders. At present there were time-guns at West Hartlepool, Swansea, Tynemouth, Kendal, and Aldershot, which were fired electrically, either by currents direct from London, or by local batteries, which were thrown into circuit at the right moment by means of relays, controlled from St. Martin's-le-Grand.

About thirteen years ago the electrical firing of guns, especially for broadsides, was first introduced into the navy, with the employment of the Abel high-tension gun-tube and voltaic piles. The gun-tubes then used were manufactured originally for the proof of cannon and for experimental artillery operations, and were of very simple and cheap construction. Experience proved them to be unfitted to withstand exposure to the very various climatic influences which they had to encounter in Her Majesty's ships, and in store in different parts of the world. The low-tension gun tubes, having a bridge of very fine platinum-silver wire, surrounded by readily ignitible priming composition, was therefore adopted as much more suitable for our naval requirements.

The arrangements for broadsides or independent firing, and also for the firing of guns in turret-ships, had been very carefully and successfully elaborated in every detail, including the provision of a so-called drill- or dummy electrical gun-tube, which was used for practice and refitted by well instructed sailors. The firing-keys, and all other arrangements connected with electrical gun-firing, were specially designed to insure safety and efficiency at the right moment.

The electric detonators for firing outrigger-torpedoes, or for other operations to be performed from open boats, corresponded, so far as the bridge was concerned, with the naval electric gun-tubes, and were fired with a specially fitted Leclanché battery. These electric appliances were now distributed throughout the navy, and the men were kept, by instruction and periodical practice, well versed in their use.

The application of electricity to the explosion of submarine mines, for purposes of defence and attack, received some attention from the Russians during the Crimean War, under the direction of Jacobi; thus a torpedo, arranged to be exploded electrically when coming into collision with a vessel, was discovered at Yeni-Kale during the Kertsch expedition in 1855. Some arrangements were made by the British at the conclusion of the war to apply electricity to the explosion of large powder-charges for the removal of sunken ships, &c., in Sebastopol and Cronstadt Harbours. In 1859, a system of submarine mines, to be fired through the agency of electricity by operators on shore, was arranged by von Ebner for the defence of Venice, which, however, never came into practical operation. Early in 1860 Henley's large magnetoelectric machine, with a supply of Abel fuzes, and stout indiarubber bags, with fittings to resist water-pressure, was despatched to China for use in the Peiho River, but no application appeared to have been made of them. The subject of the utilisation of electricity for purposes of defence, however, did not receive systematic investigation in England or other countries until some years afterwards, when the great importance of submarine mines, as engines of war, was demonstrated by the number of ships destroyed and injured during the war in America.

The application of electricity to the explosion of submarine mines was very limited during that war, but arrangements for its extensive employment were far advanced in the hands of both the Federals and Confederates at the close of the war, men of very high qualifications, such as Capt. Maury, Mr. N. J. Holmes, and Capt. McEvoy having worked arduously and successfully at the subject.

The explosion of submerged powder-charges, by mechanical contrivances, either of self-acting nature or to be set into action at desired periods, was accomplished as far back as 1583, during the siege of Antwerp, by the Duke of Parma, and from that period to 1854 mechanical devices of more or less ingenious and practicable character had been from time to time applied to some small extent, in different countries, to the explosion of torpedoes. The Russians were the first to apply self acting mechanical torpedoes with any prospect of success, and had the machines used for the defence of the Baltic been of larger size (they only contained 8 or 9 lb. of gunpowder), their presence would probably have proved very disastrous to some of the English ships which came into collision with and exploded them.

Various mechanical devices for effecting the explosion of torpedoes by their collision with a ship were employed by the Americans, a few of which proved very effective. But although in point of simplicity and cost, a system of defence by means of mechanical torpedoes possessed decided advantages over any extensive arrangements for exploding submarine mines by electric agency, their employment was attended by such considerable risk of accident to those at whose hands they received application that, under many circumstances which were likely to occur, they became almost as great a source of danger to friend as to foe.

The most important advantages secured by the application of electricity as an exploding-agent of submarine mines were as follows:—They might be placed in position with absolute safety to the operators, and rendered active or passive at any moment from the shore; the waters which they were employed to defend were therefore never closed to friendly vessels until immediately before the approach of an enemy; they could be fixed at any depth beneath the surface (while mechanical torpedoes must be situated directly or nearly in the path of a passing ship), and they might be removed with as much safety as attended their application.

There were two distinct systems of applying electricity to the explosion of submarine mines. The most simple was that in which the explosion was made dependent upon the completion of the electric circuit by operators stationed at one or more posts of observation on shore; such a system depended, however, for efficiency, on the experience, harmonious action, and constant vigilance of the operators at the exploding- and observing-stations, and was, moreover, entirely useless at night, and in any but clear weather.

The other, which might also be used in conjunction with the foregoing, was that of self-acting mines, exploded either by collision with the ship, whereby circuit was completed through the inclosed fuze, or by the vessel striking a circuit closer, whereupon either the mine, moored at some depth beneath, was at once fired, or the necessary signal was given to the operator on shore.

Continental nations had followed in our footsteps, in providing themselves with equipments for defensive purposes by submarine mines, and the Danes, Swedes, and Norwegians had pursued the subject of submarine mines with special activity and success.

In the United States the subject of the utilisation of electricity as an exploding-agent for war purposes was being actively pursued, and important improvements in exploding instruments, electric fuzes, and other appliances had been made by Smith, Farmer, Hill, Striedinger, and others already mentioned, while no individual had contributed more importantly to the development of the service of submarine explosions than General Abbot, of the United States Engineers.

Illustrations of actual results capable of being produced in warfare by submarine operations had hitherto been very few; but of the moral effects of submarine mines there had already been abundant illustrations. In the war carried on for six years by the Empire of Brazil and the Republic of Uruguay and the Argentine Republic of Paraguay, the latter managed, by means of submarine mines, to keep at bay, for the whole period, the Brazilian fleet of fifteen ironclads and sixty other men-of-war. In the Russo-Turkish war, submarine mines and torpedoes were a source of continued apprehension; and the French naval superiority was paralysed, during the Franco-German war, by the existence, or reputed existence, of mines in the Elbe.

The application of electricity to the explosion of military mines, and to the demolition of works and buildings, had been of great importance in recent wars in expediting and facilitating the work of the military engineer. The rapidity with which guns, carriages, &c., were disabled and destroyed by a small party of men who landed after the silencing of the forts at Alexandria, illustrated the advantages of electrical exploding arrangements, combined with the great facility afforded for rapid operations by the power possessed of developing the most violent action of gun-cotton, dynamite, &c., through the agency of a detonator.

The application of electricity to the explosion of mines for land-defences during active war was not an easy operation, inasmuch as not only the preparation of the mines but also the concealment of electric cables and all appliances from the enemy entailed great difficulties, unless the necessary arrangements could have been made in ample time to prevent a knowledge of them reaching the enemy.

But few words were needed to recall to the minds of civil engineers the facilities which the employment of electricity to explosive purposes afforded for expediting the carrying out of many kinds of works in which they were immediately interested. Electrical blasting, especially in combination with rock-boring machines, had revolutionised the operation of tunnelling and driving of galleries; and although in ordinary mining and quarrying operations the additional cost involved in the employment of fuzes, conductors, and the exploding-machine was not unfrequently a serious consideration, there were, even in those directions, many occasions when the power of firing a number of shots simultaneously was of great importance. There was little doubt, moreover, that accidents in mining and quarrying would be considerably reduced in number if electrical blasting were more frequently employed.

The conveniences presented by electric firing arrangements under special circumstances were interestingly illustrated by a novel proceeding at the launch of a large screw steamer at Kinghorn in Scotland, which was recently accomplished by placing small charges of dynamite in the wedge-blocks along the sides of the keel and exploding them in pairs, hydraulic power being applied at the moment that the last wedge was shot away.

In the deepening of harbours and rivers and in the removal of natural or artificial submerged obstructions, the advantages of electric firing were so obvious that extended reference to them was unnecessary.

A substitute for electrical firing which had been applied with success to the practically simultaneous firing of several charges consisted of a simple modification of the Bickford fuze, which, instead of burning slowly, flashed rapidly into flame throughout its length, and hence had received the name of instantaneous fuze or lightning fuze. The fuze burned at the rate of about 100 feet per second; it had the general appearance of the ordinary mining fuze, but was distinguished from the latter by a coloured external coating. Numerous lengths of this fuze were readily coupled up together so as to form branches leading to different shot-holes, which might be ignited together so as to fire the holes almost simultaneously. In the navy this fuze was used as a means of firing small gun-cotton charges to be thrown by hand into boats when these engaged each other, the fuze being fired from the attacking boat by means of a small pistol, into the barrel of which the extremity was inserted.

THE IRON AND STEEL INSTITUTE

THE annual meeting of this society took place at the Institution of Civil Engineers on May 9, 10, and 11 last. On the first day the chair was taken by the retiring president, Mr. Josiah T. Smith, but after some formal business had been gone through he vacated it in favour of the president elect, Mr. B. Samuelson, M.M., F.R.S. The latter proceeded to read an able address, dealing mainly with the great progress which had taken place in the iron and steel industries since the Institute was founded in 1869. He remarked on the very large makes of pig iron which were now going on in American blast furnaces, and stated that these were found to be economical even as regards fuel and wear and tear of the lining. He then dwelt at some length on improvements in the manufacture of coke, especially with a view to recovery of the waste products. The deterioration which was feared would result as regards the coke itself had not appeared in the case of the Simon-Carvés ovens, worked by Messrs. Pease and Co., who were recovering oil and tar to the value of 4*s.* 2*d.* per ton of coal. Against this was to be set increased expenses to the amount of 1*s.* 4*d.* per ton of coke, and also interest on first cost and maintenance. He further referred to the Jameson process lately described before the Institution of Mechanical Engineers, and observed that this principle was being applied to recover oil and ammonia from smouldering waste-heaps at the pit bank. Passing on to the manufacture of steel he spoke with much approval of the Bicheroux gas puddling furnaces at Ougrée in Belgium, where gas obtained from slack was used for puddling, and gave more heat for steam-raising purposes than the old system. Speaking of the future demand for iron and steel, he pointed out that the United States had fifty times and Russia five times as many miles of railway per million of people as had our Indian Empire; and strongly urged the further development of railways in the latter country. The address also touched upon many other points connected with iron and steel, such as the

increased production of Bessemer and Siemens steel, the great diminution in their price, the immense increase in shipbuilding, the proposed improvements in the patent law, and the better relations now existing between masters and workmen. At the conclusion of his address the president presented the Bessemer medal to Mr. J. G. Snelus of Workington for his achievements in introducing the basic system for the making of steel. A similar medal, bestowed upon Mr. S. G. Thomas, was reserved until his return to England.

The meeting then took up the discussion on a paper by Mr. Snelus on the Chemical composition and testing of steel rails. This paper was read at the Vienna meeting of the Institute and its discussion postponed. Mr. Snelus now added particulars of a new test for the hardness of rails, which consisted in driving a uniform punch under uniform pressure into a piece of the rail, and measuring the depth of the hole produced. Various experiments had satisfied him that this depth would be inversely proportional to the hardness of the rail. M. Cazés, engineer to the Lyons Railway, read a very long note in French on the same subject, the point of which was that, as he maintained, the wearing power of the rail depends not only on its chemical composition but also on the temperature in rolling and the amount of compression which it has experienced while being rolled. He stated that a percentage of carbon varying from ½ to 1 per cent. was found in France to give the best result for rail heads.

Two papers were then taken together: the first of these was by Mr. Wm. Parker (Chief Engineer and Surveyor of Lloyd's), on the Use of steel castings in lieu of steel and iron forgings for ship and marine engine construction. This paper gave the results of an important investigation made by Lloyd's Registry into the applicability of steel castings to heavy articles such as crank shafts, sternposts, &c., which had hitherto been chiefly made in forged iron. The result was to convince the Committee that such constructions can be made of cast steel quite as good for the purposes intended as those of wrought iron, and without the uncertainty which always exists more or less in large iron forgings. The making of such castings is mainly in the hands of three firms, who, however, differed very materially in their views of the best mode of proceeding, especially as to whether the metal should be melted in crucibles or in the ordinary open hearth. At Terre Noire, where such castings are also made, great importance is attached not only to annealing but also to tempering them in oil; and the author gave particulars of experiments made on this subject, which showed that such treatment had a marked effect in increasing the strength of the specimens whilst slightly diminishing their ductility. Reference was made to the distinction between forged steel and cast steel, and a number of experiments were quoted on similar bars of both these materials and also of wrought iron. In these experiments the strength of the cast steel specimens was in every case greater than those of the wrought iron, whilst the ductility was about the same. With the forged steel the tensile strength was designedly made low, but the ductility was very high. Taking the product of the ultimate strength and ultimate elongation as representing roughly the quality of the material, it appears that the cast steel is one-third higher than wrought iron on an average, whilst the forged steel is three times as high. Transverse tests, both by steady pressure and by impact, and also torsion tests, gave results practically similar. Finally some experiments made by Mr. James Neilson on steel, partly as cut from the ingot and partly as hammered or rolled down to a comparatively small thickness, showed that the latter process produced a very decided increase both as to strength and ductility in some cases, although the results were not very uniform. For casting crank shafts and similar work Lloyd's Committee considered that the tensile strength of the steel should not exceed thirty tons per square inch, and that a piece 1¼ inch square should bend cold through an angle of 90 degrees.

The second paper was by Mr. Wm. D. Allen, of Henry Bessemer and Co., and was on Bessemer steel in its cast and unwrought state. The object of this paper was to dispel the idea that Bessemer steel is not a safe material for casting on account of the frequent presence of cavities or blowholes within it. From daily experience Mr. Allen affirms that perfectly sound castings can be made from Bessemer steel, provided that the ladleful is alloyed either with ferromanganese or with some iron ore rich in silicon. In order that this alloy may mingle perfectly with the steel, the ladle should be violently stirred by means of an agitator, already described to the Institute. Of such steel, the reader stated that nearly 500 hydraulic cylinders had been made, and tested up to two, three, or even four tons per square inch.

An interesting discussion followed these papers. Mr. James Riley doubted whether the crucible process produced a result more uniform than the open hearth, and spoke strongly in favour of annealing and tempering in oil. He also doubted whether the work put upon forged steel gives the advantage which was claimed for it. Mr. Hall (Messrs. Jessop and Sons, of Sheffield) defended the crucible process, but disparaged Bessemer castings. Sir Henry Bessemer, however, considered that the agitator had overcome the difficulty which previously existed in making sound castings in his steel, while Sir Wm. Siemens observed on the danger that castings may contract unequally in cooling, and on its complete cure by annealing. He suggested an explanation for the curious fact of the advantage due to oil-hardening, namely, that the oil produced a compression of the outer layers, which acted on the rest of the mass, and was of more effect than any mechanical pressure could be.

The second day's proceedings opened with the reading of two papers on the vexed question of hot blasts and high furnaces. The first was by Mr. Wm. Hawdon, who gave the particulars of comparative experiments made on one of Messrs. Samuelson's furnaces at Middlesborough, which had been supplied alternately with blast from pipe stoves at 990° F. and with blast from firebrick stoves at 1400° F. The final result was an increase in the make per week from 400 to 458 tons of pig, a diminution in the coke per ton from 23·8 to 22·3 cwt., and at the same time an improvement in the quality of experiments at the various temperatures of blast between these two limits showed a gradual rate of improvement. At the same time the temperature of the escaping gases was diminished from 468° to 448°, and the volume, of course, was diminished also. Comparing the two modes of heat, he showed that the area of heating surface in proportion to the cubic capacity of the stove was much greater in the firebrick than in the pipe stove, giving a corresponding improvement in effect, and that the gases escaping from the chimney, which in the pipe stoves had a temperature of 1240° F., in the brick stoves were as low as 250° F. The result was to effect a very considerable saving of gas used for heating the blast, which gas may, of course, be utilised for steam-raising or other purposes.

The second paper was by Mr. I. Lowthian Bell, F.R.S., and dealt with the Value of successive additions to the temperature of air used in smelting iron. This paper was to some extent a rejoinder to those of Mr. Charles Cochrane, recently read before the Institution of Mechanical Engineers. Mr. Bell first considered the proposed application of hydrogen, or what is called water gas, to the blast furnace, and showed that this could produce no advantage in saving of heat. He then dealt with the question of the possible economy of coke in an ordinary furnace, and reiterated that a limit was placed to such economy by the fact that when the escaping gas consists of carbonic oxide and carbonic acid in the proportion of 2 to 1, these combined gases can no longer produce any effect in reducing iron ore. Hence the very great saving of fuel which had at first been effected by enlarging the size of the furnaces and increasing the heat of the blast, had now nearly reached its limit; which Mr. Bell still held to be represented by a capacity of about 15,000 cubic feet, and a temperature of about 1000° F. He dealt with the suggestion that the heating of the blast could be advantageously used to replace the heat evolved in burning carbonic oxide and carbonic acid; and showed that to effect any great improvement in this particular would require blasts of too high temperature to be practically available. He commented upon the results with the furnace at Messrs. Samuelson's works, and considered that the increase in the make must be due to the increased quantity of blast rather than to its higher temperature. He admitted a saving in coke of about 1 cwt., but he observed that on the other hand the furnace was driven very slowly, only supplying about half the weight of pig iron per cubic foot of capacity which was usually supplied by the furnaces in the Cleveland district.

Mr. Bell's position was strongly assailed by Mr. Charles Cochrane; but although the questions between them were still further debated, it can hardly be said that they are completely settled. The problem is one which involves several independent factors, and a variation in any one of these might produce a large effect on the final result. It was so far fortunate that on this occasion the question of stoves and of blast furnaces was considered together, and not separately, as is often the case; but

there are still other matters to be taken into consideration. One of these, for instance, is the distance between the tuyeres at the bottom of the furnace. Mr. Cochrane confidently predicted that an alteration in this particular would effect a very important saving in Messrs. Samuelson's furnaces. The large economy actually realised by the use of brick stoves was commented upon by several speakers; but the advantage of increasing the capacity of furnaces appeared to be doubted by two very high authorities upon the subject, Mr. Edward Williams and Mr. E. Windsor Richards.

On Thursday afternoon the paper read was on the Northampton iron ore district, by Mr. W. H. Butlin. It gave an interesting description of this district, well known to travellers on the main line of the Midland Railway, in which, however, the deposits of ore have only been developed within the last thirty years. The paper also contained analyses of the ore, which is of a very variable character, and also of the limestone, slags, &c.

On Friday morning the first paper read was by Mr. John Stead of Middlesborough, on a New method for the estimation of minute quantities of carbon. The author had found that the colouring matter, which is produced by the action of dilute nitric acid upon white iron and steel, has the property of being soluble in potash and soda solutions, and that the alkaline solution has about two and a half times the depth of colour produced by the ordinary acid solutions. Hence it occurred to him that the colour-matter might be separated from the iron, as an alkaline solution, by simply adding an excess of sodium hydrate to the nitric acid solution of iron, and that the colour solution thus obtained might be used as a means of determining the amount of carbon present. This method is found to succeed well, as small a quantity as 0·03 per cent. of carbon being readily detected. The method of using it was described, and also experiments made to determine (1) the influence of heating the nitric acid solution for a longer or shorter time; (2) the effect of using an excess of nitric acid to dissolve the steel; (3) the effect on the solvent power of using a greater or less quantity of soda solution; (4) the effect of the presence of small quantities of chlorine. All these experiments proved satisfactory as regards the new process. An improved form of chromometer was also described.

The next paper was on the Production and utilisation of gaseous fuel in the iron manufacture, by Mr. W. S. Sutherland of Birmingham. It was of a somewhat discursive character, containing various suggestions, especially as to a method of making wrought iron by the converter process; but its chief object was to describe the production of a cheap form of heating gas, which the author has used largely for the welding up of boiler-flues, tubes, &c. In this process part of the fuel is burnt, as completely as possible, to carbonic acid and water, but the resulting heat is stored up partly in the remainder of the fuel and partly in regenerators, that in the regenerators being made to heat up to a sufficiently high degree a quantity of steam. This superheated steam is passed through the hot fuel, and forms with it carbonic oxide and hydrogen, which go away to be stored up and used. With this process about 55,000 cubic feet of gas is made per ton of Staffordshire coal, and at a cost of about 3*d*. per 1000 cubic feet, its heating power being about one-half that of coal gas. The author pointed out that it was most important to prevent as far as possible the formation of carbonic acid, and that for this a high temperature (say 1200° C.) was required.

The following papers were taken as read:—On Coal-washing machinery, by Mr. Fritz Baare; on the Tin-plate manufacture, by Mr. E. Trubshaw; and on Improvements in railway and tramway plant, by Mr. Albert Riche.

SCIENTIFIC SERIALS

American Journal of Science, April.—Review of De Candolle's origin of cultivated plants, with annotations on certain American species, by A. Gray and J. H. Trumbull.—Remarks on Glyptocrinus and Reteocrinus, two genera of Silurian crinoids, by C. Wachsmuth and F. Springer.—Smee battery and galvanic polarisation, by H. Hallock.—The age of the Southern Appalachians, by O. B. Elliott.—Evolution of the American trotting-horse, by W. H. Brewer.

IN the *Annalen der Physik und Chemie* for 1883, part i, Ernst Pringsheim has an elaborate paper on the theoretical and practical aspects of Crooke's radiometer. This is followed by essays on Stokes's law of fluorescence, by Ed. Hagenbach; on special cases of crystallisation, by E. Lommel; on the heat-conducting power of fluids, by L. Graetz; on the relation of specific heat in gases and vapours, by P. A. Müller; on the constant result of internal friction and galvanic conduction in relation to temperature, by L. Grossmann; and on A. Guebhard's proposed method of determining equipotential lines, by Hugo Meyer. Part ii. contains papers by O. Grotian on the power of electric conduction of some cadmium and quicksilver salts in liquid solutions; by W. C. Röntgen, on the change produced by electric power in the double fracture of quartz (continued in part iv.); by A. Kundt, on the optical character of quartz in the electric field; by H. Meyer, on the magnetising function of steel and nickle; by A. von Waltenhofen, on the history of recent dynamoelectric machines, with some remarks on the determination of the working powers of electromagnetic motors; by J. Wagner, on the tenacity of solutions of salts; by S. von Wroblewski, on the absorption of gases by fluids under high pressure; by A. Schuller, on distillation in vacuum; by K. R. Koch, on the elasticity of crystals of the regular system; by C. Bohn, on absolute masses; by E. Gerland, on the methods adopted by R. Kohlrausch in his researches in contact electricity. In part iii. papers are contributed by F. Neesen, on the specific heat of water; by E. Ketteler, on the conflicting theories of light (continued in part iv.); by W. G. Hankel, on the thermoelectric properties of helvine, mellite, pyromorphite, mimetesite, phenakite, pennine, strontianite, witherite, cerussite, titanite; by F. Niemöller, on the dependence of the electromotor power of a reversible element on the pressure exercised on its fluidity; by C. Tromme, on experimental researches in magnetism; by K. Vierordt, on sound measurement; by A. Ritter, on the constitution of gaseous bodies; by K. R. Koch, on a method of testing the micrometric screw. Part iv. contains papers by F. Kohlrausch, on the galvanic gauging of the surface coil of a wire bobbin; by C. Tromme, on electrical research; by M. Baumeister, on the experimental investigation of torsion elasticity; by E. Wiedemann, on thermochemical research; by G. Kirchhoff, on the theory of light radiation; by W. Wundt, on sound measurement.

Journal de la Physique, February.—On a spectroscope with great dispersion, by M. Cornu.—On the comparative observation of telluric and metallic lines, as a means of observing the absorbent powers of the atmosphere, by the same.—Researches on the photometric comparison of differently coloured sources, and in particular on the comparison of different parts of the same spectrum, by MM. Macé de Lepinay and Nicati.—On electric shadows and various connected phenomena (second article), by M. Righi.

Verhandlungen der k.k. Zool.-botan. Gesellschaft in Wien, 1882, Bd. xxxix. Heft 2 (March, 1883), contains:—Zoology.—Biological notes on some beetles belonging to the Dascyllidæ and Parnidæ, by Th. Beling.—On Platen's ornithological collections from Amboyna, by W. Blasius.—On a new tortoise, by J. v. Hornig.—On the genus Colias, by A. Keferstein.—On the skin glands in some larvæ, by Dr. Klemensiewicz (Plates 21 and 22).—On new Hymenoptera, by Fr. Kohl (Pl. 23).—On the Myriapods of Austrian-Hungary and Servia, by Dr. R. Latzel.—The butterfly fauna of Surinam, v., by H. B. Möschler (Plates 17 and 18).—On a new mite in the inside of the quill feathers in the hen, by Dr. C. Nörner (Plates 19 and 20).—On a collection of birds from Central Africa, sent by Dr. E. Bey, by A. v. Pelzeln.—On Pselaphidæ and Scydmænidæ, from Java, Borneo, and Central and South America, by E. Reitter.—On *Icaria scudderi*, by Dr. H. Weyenbergh.

THE *Atti* of the Roman *Accademia dei Lincei* for January and February, 1883, contains papers by E. Millosevich, on the ingress of Venus on the solar disk, December 6, 1882; by A. Lugli, on the mean variation of temperature in Italy, as affected by latitude and elevation; by A. Viola, on the relations of some physical properties of gaseous bodies under constant pressure and of constant bulk; by L. Pigorini, on barbaric stations still existing in the Emilian provinces; by Tommasi-Crudeli, on the malaria of the Tre Fontane district, which appears not to have been beneficially affected by the Eucalyptus plantations elsewhere found so efficacious; by S. Tacchini, on meteoric dust and the chemical analysis of the sands of the Sahara; by the same author, on Finlay's comet and on the new asteroid (232) discovered on February 1 by Palisa; by S. Brioschi, on the algebraic relations between the hyperelliptical functions of first order; by S. Ferrari, on the relations between the meteoric elements and some agricultural returns for the year 1880 in Italy.

THE *Rendiconti* of the *Reale Istituto Lombardo di Scienze e Lettere* for February and March, 1883, contains papers by G. Ascoli, on Irish glosses, especially those of the Ambrosian Codex; by M. E. E. Beltrami, on the theory of magnetic layers; by Z. Volta, on an unpublished drama of Luigi Ceretti; by G. A. Maggi, on the transmission of undulatory motion, and especially of luminous waves, from one isotropic medium to another; by P. F. Denza, on the observations of the transit of Venus made at the observatory of the Collegio Carlo Alberto in Moncalieri.

SOCIETIES AND ACADEMIES

LONDON

Royal Society, April 12.—"The principal cause of the large errors at present existing between the positions of the Moon, deduced from Hansen's Tables and observation; and the cause of an apparent increase in the secular acceleration in the Moon's mean motion required by Hansen's Tables, or of an apparent change in the time of the Earth's rotation," by E. J. Stone, F.R.S.

The errors in the lunar theory have been traced to the effects of changes in the *unit of time* which have, apparently unconsciously, been introduced, from time to time, into astronomy with changes in the adopted data.

The argument is clearly seen by a consideration of the different expressions for the longitudes of, what may be called, the mean sun which have been adopted for the determination of the sidereal times at mean noon.

If B, H, and V denote the longitudes of the mean sun according to Bessel, Hansen, and Le Verrier, we have for 1850, January 1, Paris mean noon, $+ t$

$$B = 280\ 46\ 36''{\cdot}12 + 1296027{\cdot}618184t + 0{\cdot}0001221805 \,.\, t^2$$
$$H = 280\ 46\ 43{\cdot}20 + 1296027{\cdot}674055t + 0{\cdot}0001106850 \,.\, t^2$$
$$V = 280\ 46\ 43{\cdot}51 + 1296027{\cdot}678400t + 0{\cdot}0001107300 \,.\, t^2.$$

In all these expressions the unit of time has been *supposed* to be a Julian year of 365·25 mean solar days. The constant differences 7″·08 and 7″·39 in $B-H$ and $B-V$ are not unimportant, for they introduce abrupt changes in the record of time; but the differences in the coefficients of t and t^2 show that the *same* unit of time cannot have been adopted in these expressions. The measure of time must be continuous; let, therefore, 1 and $(1 + x)$ be the units in B and H,

then $1296027{\cdot}618184 \,.\, t + 0{\cdot}0001221805t^2$
$$= 1296027{\cdot}674055t(1+x) + 0{\cdot}0001106850t^2(1+x)^2.$$

If, therefore, $n = 1296027{\cdot}674055$
$$x = -\frac{0{\cdot}055871}{n} + \frac{0{\cdot}000114955 \,.\, t}{n}$$

To reconcile B and H, therefore, x must contain a variable term. Similar remarks apply to the difference between B and V.

Now let N be the moon's mean motion referred to 1 as the unit of time, and $(N + \delta N)$ the moon's mean motion referred to $(1 + x)$ as the unit of time,

then $(N + \delta N)\ (1 + x) = N$,

$$\text{and } t\delta N = \frac{N}{n} \left\{ 0{\cdot}055871 \,.\, t - 0{\cdot}0000114955 \,.\, t^2 \right\}$$
$$= 0''{\cdot}747 \,.\, t - 1''{\cdot}54\left(\frac{t}{100}\right)^2.$$

But Hansen determined his mean motion of the moon so as to force an agreement between his theory and observations reduced with Bessel's unit 1; and his tables, therefore, represented the observations well for many years, whilst 1 was adopted as the unit of time; but directly the unit of time was changed by the adoption either of H or V, then the effects of the erroneous determination of the moon's mean motion by Hansen became apparent. The change of error in longitude of Hansen's Lunar Tables between 1864, when Le Verrier's Solar Tables were adopted in the *Nautical Almanac*, and 1880, amounts to more than 10″.

The effect of the change of unit is also shown in the comparison of Le Verrier's Solar Tables with observation, but of course only to about the thirteenth part of the amount shown by the Lunar Tables. The necessity of adopting some definite unit of time by fixing the constants in the expression for the longitude of the mean sun is insisted upon.

If $L_0 + n_0t + S_0t^2$ is the expression adopted for the longitude of the mean sun, the quantities L_0, n_0, S_0, must never be changed. The correction δL, which from time to time may appear necessary to obtain the mean longitude of the sun from the longitude of the mean sun must not be allowed to change the adopted values of L_0, n_0, and S_0. The true longitude of the sun will then

$$= L_0 + n_0t + S_0t^2 + \delta L + \text{periodic terms}.$$

It would appear that speculations respecting changes in the time of rotation of the earth on its axis are at least premature until the theories have been revised with a unit of time freed from changes of adopted constants which are at present inextricably mixed up with any effects which would result from a change in the time of rotation of the earth on its axis.

The longitude of the mean sun when properly investigated, differs from the mean longitude of the sun by a secular term—

$$0''{\cdot}3113\left(\frac{t}{100}\right)^2.$$

As this difference has been usually neglected in the determination of the sidereal time at mean moon, an error of about

$$13 \times 0''{\cdot}3113 \,.\, \left(\frac{t}{100}\right)^2, \text{ or } 4''\left(\frac{t}{100}\right)^2$$

has been thrown upon this secular acceleration of the moon's mean motion. This accounts for the difference between Adam's theoretical value, and that deduced from eclipse observations.

Chemical Society, May 3.—Dr. W. H. Perkin, president, in the chair.—The following papers were read:—On a new oxide of tellurium, by Dr. E. Divers and M. Shimosé. When the compound of sulphur trioxide and tellurium, discovered almost simultaneously by the authors and by Weber, is treated in a vacuum, sulphur dioxide is evolved and a new oxide of tellurium is formed containing one atom of tellurium to one atom of oxygen. The decomposition takes place between 180° and 230°. The oxide is black, and quite stable at ordinary temperatures in dry air. No compound of this monoxide has yet been prepared, but in its properties it is essentially different from a mixture of tellurium and dioxide.—On tellurium sulphoxide, by Dr. Divers and M. Shimosé. The authors prepared this compound by pouring sulphur trioxide on to tellurium finely powdered and dried. It was purified from sulphur trioxide by heating to 35° and exhausting with the Sprengel pump. It is a red amorphous solid, quite stable at ordinary temperatures in sealed tubes. When heated in a vacuum to 90° it is changed into a bright fawn coloured modification.—On a new reaction of tellurium compounds, by Dr. Divers and M. Shimosé. When sulphuric acid containing a small quantity of tellurium dioxide or sulphate in solution is poured into a hydrogen-generating apparatus, and the escaping hydrogen passed through a second portion of the telluretted sulphuric acid, a beautiful red colour, due to tellurium sulphoxide, is rapidly developed.—On a simple modification of the ordinary method for effecting the combustion of volatile liquids in Glaser's furnace with the open tube, by Watson Smith. The author causes the end of the combustion tube to project from the furnace, and volatilises the liquid by gently warming the current of gas with a Bunsen burner.—On the production of ammonia from the nitrogen of minerals, by G. Beilby. The author gives the results obtained with typical oil and coal shales when distilled (1) at a low red heat, (2) at a low red heat in a current of steam, (3) at a low red heat in a current of steam, the residual coke being afterwards subjected to the prolonged action of steam, so that a large portion of the coke is consumed and the nitrogen in it liberated as ammonia. Thus a sample of oil shale furnished by (1) 2·7 lbs. of nitrogen per ton as ammonia, by (2) 3·9 lbs., by (3) 12·0 lbs.—On the specific gravity of paraffin wax, solid, liquid, and in solution, by G. Beilby.

Zoological Society, May 1.—Prof. W. H. Flower, F.R.S., president, in the chair.—The Secretary read an extract from a letter addressed to him by Mr. W. L. Crowther, C.M.Z.S., respecting the possibility of obtaining living specimens of the Thylacine of Tasmania.—The Secretary exhibited, on behalf of Mr. H. Whitely, the skin of a Bird of Paradise (*Diphyllodes gulielmi*) from the Island of Waigiou, which was believed to be the second example of this rare species yet obtained.—The Secretary exhibited a set of Radde's international colour-scales, and explained the way in which it was intended to be used.—A

communication was read from Mr. F. Moore, F.Z.S., containing the second part of a monograph of the sections *Limnaina* and *Euplœina*, two groups of Diurnal Lepidoptera belonging to the subfamily *Euplœinæ*. The present paper contained the descriptions of many new genera and species belonging to the group *Euplœina*.—Mr. Alfred Tylor, F.Z.S., read a paper on the colouration of animals, showing that the character of the ornament or decoration differs in the two great divisions of the animal kingdom—the Invertebrata and Vertebrata. Mr. Tylor pointed out that the law of emphasis, well known in architecture, was, in his opinion, applicable to natural history, and showed that the prominent characters of the animal are picked out in colour in precisely the same manner whenever colour is present. He divided his subject into several sections, and exhibited illustrations of the more important families in coloured diagrams.—A communication was read from Dr. O. Boettger, of Frankfort-on-the-Main, containing the description of new species of land-shells of the genus *Clausilia* from the Levant, collected by Vice-Admiral Spratt, F.R.S.—Mr. W. F. Kirby gave an account of a small collection of Hymenopterous and Dipterous insects obtained in the Timor-Laut group of islands by Mr. H. O. Forbes.

Mathematical Society, May 10.—S. Roberts, F.R.S., vice-president, in the chair.—Prof. M. J. M. Hill, of the Mason College, Birmingham, was elected a member.—Prof. Cayley made a communication on Mr. Wilkinson's rectangular transformation.—Mr. Tucker read abstracts of papers by Prof. Genese, relations between the common points and common tangents of two conics; by Mr. W. R. W. Roberts, on the motion of a particle on the surface of an ellipsoid; and by Mr. R. A. Roberts, on unicursal twisted quartics, part ii.; he also made a communication on two concentric circles. The circles considered were a circle which the author proposes to call the "Triplicate-Ratio" (T.R.) circle and Brocard's circle. If through a point (P) within a triangle ABC (whose trilinear coordinates are $2a\Delta/\kappa$, $2b\Delta/\kappa$, $2c\Delta/\kappa$, where κ stands for $a^2+b^2+c^2$), straight lines DPE^1, EPF^1, FPD^1 be drawn, then the circle $DD^1EE^1FF^1$ is the T.R. circle. The origin of the name is due to the fact that the intercepts on the sides (DD^1, EE^1, FF^1) are equal to a^3/κ, b^3/κ, c^3/κ respectively. If $\lambda^2 = a^2b^2+b^2c^2+c^2a^2$ and $\mu = \lambda/\kappa$, then the triangles DEF, $D^1E^1F^1$, which are equal to one another, and are similar to ABC, have their sides $= \mu a$, μb, μc. The lengths D^1E, E^1F, F^1D are equal to abc/κ, so that the perimeter of the above-named hexagon is $(a^3+b^3+c^3+3abc)/\kappa$. Other curious properties were pointed out. If the angle BFD be denoted by ω, then $\cot\omega = \cot A + \cot B + \cot C$, and several other trigonometrical relations were indicated. If through A, B, C lines are drawn parallel to the sides of DEF, $D^1E^1F^1$, these by their intersections determine five of the points on Brocard's circle, the other two Brocard points being P and H (the centre of the circum-circle). Lastly the trilinear equations to the two circles were given, and it was shown that the two circles are concentric. The T.R. circle also divides each side into segments which are in the duplicate ratios of the sides.—The Rev. M. M. U. Wilkinson read a second paper on spherical functions.

Geological Society, April 25.—Mr. J. W. Hulke, F.R.S., president, in the chair.—Rev. William Franklen Evans, Ernest Hall Hedley, and Henry James Plowright were elected Fellows, and Dr. J. S. Newberry, of New York, a Foreign Member of the Society.—The following communications were read:—On the skull of *Megalosaurus*, by Prof. R. Owen, C.B., F.R.S. The specimens described in this communication were obtained by Edward Cleminshaw from the freestone of the Inferior Oolite near Sherborne (Dorset) from some blocks which had been quarried for building purposes. These were sent by him to the British Museum, where the remains have been developed. One block includes a great proportion of the right side of the facial part of the skull, the missing parts being the fore-end of the premaxillary, the suborbital end of the maxillary, and the upper hinder pointed termination of the same bone. Ten teeth are preserved in the maxillary bone. Another block contains the outer side of the right mandibular ramus with teeth and with some other fragments. In a third block is the anterior part of the left mandibular ramus with portions of the teeth. These remains were described in detail; and in conclusion the author discussed the bearing of these and other Megalosaurian remains upon our knowledge of the structure of that animal and its affinity to existing Reptilia, and criticised some of the evidence on which the relationship of the Dinosauria to birds is inferred, a relationship which he had suggested in 1841, but upon grounds which appeared to him to be more satisfactory.—Notes on the Bagshot sands, by Mr. H. W. Monckton, F.G.S.—Additional note on boulders of hornblende picrite near the western coast of Anglesey, by Prof. T. G. Bonney, F.R.S.

Institution of Civil Engineers, April 8.—Mr. Brunlees, president, in the chair.—The paper read was "On the Diamond Fields and Mines of South Africa."

EDINBURGH

Mathematical Society, May 11.—Mr. J. S. Mackay, F.R.S.E., president, in the chair.—Mr. D. Munn, F.R.S.E., gave an address on the geometry of the nine-point circle, and Dr. C. G. Knott, F.R.S.E., a paper on Newton's "Opticks."

BERLIN

Physical Society, April 30.—Dr. Pringsheim reported on his recently published measurements of the wave-lengths of the least refractive rays of the solar spectrum. In order to obtain them he used a radiometric torsion apparatus, similar to those used by Crookes, which carried a small mirror by which the revolution of the torsion-beam caused by the ray could be observed. The source of light were solar rays reflected into a dark room by a heliostat, first united in a focus by a concave mirror, then rendered parallel and directed upon a revolvable grating-mirror, which produced a whole series of spectra. The various divisions of the first spectrum were directed upon the torsion-apparatus by means of a slit, and it was noted up to what wave-length the mirror still performed part of a revolution. In order to exclude the visible rays of the second spectrum which were mixed with the infra-red ones of the first spectrum, and prevent their reaching the torsion-apparatus, Dr. Pringsheim cut them off partly by an iodine solution, partly by an ebonite plate according to Abney. The extreme limit of the spectrum where an effect was still observed was at the wave-length $\lambda = 0{\cdot}00152$mm.—Prof. Neesen reported upon a treatise entitled "On the Contractions of Volume as a Measure of Chemical Affinities," sent to the Society for publication in its *Transactions* by Herr Müller Erzbach of Bremen. He shows in a number of salts formed by selenic and chromic acids that in chemical combination a stronger contraction of volume corresponds to a greater chemical affinity, and is shown in the heat generated when the combination takes place; while in those salts which show a smaller contraction, acid and base are bound together by less powerful affinity.

CONTENTS

THURSDAY, MAY 24, 1883

SCIENCE AND ART[1]

WE stated in our article last week that we should take an early opportunity of noticing some of the pictures in this year's Academy, with especial reference to the points which we then mentioned. The following notes are a fulfilment of that promise. It is to be understood that only those pictures which illustrate, either by their perfection or their defects, the points in question have been referred to. The pictures have been classified according to the particular class of natural phenomena which they portray.

SKY COLOUR

2. "Homewards," Wm. J. Monkhouse Rowe. Sky not zoned; impossible colour of clouds. Perhaps our word zoned requires some little explanation. When the sun is at such an altitude above the horizon that the blue rays are absorbed, and there begins to be colour on cloud and sky, then the sky at the same altitude is always of the same colour, and the colours gradually modulate from the warmest at bottom to the coolest at top. These remarks with regard to the sky have, of course, nothing whatever to do with the clouds, but whatever the colour of the clouds may be, and this will depend upon various conditions, any true clouds, however they may be coloured, shown on the picture at the same altitude, will be surrounded by the same sky colour, even if the intensity differs in consequence of different distances from the sun's place. For instance, if a painter chooses to put a bright green sky at 5° or 10° elevation on the right of his picture, and then paints a blue sky at the same elevation above the horizon on the left, he is showing something which is impossible: his picture is not perfectly zoned.

76. "Welbourne Hall, Yorkshire," H. A. Olivier. Sky well zoned, but its reflection from water too intense. Reflection will naturally lower the tone of the reflected light, but, all the same, this difference may not come out very strongly, for the reason that the reflection is most frequently and necessarily shown in a darker part of the picture, so that although by mere contrast the tone seems lowered, it will yet appear to be brighter than the tone seen in the region of more general illumination.

132. "To Pastures New," James Guthrie. Sky of impossible colour.

121. "Freshening," A. Harvey Moore. Sky and sea both admirable.

157. "Corrie, Isle of Arran," John MacWhirter, A. Sky admirable; water a little doubtful. It is also not level.

218. "The Dogana and the Island of San Giorgio, Venice," Frank Dillon. Sky colour and water reflection good.

233. "After Sundown," Frances R. Binns. Excellent in sky colour, but zoning might have been more perfect.

242. "Superstition," Everton Sainsbury. Good and bold sky colour, especially the red, but the sky is too much worked to resemble cloud.

246. "Autumn," A. Glendening, jun. Good, and forms of clouds quite excellent.

247. "The Forgotten Sheaf," F. S. Walker. Zoning gone wrong; green never rests on white, nor on gray.

269. "And the Unclean Spirits went out of the Swine," Briton Riviere, R.A. Bold and perfect sky and clouds.

297. "Windsor," Vicat Cole, R.A. Might have been more evenly zoned.

315. "A Mortally Wounded Bandit Chief Exhorting his Comrades to Return to an Honest Living," J. R. Herbert, R.A. There is no relation between the light, the colour of the sky, and that of the landscape.

327. "Grouse-driving on Bowes Moor, Yorkshire," George Earl. It is difficult to understand by what means the sky to the left is illuminated.

331. "Carting for Farmer Pengelly," J. C. Hook, R.A. Sky and clouds admirable; green on the cliff very striking.

371. "A Silent Pool," Fred. E. Bodkin. A slight change in the colour of the clouds would make this an admirable picture.

394. "November," E. A. Walton. Let us hope this is not true sky colour. Far too deep a green, and there is no reason why the clouds at the top of the picture should not be as intense in their lower portions as the mass of cumulus on the horizon.

398. "Ben Ray," H. W. B. Davis, R.A. There is not sufficient relation between the colour of the sky and the colour of the landscape. (Compare 702.)

700. "Trabacolo Unloading at the Custom House, Venice," Clara Montalba. The sky colour is wrong. There could have been no green where the artist has placed it.

702. "At Kinlochewe," H. W. B. Davis, R.A. (compare 398). In this case the sky is wedded to the landscape, and we have a perfect and harmonious whole beautifully luminous.

713. "A Summer Evening, Folkestone," W. Ayerst Ingram. Nearly perfect zoning of cloudless sky, but the reflection from the water has been a little too much toned down perhaps.

773. "Winnowing Gleanings," H. Gillard Glindoni. Sky and seascape both admirable.

826. "The Boundary of the Heath," J. C. Harrison. Careful study of sky. Bank of trees against it very effective.

793. "Rochester from the River," Charlie W. Wyllie. A pleasing picture—both sky and water good.

1438. "Leaving Labour," E. B. Stanley Montefiore. Impossible green sky.

1503. "Lost Sheep," Robert Page. It is a pity this artist takes the trouble to paint a sky, because it is evident he does not know the difference between sky and clouds.

1483. "A Spanish Aqueduct," Adrian Stokes. Note colour of sky and landscape and effect of heat under tropical sun.

CLOUDS

225. "On Solway Sands," Thomas Hope M'Lachlan. Blue clouds.

257. "Still Waters run Deep," George Chester. The clouds in this picture are hideous in form and impossible in colour.

339. "Night into Day," Vincent P. Yglesais. This may be a view in Mars. It is fortunately impossible here.

577. "Rye, Sussex," Leslie Thompson. A new kind

[1] Continued from p. 51.

of cloud is here represented, one resembling mashed potatoes.

602. "A Calm—Bay of Naples," F. W. Jackson. Good study.

1461. "Between the Showers," Henry Moore. Good study of clouds.

DISTANCE AND ATMOSPHERE

279. "Gathering the Flock," H. W. B. Davis, R.A. Perfect distance, toning carefully preserved, and the picture is free from the exaggeration which one so often laments. The illumination of the sheep is, however, too local.

321. "Highlands and Lowlands," William Linnell. Admirably managed distance.

479. "Light in the West: After Rain," Alfred W. Williams. The artist seems to have too strongly contrasted the peaks against the sky. The peaks though high are really distant, and hence there is atmosphere between us and them.

96. "Snowdon," Joseph Knight. Everything that could be desired.

255. "Llyn-yr-Adar on the Adder's Pool, Carnarvonshire," J. W. Oakes, A. This is the picture referred to at length in the preceding article. We repeat that it is a pity the author did not study the rainbow before he attempted to paint it.

843. "Spring Time at Tillietudlam Castle, Lanarkshire," David Murray. The effect of sunshine on grass is here carefully rendered, but there is a little too much colour in the delicate clouds in the centre of the picture.

SUNSETS

98. "Parting Day," B. W. Leader, A. Careful study, rifts admirably attempted, but the treatment of them is not quite perfect, especially on the right of the picture. As a rule the clouds must be lower down than they are here represented to give the effect sought to be rendered.

Note on Rifts.—These rifts, which have attracted the attention of Mr. Leader, are only possible when the air is very densely charged with aqueous vapour, for the reason that they are a projection upon the distant sky of a cylinder, less illuminated than the surrounding air, owing to the interposition of a cloud low down in the atmosphere. Now, as whatever the condition of the air may be it must obviously get more dense as the earth is approached; the lower the cloud the stronger will be the rift, and the more the cylinder of shadow is directed to the point overhead the more definite will be the rift, for the reason that along the line of sight the greatest distance will then be in shadow. We are really in such a case dealing with a partial eclipse of a long column of air, and as at any one place the conditions of the atmosphere at the same time will be almost, if not quite, identical, if rifts produced by clouds are shown on one side of the picture, the other side of the picture should show rifts produced in like manner. This we think is a point which Mr. Leader has very pardonably missed. The reflection of the clouds in the water is not quite true.

164. "Sunset Fires," John MacWhirter, A. This picture is spoiled because the artist has made no distinction between the colours of the sky and of the clouds.

399. "At Last!" Fred. C. Cotman. Sunset and water reflection; a beautiful picture.

1471. "An Autumn Evening," B. W. Leader, A. This is a very fair sunset sky, but a little too cool in colour, and the reflection in the water is not good. There is however a careful bit of painting in the way in which the top of the cumulus is reflected over the bridge.

MOONS, &c.

214. "Tipt with Eve's Latest Gleam of Burning Red," James S. Hill. It is quite impossible that such a moon should be at such a height at sunset, besides which the moon is more shapeless than she should look under the given cloud conditions.

232. "Too Late," Frank Dicksee, A. Everything about this picture when we leave the figures, with which we are not at present concerned, is wrong. We have an impossible moon in an impossible sky. The artist has attempted to paint the old moon in the new moon's arms, one of the most beautiful natural phenomena visible after sunset, but by a strange fatality almost every point where science could have assisted the artist has been neglected. As is known to many children, the appearance of the complete body of the moon on such an occasion as this, when only a very thin crescent is illuminated by the sun, depends upon the fact that the earth reflects light to the moon, hence the term "earth shine," the equivalent of "la lumière cendrée" of the French. Under these conditions we have in fact a thin crescent illuminated by direct sunlight, whilst the rest of the moon is illuminated by light reflected from the earth. One of the points of this earth illumination which Mr. Dicksee has entirely missed is this, that the earth-light must be equally distributed over the whole surface of the moon which it illuminates. Sunlight reflected from the earth to the moon, and then reflected back again from the moon to the earth, must be quite general in its action, and must equally light up each part of the lunar surface. Hence the absolutely equable illumination which is always seen, but which this picture fails to show. The fact that the old moon thus illuminated appears to rest in the new moon's arms depends upon irradiation, by the operation of which a thing very brilliantly illuminated looks larger than when it is dimly illuminated. That part of the moon, therefore, which shines by the brilliant light of the sun, appears to belong to a larger body than that part which receives its less brilliant illumination from the earth. This appearance is so obvious that it has given rise to the old world illustrations, in which the "old" moon is represented as in a boat, because in fact the two horns of the crescent moon extend beyond the old moon as we have said, and appear to form part of a larger circle. This point also Mr. Dicksee has entirely missed. What we have to say touching the colour of the sky is, that neither Mr. Dicksee, nor any one else, ever saw such a colour as he has painted to indicate the place of sunset. At such a height above the horizon a green colour is impossible, it must either be red, or yellow, or grey, according to the state of the atmosphere at the time. We have ventured to speak thus at length with reference to this picture, because we consider it a very typical case, and surely Mr. Dicksee, when he becomes acquainted with the facts to which we have drawn attention, and as to which there can, we believe, be no dispute, will regret that he should have disfigured his

picture by disregarding them. Had these things been correctly painted the picture would have been just as beautiful to the ignorant as it is at present, whilst it would have had the additional advantage of being also pleasing to look upon by those who can in its present form only regard it with regret. It may be said that these are simple matters. Be it so. Simple or not they are typical, and that is the point we wish to urge.

260. "The Ides of March," E. J. Poynter, R.A. The comet is admirably rendered, but does not the little lamp give out rather too much light?

697. "The Dawn of Night," Richard Whatly West. It is not easy to understand this picture, as the title of it is "The Dawn of Night"; but if we have the moon rising then the sky is far too dark.

807. "Can He Forget?" Edward H. Fahey. This artist is to be congratulated on his moon. He has painted it so that any one with a very simple calculation can determine that he is quite wrong. If the young lady asking the interesting question had had a crown in her hand, the coin would have covered the real moon. She herself would hardly cover the false one. So, to one who knows, it looks like Nadar's balloon without the netting being inflated. Further, each part of the moon when she rises is generally under the same atmospheric conditions, so that such a variation in its illumination as is here shown is almost impossible unless a definite cloud is darkening its surface, and no such cloud is here to be seen.

888. "Moonlight," Robert Jobling. It would have been better if in this moonlight scene the artist had discriminated more between the clouds and their background the sky.

546. "Moonlight Bay, Milford Haven," F. W. Meyer. Very careful study of moonlight.

Water

28. "Catching a Mermaid," J. C. Hook, R.A. Colour and forms of waves, and swirl and dash on rocks, good.

133. "Wind against Tide: Rillage Point, Ilfracombe," J. Geo. Naish. Sea and sky excellent in the distance; water a little weak in foreground, both as to colour and forms of waves.

202. "Oyster Dredgers," C. Napier Henry. Water in this picture is not level.

281. "High Tide at Kynance, Cornwall," Sidney R. Percy. The wave at the left is being forced back by nothing, and is altogether too solid.

282. "A Rising Gale: Dunbar Sands, Padstow, Cornwall," Walter J. Shaw. Very bold low front view of breaking waves. Reflection good.

301. "The Gull Rock: off Kynance Cove, Cornwall," Edmund Gill. Colour and form of water quite admirable.

467. "A Travelling Cobbler," Joseph Henderson. Colour of water very true to nature. The distant land deserved more careful painting.

498. "The Last of the Crew," Briton Riviere, R.A. This ice is a careful study of form and colour.

495. "A Fisherman's Garden," Theodore Hines. Water not level.

711. "The Sad Sea Wave," John Francis Faed. Colour of water very brilliant.

695. "Lobster Fishers," Colin Hunter. Form of waves very careful study.

778. "A Haven," C. E. Holloway. Note colour of water and form of waves. The artist should say where this Haven is, that it may be avoided by all who love the beautiful.

809. "Welsh Dragons," John Brett, A. Note colour of water and rocks. Mr. Brett is again quite perfect in his treatment, but we rather doubt the colour of some of the cumuli that float over the sea, and also their sharp darkened boundaries.

145. "Adrift," R. C. Leslie. An admirable study of water, far too good to be skied.

Reflection

36. "Love Lightens Toil," J. C. Hook, R.A. The reflection from the water has not been carefully studied. Does not the green of the grass come too low down to the water? The water, too, is not level. Current indicated.

86. "A Quiet Noon," Peter Graham, R.A. The reflection of the clouds from water is not quite in accordance with their form; the clouds themselves are admirable.

83. "The Enchanted Lake," Albert Goodwin. Careful study of reflection. The artist has left out what most artists would incontinently have put in, but there are several blunders; for instance, it is the under side of the umbrella which should have been reflected, and not the upper one, and the colours of the objects reflected are too entirely lost, as if the reflection were from the bottom of the enchanted lake instead of from its surface. The artist's idea has evidently been that there have been two transmissions of the light through the water, in consequence of which its original colour has been lost. This cannot have been so.

123. "On the Marshes," Percy Belgrave. The ordinary laws of reflection do not seem to apply in this case.

162. "Loch Scavaig, Isle of Skye," Sydney R. Percy. Even if the moon had not been veiled by a cloud we could not get this effect, nor with such rough water would the wake alone have been so illuminated; we should have had side reflections as well.

168. "Loch Alsh," Colin B. Philip. Good; distance and water reflection carefully managed

356. "Among the Trawlers, Tarbert, Loch Fyne," Andrew Black. Reflection from water very admirably managed.

508. "Green Pastures and Still Waters," B. W. Leader, A. The reflected images of the trees in the distance are about one and a half times as long as the trees themselves. Still water does not magnify the height of objects when they are reflected in it. One of the branches of the tree to the left has also considerably suffered by the reflecting process.

648. "A North Country Stream," Alfred W. Hunt. Very perfect study of water, the light reflected by the surface being mingled with that coming from the bottom.

688. "Willows Whiten, Aspens Quiver," Keeley Halswelle. Admirable landscape and water, but the colour and shapes of the clouds are unsatisfactory, and the picture would be better without them.

1509. "A Pebbled Shore," Colin Hunter. Note the way in which the cumuli are reflected from the waves beneath them. Glorious picture.

1493. "Toil, Glitter, Grime, and Wealth on a Flowing Tide," W. L. Wyllie. An admirable picture, but we question whether the artist is justified in getting such a brilliant reflection from the surface of the water to the

left, where the sky as indicated does not appear to be a very luminous one.

142. ". . . these Yellow Sands," John Brett, A. In this admirable picture, in which the sea and sky are quite perfect, Mr. Brett has attempted some difficult effects. More transparent water has never been seen on a canvas, and the colour of the yellow sand at its bottom is beautifully mingled with the light reflected from its surface.

626. "Sounding for Shallows at Low Nile," Tristram Ellis. A bold attempt at reflection in the Nile water, but, as a matter of fact, the real colour is not so entirely subordinated by reflection.

SNOWSTORMS

764. "The Joyless Winter Day," Joseph Farquharson. The storm must have been very considerate to the artist. In spite of the driving blast there is not a single snowflake to be seen in the first twenty yards.

THE LIVING ORGANISMS OF THE ATMOSPHERE

Les Organismes vivants de l'Atmosphère. Par M. P. Miquel, Docteur ès Sciences et Docteur en Médecine, Chef du Service micrographique à l'Observatoire de Montsouris. (Paris: Gauthier-Villars, 1883.)

PLUS occidit aer quam gladius, such is the main idea contained and explained in M. Miquel's very able and interesting book. If the modern theories are true, it must be certainly conceded that although the sword and gun are very murderous tools, air is yet more so. But on the other hand one may say of our atmosphere's murderous propensities what a French writer said when he was told that coffee was a poison: "Well, it may be a poison to be sure, but it must be a very slow one; I have been indulging in it for over fifty years." In fact, if Voltaire and many other men took too much of it, it began to tell on them only very late. Taking it for granted that coffee is murderous, it must be also granted that it is not always so. Such is also the case of the atmosphere we live in.

The influence of infinitely small organisms contained in the air and water, as well as in the body of man and animals, can no longer be denied, at least, in a general manner. Certainly much remains to be done to bring the Microbe Theory to the point it must attain; many inconsistencies and discrepancies yet interfere with its general harmony; but Davaine's and Pasteur's experiments and discoveries have certainly opened new ways in science.

Now that it is granted that the organisms alluded to are to be found and may thrive in the air, it is interesting to know what these are, how abundantly they may be found in the atmosphere, and by what means they may be captured and experimented upon. To these important questions M. Miquel answers in a very precise and interesting manner.

It is not a difficult thing to detect the corpuscles contained in the atmosphere; a mere sunbeam in a room shows hundreds of them dancing in the light. But it is less easy to ascertain the nature of these little atoms; great skill is required to do that. Some are vegetable, some are mineral, some are animal.

M. G. Tissandier has established that a great quantity of mineral atoms is contained in the atmosphere; the most interesting of these are meteoric iron melted into the form of little globules. Some infusoria are also to be found, but bits of wool and silk, pollen and spores are more abundant. As one may easily believe, all these corpuscles are less abundant in the atmosphere after a fall of rain. For instance, M. Tissandier finds in a cubic metre of air 0·023 gramme of dust after a rainless week; 0·006 gramme the day after a heavy rain.

The description given by M. Miquel of the numerous instruments contrived by himself and by others to collect the corpuscles contained in the air is good and interesting, but is not easily condensed. Another very important chapter of this book is that concerning the nature and origin of the aërial corpuscles among which pollen, flour, and spores are most abundant. For instance, the number of spores to be found in a cubic metre of air is about 14,200. But this number changes very much according to the season. In winter the mean number is 6200; in spring, it is 13,000; in summer, 28,000; in autumn, 9800. The reason of these variations is easy to understand.

However abundant spores and pollen, woollen and silk threads may be in the air, that is a question of little importance when compared with that of the presence of bacteria in the atmosphere. Bacteria are to be found, often in great quantity, in the air. Generally speaking, according to M. Miquel's experiments and observations, bacteria are more abundant when the weather is dry; the reverse is to be observed concerning spores of inferior cryptogams. The direction of the prevailing wind has much to do with the number of bacteria found in the air. M. Miquel shows, by means of a diagram, how the air having passed through part of Paris, before coming to the Montsouris Observatory, contains more bacteria than that which passed only over the suburbs and country around the town. South winds bring from 42 to 77 bacteria to a cubic metre of air; northern ones bring from 108 to 152. Other experiments give the same results. M. Miquel draws from his numerous experiments the conclusion that the air in Paris contains nine or ten times more bacteria than does that outside of the fortifications or close to them. For instance, in the Rue de Rivoli, M. Miquel finds an average number of 760 bacteria in autumn, 410 in winter, 940 in spring, and 920 in summer; that is, a mean annual number of 750 bacteria per cubic metre of air. At Montsouris the mean annual number is 75. The minimum number found by M. Miquel is 45 (winter 1882); the maximum is 3000 (summer 1881) bacteria per cubic metre.

In hospitals, the air contains a much greater quantity of bacteria, as might be expected; the cubic metre contains an average of five or six thousand! In some cases M. Miquel has found ten, even sixteen, twenty-one, and *twenty-eight thousand* bacteria per cubic metre of air. These last numbers are stupendous.

These bacteria in the air, liable every moment to penetrate into our lungs and body, are of many sorts. Some are spherical,—the *sphero-bacteria*; they generally have no power of locomotion; some are coloured red or yellow. M. Miquel remarks that although some of these bacteria must exert a pathogenetic action, he has not been able to produce any disease in ani-

mals by means of these organisms. It may be that the atmosphere kills these bacteria, it may be that the animals experimented upon were not liable to catch the disease; at all events it would seem that no pathogenetic bacteria are to be found in the air. This is a very important conclusion, but it is not yet sufficiently supported by facts. How could scarlet fever, measles, and other diseases be brought by a physician from a patient to a healthy person if the bacteria could not resist the action of the air for some time?

Other bacteria present a more elongated shape: they are called *bactéries en batonnets.* They generally move about, sometimes very slowly, sometimes with great rapidity, in various manners, when they are allowed to remain in a suitable liquid. M. Miquel has remarked that one of these bacteria converts sulphur into hydrosulphuric acid in a very energetic manner; together with another similar bacterium it is the principal agent that converts urine into sulphuret of ammonia. M. Miquel cannot say exactly as to the presence of pathogenetic bacteria in the atmosphere, nor especially as to their precise nature and *modus faciendi.*

Bacilli are also to be found in the atmosphere; they may be long or short; the less they move about the longer they become. One of these bacilli resembles very much the *Bacillus amylobacter* (van Tieghem). Another one seems pathogenetic; it brings on, in animals, a phlegmon that generally terminates—as is the custom of most phlegmons—in suppuration. Of course many other pathogenetic bacilli perhaps exist in the atmosphere, but that question has not been specially discussed by M. Miquel. He shows very well how considerable an influence the rainfall exerts on the number of the bacteria contained in the air. Temperature has little to do with this as diagrams show; rain on the contrary has a great effect. As soon as the weather becomes dry the number of the bacteria increases; when it is rainy this number falls rapidly. This result is one of the most important among those M. Miquel has attained, inasmuch as this *savant* shows that rainy periods are those during which the bacteria multiply.

If the number of these organisms is considerable in the air we breathe every day, one thing must however console us in some degree. If these bacteria are murderous, they are somewhat like the coffee; they kill very slowly in most cases. Many of them must each day come into our lungs and body, and yet we feel none the worse for it generally. This does not mean that they are not dangerous; it means only that they are not always able to act a dangerous part. For what reason, we know not yet. Typhoid fever, cholera, yellow fever, measles, scarlet fever, and a great many other diseases are contagious; but all persons who live with patients suffering from either of these diseases do not catch them. Most doctors and medical students do not catch any contagious disease in the hospitals, and yet they doubt not the nature and danger of these diseases.

Whatever opinion one may entertain as to the Microbe Theory, it must be admitted that M. Miquel's book is exceedingly useful and well arranged. M. Miquel understands the matter thoroughly, and his book will certainly be much read abroad, as it has been in France.

HENRY DE VARIGNY

ANIMAL TECHNOLOGY

Animal Technology as Applied to the Domestic Cat. An Introduction to Human, Veterinary, and Comparative Anatomy. By Burt G. Wilder, B.S., M.D., and Simon H. Gage, B.S. (New York and Chicago: A. S. Barnes and Co., 1882.)

MESSRS. BURT WILDER AND GAGE are not the first anatomists to employ the domestic cat as an introduction to the study of vertebrate anatomy. In 1881 Mr. St. George Mivart published an elaborate treatise on the Cat, as a type for examination and comparison with other vertebrates; and as far back as 1845 M. Straus-Durckheim issued his well-known work in the French language on this animal.

The book now before us differs however in its scope and mode of treatment from its English predecessor. It is not like Mr. Mivart's, a systematic treatise on the anatomy of the cat, both macroscopic and microscopic, with chapters on its development, psychology, specific forms, geographical distribution, &c. But it is a practical treatise written with the object of instructing the student in the methods of dissecting and displaying the structure of this animal.

As preliminary to the anatomical description, the authors have written some short chapters on the instruments employed in dissecting, the modes of using them, the methods of injecting, and the preparation and preservation of anatomical specimens, so as to justify the title of Anatomical Technology given to the book. We would especially direct attention to the sections on the maceration of bones and the preparation of skeletons as furnishing the young anatomist with useful hints on these subjects.

Those who are familiar with the papers on Anatomical Nomenclature by Prof. Wilder in the American Journal *Science*, and elsewhere, will not be surprised to find that he has in this work again enunciated his views on Terminology, and adopted many but little used, as well as new terms in his descriptions. There can be no doubt that the terms used in anatomical description in many instances would be improved by being altered. No one who is engaged in the comparative study of the anatomy of the human body, with that of other vertebrates, but must constantly feel a difficulty in the use of the terms employed to express position. He has ever to keep in mind that a surface which is superior in man is anterior in any other vertebrate, and that a surface which is posterior in man is superior in vertebrates generally. Hence such terms as dorsal and ventral, cephalic and caudal, are much to be preferred to express corresponding surfaces throughout the vertebrata, whatever may be their direction, than posterior, anterior, inferior, superior. If indeed the recommendations made by the Edinburgh anatomist, Dr. Barclay, in the early part of this century, had been attended to, then anatomical description would by this time have been on a much more satisfactory basis than it is. The delay and difficulty in effecting the necessary reforms are largely due to the works on human anatomy having been for the most part written by men, who are specialists in that department only, and have not had a wide and philosophical training in the whole subject. The introduction, however, of biological study into

the scheme of a general education, and the publication of such books, as the one now before us, as guides to a practical knowledge of the structure of animals, will break up the conservative instincts of the purely human anatomists, and will lead in time to the adoption of a more scientific nomenclature.

To turn now to the descriptive part of this book. The impression we have derived from its perusal leads us to say that it is well adapted to the purpose for which it has been written. The authors have evidently studied the anatomy of the cat, not from the dissection of a single animal, but from numerous specimens. The methods of displaying structure, and preserving the parts for future observation and study are workmanlike and practical. The descriptions are clear and concise. Though at times terms are employed, such as ectal for external, ental for internal, trochiter for the great tuberosity of the humerus, and trochin for the lesser tuberosity, which are novel, and at first require a little thought to gather their meaning, they soon become familiar, and without doubt conduce to give clearness and accuracy to the description.

We ought not to omit to say that, as preliminary to the description of the cat's brain, the authors give an account of the dissection of the brain of the frog and the Menobranchus.

The work is illustrated with 130 figures in the text, and with four lithographed plates of the brain of the cat. The plates are neatly executed; but the figures in the text are in many cases coarse and inartistic. Surely in the United States, where the art of engraving on wood, as is shown in the illustrations to Scribner's and other monthly magazines, has attained such a high order of excellence, the authors ought to have been able to procure a draughtsman and woodcutter who could represent muscles, more like nature, than is given in say Figs. 66, 67, and 72.

OUR BOOK SHELF

Magyarország Asványai, Különös tekintettel termöhelyeik megállapitására. (*The Minerals of Hungary, with Special Regard to the Determination of their Occurrences.*) By Michael Tóth, S.J., Professor at the Gymnasium, Kalocsa. (Budapest, 1882.)

WE have here a contribution to science which reaches us from the far east of Europe, from Hungary. The author has aimed at nothing less than to give a complete catalogue of all the minerals that occur in that country, noting the exact place of the occurrence of each, and adding such statistical and other information as may enable the reader to form a judgment as to the economic value of the subject of the article. Special attention is given to such minerals as are of recent discovery or of such importance as to be likely to affect the future history of the district in which they are found.

Prof. Tóth is, we believe, the first writer who has attempted a complete account of the minerals of Hungary. His work would have been more widely useful had he seen fit to employ some language that is more widely known than his native Hungarian. But in the case of a work like this, which consists largely of names of places and of those technical names of species which are common to all the civilised world, the unfamiliar tongue does not render the book altogether useless. The author would seem to have looked forward to his work being used in England, for he has prefixed an English title-page, and frequently refers to the collections in the British Museum and the Museum of Practical Geology.

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications.*

[*The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to insure the appearance even of communications containing interesting and novel facts.*]

Natural Selection and Natural Theology

THE amicable discussion between Dr. Romanes and myself, "endeavouring to help in determining the true position of an important question," has now (in NATURE, vol. xxvii. p. 527) reached a critical point, one seemingly capable of settlement by scientific inquiry, and upon which a brief note may be pertinent.

I take Dr. Romanes now to agree with me that the physical distinction of the less fit organisms, or, more generally, that the action of the environment, is not in a proper sense the cause of the advantageous variations of surviving organisms; also that natural selection does not explain and has no call to explain the cause of variation. As to this, he says, the theory merely supposes that variations *of all kinds and in all directions* are constantly taking place, and that natural selection seizes upon the more advantageous. Now if variation in animals and plants is lawless, of all kinds and in all directions, then no doubt the theory of natural selection may be "the substitute of the theory of special design," so as to efface that evidence of underlying intelligence which innumerable and otherwise inexplicable adaptations of means to ends in nature was thought to furnish. If it is not so, then the substitute utterly fails. For omnifarious and purely casual variation is essential to it in this regard. F y is it said that "the theory merely supposes" this. For omnifarious variation is no fact of observation, nor a demonstrable or, in my opinion, even a warrantable inference from observed facts. It is merely an hypothesis, to be tried by observation and experiment. I am curious to know how far the observations and impressions of the most experienced naturalists and cultivators conform to my own, which favour the idea that variations occur, in every degree indeed, but along comparatively few lines. That the investigator of any flora or fauna should so conclude as to actual and accomplished variation, is natural, but may go for little, the theory of course supposing that numberless non-occurring forms have failed in the struggle and disappeared. But there is no evidence that all sorts of varieties ever appeared or tended to appear, and there is a musty maxim about "*de non apparentibus et de non existentibus*" which is not devoid of application.

Moreover, as to the vegetable kingdom, it would seem that this question of omnifarious variation may be tested in the seed-bed and the nursery, from which Darwin took the idea and the term of natural selection. These indeed are actual experiments—very numerous and extensive—for the testing of incipient variation. If experienced nurserymen, gardeners, and others who raise plants from seed in a large way, usually with eyes watchful for variation, would give their testimony in this regard, they might materially contribute to the settlement of an interesting question.

We need not hold Dr. Romanes to the terms of his fundamental supposition, "that variations of all kinds and in all directions are constantly taking place." He probably means only that incipient variations are wholly vague and irrespective of ends—are as likely to occur in the direction of unfitness as of eventual fitness to the environment and to use, the divinity that shapes the ends—if ends there be—acting only through the surroundings. And we all understand that the particulars in which progeny differs from parent are potential in the germ, or in the cells of which the germ consists, and therefore wholly beyond observation. The upshot is, that, so far as observation extends, it does not warrant the supposition of omnifarious and aimless variation; and the speculative assumption of it appears to have no scientific value.

ASA GRAY

The Fauna and Flora of the Keeling Islands, Indian Ocean

I HAVE only recently been able to obtain my copy of Mr. Wallace's "Island Life," in which I find an estimate of the fauna and flora of the Keeling Atoll in the South Indian Ocean. I had the fortune to visit that outlying spot in the year 1879,

and made a collection both of its plants and of its animal life. With the exception of my birds and a few of the insects, my collections were destroyed by sea water, so that it is now impossible for me to give a definite list, but I may note that rats were in such numbers as to have become almost a plague. A goodly herd of introduced *Rusas*, a cross between the Sumatran (*C. equinus*) and Javan (*C. Hippelaphus*) species, were in excellent condition, and were living wild on Direction Island, where also pigs were living in the same state. Among birds, the *Gallus bangkiva* (introduced) was in considerable numbers; I saw also the nest of the *Ploceus hypoxanthus*, which comes, not every year, but very often to breed there, but the progeny seems either to die or to return to Java (?). I did not see the snipe, but of the *Rallus philippinus* I got several specimens. Egrets, blue and white, abounded and rested on the high trees on some of the islands. Lizards of several species are now found on most of the islands in large numbers. Of insects the number of species is very considerable. Coleoptera were represented by *Melolonthidæ*, *Cetoniidæ*, *Carabidæ*, *Elateridæ*, *Chrysomelidæ*, but as I have not my journals of that date by me, I cannot recall other families nor state the number of genera represented. Of Hemiptera I caught a good many species, mostly of small size. Many species of ants were observed. Neuroptera are represented, unfortunately, by the *termite*, introduced some years ago in furniture, it is said, but it occurs now on every islet of the group in myriads. I am told that during the cyclone of a few years ago, the whole surface of the sea was covered with the mangled bodies of dragon-flies for miles out to sea, but that since then very few have been seen. Of Lepidoptera I caught many species both diurnal and nocturnal, some very handsome, of which I sent a small collection to London in 1879. The Atlas Moth is rather common. Orthoptera were represented by the ubiquitous cockroach, and a few *Acridiidæ*.

Mr. Ross told me that on several occasions the large fruit bat, called the flying fox, has reached the islands, and once a pair arrived together, but died, from exhaustion apparently, soon after arrival. Under favourable circumstances, as in the case of an unusually strong pair, these may yet become inhabitants of the islets.

There are, I believe, considerable additions to the flora since Mr. Darwin's visit. It is only within recent years that the islands have become so greatly covered with cocoanut plants. Their original vegetation consisted principally of "iron wood" (sideroxylon ?) and other trees, and of low shrubs. These were nearly all burned out by accidental fires, one of which burned for three months. HENRY O. FORBES

Fatunaba, Timor Dilly, January 21

"Festooned" or "Pocky" Clouds (Mammato-Cumulus)

UNDER one of these names letters have appeared at different times in NATURE, notably on October 19, 1871. These were followed by a paper read before the Meteorological Society by Mr. R. H. Scott in February, 1872, in which he collects all the observations which had then been recorded, and the theories which had been propounded to explain them.

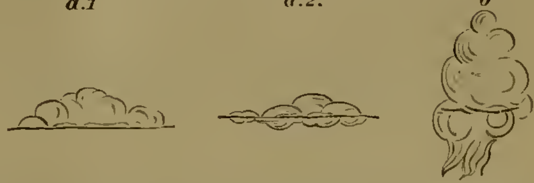
For several years I have been watching this kind of cloud, and I think that its formation is capable of a very simple explanation, partially in agreement with that suggested by Mr. Jevons in the earliest notice of these clouds (*Phil. Mag.*, July, 1857). The name is applied to a peculiar festooned appearance sometimes seen below cumulus and stratus clouds. In Orkney Mr. Clouston has found that it is usually followed by a severe gale; but in Lancashire, where the festoons are called "rain-balls," it is only considered a sign of rain. Other observers in the tropics have also seen it with thunderstorms, and not necessarily with wind. In this country I have observed it both in heavy gales and also in an ordinary summer thunderstorm. The method by which I have endeavoured to discover its origin has been to try and trace its life-history; that is to say, to follow its growth from other forms of cloud and to watch the forms into which it develops.

On one point almost all observers are agreed, that the festoons are frequently seen just before a cloud begins to break up. The first time that I was fairly able to trace the formation of the cloud was one summer evening in London, when towards sunset a flat-based cumulus, like that marked *a* 1 in the figure, suddenly became festooned at the base and diminished on the top, as marked *a* 2 in the figure. A few minutes afterwards the whole cloud evaporated. The succeeding night was fine. The explanation which immediately suggested itself was that the ascentional current which had formed the flat-based cumulus had suddenly failed, and that the festoons were simply the masses of vapour falling downwards for want of support.

Another very striking case is marked *b* in the figure, and was observed before a shower. Here a detached cumulus was observed to form first festoons, and then they in turn degenerated into raggy cloud, the whole disappearing very shortly, but was quickly followed by fresh rain-bearing clouds. The impression which the whole conveyed to me was that the festoons were formed by a sudden drop of the cloud, and that the "rag" was produced when the drop was less sudden. The appearance of the "rag" is not very well rendered in the diagram, but it is very difficult to delineate clouds by any engraving.

These are two typical cases of many which I have observed, and always with the same result—that the constant condition necessary for the formation of festoons was the sudden failure of an ascentional current of air. If so, the explanation of its prognostic value is very simple. Before many squalls or showers we are all familiar with the short, abortive gusts which so frequently precede them. Now we have only to assume that the ascentional uptake in front of the main body of the shower is as unsteady as the surface wind, and we have at once all the conditions of the formation of festoons. Almost all observers agree that they are usually formed at the edges of cloud masses. In the case of rain or thunder they ordinarily appear just before or after the rain; but in the case of a gale following some time afterwards, as observed by Mr. Clouston, the festoon must have been formed by some local squall or shower which bore some

a.1 *a*.2. *b*



relation to the disturbed weather which produced the gale. I once saw festoons in the west of Scotland during the hardest gale I have ever seen in this country. They were formed on the outskirts of a north-westerly squall.

Allied to festooned cumulus we may mention festooned stratus and festooned cirrus. The former is quite common in London during the summer, associated with showers or thunderstorms, while the latter is rare. In both the same idea seems to hold good as for cumulus, that they are formed by the sudden failure of the current, whatever it may be, that forms the stratus or cirrus.

It might appear, at first sight, that a uniform stratus could not fall in lumps; but however uniform it may seem, viewed from below, there is probably no such thing as a uniform stratum of cloud. Some portions are always denser, or composed of larger drops, and these, falling first, give the "pocky" appearance. In many simple cases, which I have been able to follow, there often seems to be a rough correspondence between bosses on the upper surface and festoons on the lower. In *a* 2 there is an unsuccessful attempt to depict such a case, which is drawn from nature.

The name of "festooned cloud" has been objected to as suggesting a lengthways arrangement of vapour, like the cloud called "rolled cumulus," with which it has probably nothing in common. Mr. Clement Ley has proposed the name of "tubercled cloud" as more applicable. Prof. Poey, who has also studied this cloud, has proposed the name of "globo-cumulus."

The general conclusion then, is that festoons are caused by a sudden failure of an ascentional current associated with showers or squalls, but whether they portend rain or wind depends on the circumstances under which they are observed.

21, Chapel Street, S.W., April 27 RALPH ABERCROMBY

The Sacred Tree of Kum-bum

PERHAPS the following statement may throw a little light on what was the tree seen by the Abbé Huc:—

On his voyage home from China the Abbé touched at Ceylon. This must have been in 1852 or 1853, as far as I can recollect. I was invited to meet him at breakfast, at the house of my kind

friend, Sir Charles Macarthy, then Colonial Secretary, my zoological and botanical tastes being well known to the latter.

The conversation turning on plants, the Abbé described a wonderful tree which he had seen, on the leaves of which were impressed thousands of likenesses of Buddha. Nothing was said about "Thibetan characters," nor did he lead us to suppose it grew larger than an ordinary cinnamon-tree (*not bush*), as it grows wild. His description was so detailed that, in spite of the florid language of a French traveller, I at once recognised a plant which grew not uncommonly in our gardens, the leaves of which were often placed in the finger-glasses after repasts, as on being crushed, they imparted a delicious fragrance to the hands. Looking up and catching the eye of our hostess, in which lurked an amused smile, I made the motions of dipping hands in a finger-glass. She instantly caught my meaning, whispered her instructions to the servant behind her chair, and each finger-glass —which useful adjunct to a meal was shortly after placed on the table—contained a leaf or two of what we used to call by a variety of names, such as the "profile laurel," or "figure laurel," or "face laurel."

The face of the Abbé was a picture to behold. "But here it is!" he exclaimed. "Where did this come from?" We then explained that it grew not a dozen yards from where he sat, to his great astonishment, and I fancied not a little chagrin, that his wonderful plant should be so well known and common.

The plant is, I believe a *laurel.* It has flashed across me that it *may be* a *citron*, but the plant is so well known in Ceylon, that if your contributor, Mr. W. T. Thiselton Dyer, wishes to ascertain its name, he has but to write a line to the Director of the Botanical Gardens, Peradenia, who will at once recognise it. The leaves are broad and pointed, shaped in fact somewhat like the cinnamon. Down each side of the midrib, extending along the veinlets (I write from memory, remember) are patches of pale greenish-yellow, much lighter than the ground-colour of the leaf. These take innumerable fantastic, face-like shapes— always profile—and with the aid of a pin, or point of a dessert-fork, we, in

"Those merry days,
The merry days, when we were young,"

used to put in an eye, and amuse ourselves in trying to find likenesses of our friends and acquaintances. It was a source of much fun among the young people.

The events of that morning were, from a variety of circumstances, deeply impressed on my memory, and I am positive that *then* nothing was said about "Thibetan characters" on the leaves or on the bark, nor of the *great size* of the tree, and the Abbé distinctly recognised the leaves as identical with those he had seen. You will perceive he calls it the "Tree of the Ten Tnou-and *Images*" (the italics are mine). This name would well apply to the "profile laurel," for no two faces are ever alike, but does not include *characters.*

Whether the size of the tree and the "Thibetan characters" grew (in the Abbé's brain?) after he left Ceylon, I do not know. The "real article" seems to have vanished. A bungling attempt to deceive by etching in lilac leaves could easily be detected, but "travellers see strange things"! E. L. LAYARD

Brit. Consulate, Noumea, New Caledonia, March 5

Sheet-lightning

THE correspondence on this subject (NATURE, vol. xxviii. pp. 4 and 54) can scarcely be said to contribute anything in support of the statement that sheet-lightning and the so-called summer or heat-lightning, are nothing else than the reflection of, or the illumination produced by, distant electrical discharges. The table given in the review (NATURE, vol. xxvii. p. 576) is not a record of instances of sheet-lightning, but only the number of hours, sorted according to the twenty-four hours of the day, in which sheet-lightning or heat-lightning was observed at Oxford during the twenty-four years ending 1876. In constructing the table, all those hours were excluded in which thunder was heard, and also the hour immediately preceding and following the hour of occurrence of thunder. Only those hours, therefore, were included during which any thunder that may have accompanied the lightning was at some distance from Oxford.

It follows simply as a matter of statistics that, if all cases of sheet lightning are nothing else but the illumination produced by distant electrical discharges, the curve of thunder and the curve of sheet-lightning and heat-lightning should be approximately parallel to each other after darkness has fairly set in. The Oxford observations show that such is not the case. To make this quite clear we give the results for August only :—

	Thunder.	Lightning.
8-9 p.m.	5	0
9-10 ,,	3	3
10-11 ,,	4	6
11-mid.	3	14
Mid.-1 a.m.	2	14
1-2 ,,	2	12
2-3 ,,	1	4
3-4 ,,	0	3

These two sets of figures from 8 p.m. to 4 a.m. furnish two curves quite distinct from each other; and the difference is not to be explained by the degree of facility for recording the observations afforded by each separate hour. It may be added that a similar result is obtained from electrical manifestations in other parts of the globe during the summer months. It is from these facts that it was concluded that no inconsiderable number of the cases of sheet-lightning and heat-lightning are not illuminations produced by distant electrical discharges, but, as suggested by Loomis, are rather to be considered as due to the escape of the electricity of the clouds in flashes so feeble that they produce no audible sound, and they occur when the air being very moist offers just sufficient resistance to the electricity to develop a feeble spark. THE REVIEWER

Solar Halo

THE following, taken from vol. i. *Philosophical Transactions*, p. 219, may interest your readers, as the phenomenon appears to coincide almost exactly with the one recorded in NATURE, vol. xxviii. p. 30. I omit the illustration, though it corresponds almost exactly with the one in NATURE, except that there were mock suns.

"An account of four suns, which very lately appear'd in France, published in the French *Journal des Sçavans* of May 10, 1666 :—

"'The 9th of April of this present year, about half an hour past nine, there appear'd three circles in the sky. *One* of them was very great, a little interrupted and white everywhere, without the mixture of any other colour. It passed through the midst of the sun's *disk*, and was parallel to the *horizon*. Its *diameter* was above a hundred degrees, and its *center* not far from the *zenith*.

"'The *second* was much less, and defective in some places, having the colours of a rainbow, especially in that part which was within the great circle. It had the true sun for its center.

"'The *third* was less than the first, but greater than the second, it was not entire, but only an arch or portion of a circle whose center was far distant from that of the sun, and whose circumference did by its middle join to that of the least circle, intersecting the greatest circle by its two extreams. In this circle were discerned also the colours of a rainbow, but they were not so strong as those of the *second*.

"'At the place where the circumference of this third circle did close with that of the *second*, there was a great brightness of rainbow colours mixt together. And at the two extremities where this *second* circle intersected the *first*, appear'd two parhelias or mock suns,' &c., &c."

In a note to this account it is stated that "Five suns appear'd the 29 March, A., 1629, at Rome between 2 or 3 of the clock in the afternoon." In the illustration given we find two circles similar to those given in NATURE. It seems that two of these suns "which were in the intersection of two circles, appear'd in that of a circle, which passed through the sun's diske, with another, that was concentrick to the sun."

The phenomenon of last week was minus the parhelia; can any reason be given for this? THOS. WARD

Northwich, May 15

IN reply to Mr. Mott's query (p. 30) I beg to say that I measured the halo with a sextant as carefully as possible, and made the semidiameter 25°. [Another halo occurring on the 13th measured 23° 20′.]

With regard to the mock moons, they were perfectly equidistant from the horizon all the time I observed them, and I regret that I did not notice that Mr. Mott had seen them otherwise. I read his letter rather hurriedly and thought the expression "out of place" referred to their position above the moon, and not to a want of parallelism with the horizon. SM.

Temple Observatory, Rugby, May 17

Mock Moons

I NEVER noticed that mock moons and mock suns are not always at the same altitude as the moon or sun, but I would point out that when objects are high up, it is very difficult to decide on their relative altitudes. If mock moons are at the same altitude as the moon, then of course they are not on a great circle, but on a small one, and in consequence, except when they are low down, a straight line passing through the mock moons will pass above the moon, and when they are high up, at a considerable distance above it. In such a case, if the observer does not look straight at the moon, he may easily suppose that one of the mock moons is higher up than the other. Is your correspondent (vol. xxvii. p. 606) *sure* that they were not at the same altitude on the occasion he refers to? If, instead of facing the moon and looking straight at it, he looked more at the right-hand mock moon, the illusion would be produced of the left-hand one appearing higher up. The same illusion is caused when the horizontal lines of buildings or of a window cut the line passing vertically through the moon obliquely; so that great care is required in making these observations.

I might add that I observed the mock moons and halos on the evening alluded to (April 16) from Sunderland till after 11 p.m., and that I noticed nothing unusual in their positions or size. The mock moons (or suns) are always outside the ordinary halo when their altitude is considerable. On that occasion there was also visible a considerable part of the horizontal halo passing through the mock moons, forming long tails to them away from the moon; also vertical and horizontal rays proceeding from the moon; forming a faint cross with the moon at its centre. The horizontal rays were narrow, and reached at one time to the ordinary halo, but were much fainter than the "tails" of the mock moons. The vertical rays did not reach quite so far, and were broad and indefinite; otherwise I suppose their character (except as to brightness) would be much the same as that of the "sun pillar" described by several of your recent correspondents.

T. W. BACKHOUSE

Sunderland, May 12

Helix pomatia

ALTHOUGH this species is decidedly local in this country, yet it is interesting to note that the counties in which it has been recorded are contiguous to one another. Its course of distribution appears to pass through Kent, Sussex, Surrey, Hants, Wilts, Gloucestershire, Berks, Oxon, Bucks, Herts, and Northamptonshire, and this seems to support Mr. Stokoe in his suggestion (NATURE, vol. xxviii. p. 6) that it may be a geologically recent importation from France (to the *northern* portion of which it is confined in that country).

In Murray's "Handbook to Surrey," p. 70, *Helix pomatia* is stated to abound at Tyting Farm near Guildford, "said to have been introduced from Italy" by an Earl of Arundel, and Bevan's "Guide to Surrey," p. 111, mentions the same locality as the "habitat of the edible snail imported from Italy," &c. I visited this spot in September, 1880, in quest of *H. pomatia*, and mentioned my object to a farm labourer, who speedily produced three specimens from under a log of wood, but told me that they were not at all plentiful there, as the soil was *sandy* and not *chalky*, and he said I must look for them on the neighbouring chalk downs, whence his master the farmer procured his for the purpose of adopting the diet, which, when ill, he had been advised to try. Be *H. pomatia* indigenous or not, there is no doubt its presence in England has been assisted by importations, for Mr. Lovell Reeve mentions its being introduced from Italy by an English nobleman in the vicinity of Box Hill and Reigate (cf. also Gray's "Turton," ed. 1840, p. 35).

The *Helix scalaris* referred to in Venables' work on the Isle of Wight is cited in that book as a monstrosity of *H. aspersa*, and Moquin Tandon's figure of the variety *scalaris* is of the usual coloration of that species. The name, however, was originally bestowed by Müller on a variety of *H pomatia* (Lamk. "An sans vert," second edition, vol. viii. p. 32), and is figured as such by Draparnaud, but Venables' reference seems to apply to a scalariform variety of *H. aspersa* observed by Dr. Gray near Ventnor.

W. C. ATKINSON

Streatham, S.W., May 11

Cape Bees

I CAN endorse all that Sir J. H. de Villiers says concerning the sense of smell in the wild bees of the Cape. The aversion they have to sweating horses is well known, as also to the scent of chopped carrots. The following instances of this have come under my own notice:—

A party of young men who had been springbok hunting all the morning, off-saddled their horses during the hottest part of the day, under the shadow of a great krantz (cliff); they had but just tied them to some trees, when the poor animals were attacked in the most vicious manner by an immense swarm of rock bees from the krantz, and so dreadfully were they stung, that, although the thongs that bound them were cut through as quickly as possible to enable the poor things to escape, one beautiful horse was stung to death, and two more of the number were so maddened that they galloped off, and for many days were quite unfit for use.

One of the Hottentot children upon our place, playing in the garden near some hived wild bees, mischievously chewed up a carrot, and spat it into the entrance of the hive; the boy was perfectly naked, and the next few minutes might have been his last, had not the European gardener happened to be near, and hearing his shrieks, hastened to the spot, thrust the child into a newly-dug trench, and quickly covered him with earth; but he had a narrow escape of his life, for he was literally covered with stings.

The precursor of a storm in the Karoo is generally a whirlwind of dust, and our boys used to take advantage of the dislike to storms evinced by bees, to throw up large handfuls of dust into the air, when a swarm was passing overhead, when sometimes the bees would be deceived and settle immediately.

M. CAREY-HOBSON

Late of Graaff Reinet, Cape of Good Hope

The Effect of the Change of Colour in the Flowers of "Pulmonaria officinalis" upon its Fertilisers

YESTERDAY I had an opportunity of convincing myself by direct observation that the change of colour in the flowers of *Pulmonaria officinalis* is of the same significance as in *Ribes aureum* and Lantana, according to Delpino and Fritz Müller (compare NATURE, vol. xvii. p. 79).

In a small locality about twenty yards long and two broad, where many hundred flowers of Pulmonaria were in all stages of development, its principal fertilisers were the females of *Anthophora pilipes*, F.; they visited almost exclusively the red flowers and those just beginning to change towards blue, but only exceptionally blue ones.

The first individual which I watched when it was flying from flower to flower did so without any exception. Another individual newly alighting on the place at first now and then visited one or some few blue flowers, but the longer it continued its predatory flight the more it neglected the blue flowers and selected only the red ones.

A third female of Anthophora which I followed indiscriminately visited (*a*) red flowers of Pulmonaria, (*b*) large blue flowers of Glechoma, both in the following order:—(*a*) 16, (*b*) 1, (*a*) 23, (*b*) 1, (*a*) 21, (*b*) 62, (*a*) 5 flowers; then it left the place without having touched a single blue flower of Pulmonaria.

The fourth and last female of Anthophora I followed neglected completely the flowers of Glechoma; but when it visited the red flowers of Pulmonaria and met for some time only with already emptied ones, it became more and more disturbed and hurried, and then indiscriminately visited blue and red flowers until anew it found honey in a red one. It visited (*a*) red and (*b*) blue flowers of Pulmonaria in the following order:—(*a*) 52, (*b*) 1, (*a*) 18, (*b*) 3, (*a*) 16, (*b*) 1, (*a*) 34, (*b*) 3, (*a*) 7, (*b*) 1, (*a*) 42, (*b*) 1, (*a*) 13; in summa (*a*) 182 red, (*b*) 10 blue flowers.

It is easy to be seen whether a flower of Pulmonaria when visited by Anthophora contains some honey or not; in the first case the proboscis of the bee rests at least 1 to 1½ seconds in the corolla tube, whereas in the other case it is instantly withdrawn. All blue flowers of Pulmonaria which were visited proved thus to be empty of honey, and in all which I examined with a lens in this locality the stigma was supplied with pollen.

We may, I think, safely conclude from these observations that the blue colour of older flowers of Pulmonaria, whilst increasing the conspicuousness of the clusters of flowers, at the same time indicates to such intelligent bees as Anthophora to which flowers they have to restrict their visits as well to their own as to the plant's profit.

HERMANN MÜLLER

Lippstadt, May 8

The Soaring of Birds

In the discussion about the soaring of birds which has lately been carried on in NATURE, I do not remember to have observed that any one quoted from Mr. Darwin's account of the condor. He says ("A Naturalist's Voyage Round the World," chap. ix. p. 186):—"When the condors are wheeling in a flock round and round any spot their flight is beautiful. Except when rising from the ground, I do not recollect ever having seen one of these birds flap its wings. Near Lima, I watched several for nearly half an hour, without once taking off my eyes: they moved in large curves, sweeping in circles, descending and ascending without giving a single flap. . . . The head and neck were moved frequently, and apparently with force; and the extended wings seemed to form the fulcrum on which the movements of the neck, body, and tail acted. If the bird wished to descend, the wings were for a moment collapsed; and when again expanded with an altered inclination, the momentum gained by the rapid descent seemed to urge the bird upwards with the even and steady movement of a paper kite. In the case of any bird *soaring*, its motion must be sufficiently rapid, so that the action of the inclined surface of its body on the atmosphere may counterbalance its gravity."

Cambridge, May 17 JAMES CURRIE

Intelligence in a Dog

Some time since a friend of mine, Mr. J. W. Schaub, a mechanical engineer at the Edgmore Ironworks of Wilmington, Del., informed me of an exceedingly interesting case of intelligence in a black and tan terrier belonging to him. The old mother dog and her playful family entered his bedroom while he was dressing, and one of the pups snatched his stocking as he was in the act of putting it on, running out of the room with it. The mother at once followed the young offender, took the stocking from him, and returned it to the master. Mr. Schaub said that her conduct gave evidence of displeasure at the action of the pup, and she impressed him with the idea that she felt in some way responsible for the conduct of her young. Being greatly interested in the matter, Mr. Schaub contrived to have the offence committed on many successive mornings, the same performance being repeated each time.

St. Louis, U.S., April 24 FRANCIS E. NIPHER

Mid-height of Sea Waves

Can any of your readers furnish me with the formula, or other means, for finding the difference between the mid-height of a sea-wave and the sea-level? W. PARFITT

A CURIOUS SURVIVAL

The thirteenth Annual Report of the Deputy Master of the Mint, just issued, contains some interesting information showing how persistently an ancient system of computing the value of bullion has survived in this country. The facts are fully set forth in an appendix to the Report by Prof. Chandler Roberts, who has recently and successfully advocated the adoption of the decimal system in the bullion transactions of the Mint. In order to make the matter clear, it may be well to state that the Troy pound, still used in this country for weighing the precious metals, is believed to have been derived from the Roman weight of 5759·2 grains, the 125th part of the large Alexandrian talent; this weight, like the Troy pound, having been divided by the Romans into 12 ounces. The earliest statute of this kingdom in which the Troy weight is named is the 2 Henry V. st. 2, c. 4, but the Troy weight is universally allowed to have been in general use from the time of King Edward I. The most ancient system of weights in this kingdom was the Moneyer's pound or the money pound of the Anglo-Saxons, which was continued in use for some centuries after the Conquest, being then known as the "Tower pound," or sometimes the Goldsmith's pound. It contained 12 ounces of 450 grains each, or 5400 grains, and this weight of silver was a pound sterling. The Tower pound was abolished in 1527 by a statute of Henry VIII., which first established Troy weight as the only legal weight for gold and silver, and from this time to the present our system of coinage has been based on the Troy weight, the Troy pound containing 5760 grains.[1]

The bullion transactions of the Mint have hitherto been based on an Assayer's weight termed the "carat pound," the final division of which corresponds with the number of grains in the Troy pound, and side by side with this system a curious method of expressing the 'standard' or composition of ingots or coins of gold and silver has been retained until the present year. For instance, the ordinary conception of the composition of a sovereign would be that it is an alloy or mixture of the two metals gold and copper in definite proportions, and the most simple way of expressing its contents would be to describe them as consisting of 91·66 per cent. of gold and 8·34 per cent. of copper. An assayer or bullion dealer, on the other hand, using the old system, would simply consider the composition of the coin to be gold of 'standard fineness,' that is to say, containing two carats of alloying metal in the pound; and in dealing with any particular alloy of gold and copper would in no way regard its per-centage composition, but would consider it as being so much "better" or "worse" than the one definite and legal standard, according as it contained more or less of the precious metal. The French 20-franc piece, which contains 90 per cent. of gold, would thus be described as "worse 0 carats $1\frac{5}{8}$ carat grains," and an Austrian ducat, which contains 98·61 per cent. of gold, as "better 1 carat $2\frac{5}{8}$ carat grains." The cumbersome nature of this system is evident; it has the disadvantage of being unintelligible to those who employ the decimal system, and who are therefore in the habit of mentally referring to pure gold as 1000 It is even found wanting in clearness by many who are conversant with the ordinary operations of coinage and bullion transactions generally. For instance, the meaning of "worse 0 $1\frac{5}{8} + 1$" as the assay report of an ingot is at least obscure, while the equivalent statement that the standard fineness of the ingot is 900 at once suggests that 1000 parts of the metal contain 900 parts of gold.

The ancient system of reporting the results of assays possesses however many points of interest, and Prof. Roberts adds a few details respecting it, taken from a work by Snelling,[2] an authority on the computation of the value of bullion, who, writing in 1766, observes that "by the word SILVER we understand not only the metal so-called, pure and unmixed, but also when in a mass with copper; and if but one-half, two-thirds, or any other proportional part of it be silver, yet the whole bears that name. The same is to be understood of GOLD, when by itself, or in a mass with silver and copper together, or with either of them alone."

"This is the reason that inquiries are not made, what quantity of fine gold or fine silver is contained in any mixture, which seems to be the most natural inquiry, but how much standard it holds." Thus it is that "the Assay Master, in reporting the result of an assay, does not give the absolute fineness or the quantity of fine silver or fine gold present, but only the relative quantity or fineness, that is, how much the mixture is more or less than standard. In the case of gold of 20 carats fine (or 20 parts of pure gold in 24 parts of the alloy) the assayer puts down **Wo.** $\overset{\text{car.}}{\text{ij.}}$, and if it is 23 carats $3\frac{1}{2}$ grains fine, he puts down **Br.** $\overset{\text{car.}}{\text{j.}}$ $\overset{\text{gr.}}{\text{iij.}}$ ob." The last sign represents an obolus or half of a carat grain, but in modern times the final division has been 1/60th of a carat grain.

It may be pointed out in defence of this complicated system, that, as Snelling proceeds to remark, "the quan-

[1] "On the Abolition of the Troy Pound," the third Report of the Commissioners appointed to inquire into the condition of the Exchequer Standards. Parliamentary paper [c. 30], 1870.

[2] "Doctrine of Gold and Silver Computations," by Thomas Snelling. (London, 1766.)

tity of "betterness" or "worseness" in an ingot being added to or subtracted from the weight of it, gives the quantity of standard metal contained in it," and that therefore the "betterness" or "worseness" affords a ready means of determining the amount of copper or gold required to standardise the whole. Further, if a number of ingots of varying weights and fineness have to be dealt with, a similar result will be arrived at by taking the algebraical sum of the several products of their weights and "betterness" or "worseness." These advantages, however, apply to individual calculations, and become unimportant when standarding tables adapted to the decimal system are available.[1]

In a letter to Mr. Fremantle, Prof. Roberts advocated the abolition of this old system of carats and grains and the adoption of the decimal system. This has accordingly been carried into effect. Gold of the value of two millions sterling has recently been imported for coinage, and the simplicity and accuracy of the new system has been abundantly demonstrated.

The facts above stated may seem comparatively unimportant in themselves, but the Mint may at any time be called upon to coin (as was the case in 1872) fifteen millions sterling of gold in a single year, and extreme care has to be taken to insure accuracy in the standard fineness of the metal. It is curious that the old system described above should not have given place before now to that which has long been adopted in other countries.

THE POISONOUS LIZARD[2]

THE Gila Lizard of Arizona and Sonora has anterior, deciduous, grooved teeth, which communicate by ducts with large glands within the angle of th ower jaw —an apparatus so strongly resembling the poison-fangs of serpents as to suggest that this lizard has venomous properties. It is said by the natives of Mexico to be very poisonous, but others again have declared that it is perfectly harmless. One specimen sent to Sir John Lubbock killed a frog in a few minutes and a guinea-pig in three minutes.

The conflicting statements are probably due to the fact that the teeth are very small and easily removed. Some specimens of the creature reach the length of three feet. As experiments made by allowing the lizard to bite animals are untrustworthy on account of the uncertainty of getting the poison equally introduced into the tissues at every bite, Doctors Weir Mitchell and Reichert collected the saliva so as to be able to inject it in known quantities. The saliva was obtained by making the animal bite on a saucer-edge. It dropped in small quantities from the lower jaw, and had a faint and not unpleasant aromatic odour. It was distinctly alkaline, in contrast to serpent venoms, which are all alike acid. Four and a half minims of it diluted with half a cubic centimetre of water and injected into the breast of a large pigeon caused the bird to walk unsteadily after three minutes. At the same time the respiration became rapid and short, and at the fifth minute feeble. At the sixth minute the bird fell in convulsions with dilated pupils, and was dead before the end of the seventh minute. There was not the least trace of any local effect of the poison, as there would have been in the case of crotalus venom. The muscles and nerves were perfectly sensitive to stimulation mechanically or by weak induced currents. The heart was arrested in complete diastole, and was full of firm, black clots. The intestines looked congested. In another experiment it was found that the poison gradually lowered the arterial tension and rendered the pulse irregular. Its action on the pulse is not due to any effect upon the pneumogastric nerves, as it is just the same when these nerves are cut. When applied to the heart of a frog it arrests its pulsations in diastole, and the organ afterwards contracts slowly—possibly in rapid *rigor mortis*. The cardiac muscle loses its irritability to stimuli at the time it ceases to beat. The other muscles and nerves respond readily to irritants, but the spinal cord has its power annihilated abruptly and refuses to respond to the most powerful electrical currents.

The authors conclude that "this interesting and virulent heart poison contrasts strongly with the venoms of serpents, since they give rise to local hemorrhages, and cause death chiefly through failure of the respiration and not by the heart, unless given in overwhelming doses. They lower muscle and nerve reactions, especially those of the respiratory apparatus, but do not as a rule cause extreme and abrupt loss of spinal power. Finally, they give rise to a wide range of secondary pathological appearances which are absent from Heloderma poisoning."

This distinction between the action of the poison of Heloderma and serpent venom is correct as far as regards the poison of the rattlesnake and perhaps also the Crotalidæ generally, but the distinction is by no means marked between the poison of Heloderma and the venom of the cobra. This venom was found by Sir Joseph Fayrer and Dr. Lauder Brunton to have but a slight local action as contrasted with that of the rattlesnake or of the daboia, and to produce no local hemorrhage. The effect of cobra poison on birds also is very much the same as that of the Heloderma; and in the experiments given in this preliminary paper, the effect of the Heloderma poison on the heart of the frog is very much like that of cobra poison, the failure of action with subsequent and gradually increasing contraction being almost precisely the same.[1]

In Brunton and Fayrer's experiments on cobra poison, the fall of blood-pressure was less marked, but it still occurred. Paralysis of the spinal cord also is produced by cobra poison, and the experiments in this preliminary paper are too few to enable us to decide whether the paralysing effect is greater from the poison of Heloderma than from cobra venom. We shall look with much interest to the further study of the venom of this curious animal, which the authors intend to make on the arrival of the fresh specimens which they are about to receive.

ON THE CONDENSATION OF VAPOUR FROM THE FUMAROLES OF THE SOLFATARA OF POZZUOLI

THE fumaroles of the Solfatara of Pozzuoli, and especially the larger fumarole known as the *Bocca della Solfatara*, give a striking illustration of the action of smoke in causing the condensation of aqueous vapour in the manner demonstrated by the experiments of Coulier, and more especially by those of Dr. Aitken.

Persons who have visited the Solfatara will remember that one of the feats by which the *ciceroni* of the place try to excite the wonderment of visitors is to light some paper or a few dry branches, and put the flaming body before or inside the mouth of the principal fumarole, augmenting thus very greatly the volumes of cloudy vapour escaping from the fissure. This phenomenon can be observed in all volcanic fumaroles. A flame is not indispensable, the condensation of the vapour being also produced by the mere smouldering of tinder.

Prof. Piria first tried to explain the phenomenon. He thought that small quantities of sulphuretted hydrogen issued from the soil together with the aqueous vapour:

[1] Tables on the system above described were first published in the year 1651, having been prepared by Mr. Reynolds, Assay Master at the Mint in the Tower A second edition was afterwards issued with corrections and additions in 1677.

[2] "A Partial Study of the Poison of *Heloderma suspectum* (Cope), the Gila Monster." By Dr. S. Weir Mitchell and Dr. E. T. Reichert of Philadelphia.

[1] Brunton and Fayrer on the Poison of Indian Venomous Snakes (*Roy. Soc. Proceedings*, January 22, 1874, p. 126).

on mixing with air and coming in contact with a flame, or an incandescent body, the hydrogen sulphide would be oxidised, and resolved in sulphur and water (with the production of small quantities of sulphur dioxide); the sulphur, minutely divided, would remain long suspended in air, and cause the condensation to cloudy consistency of the aqueous vapour. Piria illustrated his explanation by a simple experiment: if in a vessel containing a mixture of sulphuretted hydrogen and air a lighted taper is introduced, a dense mist is rapidly formed; a similar mist is produced when glowing charcoal, or highly heated lava, or pumice, or glass, or red-hot iron is introduced in the gaseous mixture. When there is a large proportion of H_2S, the oxidation is very rapid, and the mixture explodes and burns.

Piria's explanation cannot be applied to the *Bocca della Solfatara*, where the presence of H_2S cannot be detected either by the sense of smell, or by the lead-acetate test-papers. In the "Memorie Geologiche sulla Campania (*Rendiconti della Reale Accademia delle Scienze di Napoli*, 1849, p. 137) Prof. A. Scacchi, after having opposed Piria's opinion, gives the following explanation: "I believe the increase of the vapoury cloud due to the carbonic acid produced in the combustion of the tinder, its affinity for water causing the precipitation of the invisible vapour, and thus producing a mist." According to Prof. Scacchi, in the presence of large quantities of aqueous vapour, and at the temperature of the fumarole, carbonic dioxide would act as hydrochloric acid gas which fumes in ordinary air.

Since 1849 no one (as far as I have gathered) has suggested any new opinion or tried some experiment to explain the phenomenon in question. I thought it would be interesting to test experimentally at the Solfatara the opinion of Prof. Scacchi. I was inclined to believe that, if at the ordinary temperature carbonic dioxide does not condense aqueous vapour from the air, there was very little probability that the condensation would be caused at temperatures as high as those of the vapours issuing from the Bocca of the Solfatara (about 90° Centigrade externally); the action of flames or smouldering bodies in augmenting the vapoury cloud appeared to me as chiefly due to the condensation around the minute particles of soot or dust produced during the combustion.

The following experiments were done during a clear day, when abundant vapours were issuing from the large fumarole:—

1. A Wolff bottle (1 litre capacity), from which a constant current of carbon dioxide was obtained (by pouring dilute hydrochloric acid on marble fragments), was placed on the ground inside the fumarole. The cloud of vapour augmented.

2. By means of a caoutchouc tube the CO_2 from the generator was conducted near the hottest invisible vapour. This vapour became interspersed with cloudlets of condensed vapour, and the cloudy pillar outside the Bocca greatly augmented.

3. A large bottle (of about 15 litres capacity) filled with carbon dioxide was brought inside the cavity, and the CO_2 poured out. The effect was most striking outside by the voluminous, but not immediate, outbursts of cloudy vapour.

4 With bellows of the kind used for sulphuring vines, I blew sulphur dust inside the cavity. This caused the production of great volumes of visible vapour. The same effects were produced every time that minutely divided bodies (wheaten flour, oxide of magnesia, chalky dust, &c.) were blown, or thrown, inside the cavity or near the invisible vapour.

5. The effect was very striking when the action of the carbon dioxide (from the Wolff bottle) was combined with the action of the sulphur dust.

6. A small alcohol flame augmented the cloudiness of the vapour.

7. The smoky flame of burning naphthalene acted much more powerfully than the alcohol flame.

From these experiments, which (with the exception of 3 and 6) were often repeated, the following conclusions may be drawn:—

1. Carbon dioxide helps to condense watery vapour.

2. Minute bodies suspended in air are a powerful cause (the principal cause, as Coulier and Aitken have shown) in the condensation of aqueous vapour.

3. The action of flames, or of incandescent bodies, in augmenting so remarkably the volumes of visible vapour rising from the fumaroles of the Solfatara must be ascribed both to the carbon dioxide and to the minute carbonaceous particles set free during the combustion.

Of these conclusions the first requires to be confirmed by careful laboratory experiments. ITALO GIGLIOLI

Laboratory of Agricultural Chemistry,
R. Agric. College, Portici

STATE OF THE ATMOSPHERE WHICH PRODUCES THE FORMS OF MIRAGE OBSERVED BY VINCE AND BY SCORESBY

In 1881, when I wrote the article *Light* for the *Encyc. Britt.*, I had not been able to meet with any detailed calculations as to the probable state of the atmosphere when multiple images are seen of objects situated near the horizon I had consulted many papers containing what are called "general" explanations of the phenomena, but had found no proof that the requisite conditions could exist in nature:—except perhaps in the case of the ordinary mirage of the desert, where it is obvious that very considerable temperature differences may occur in the air within a few feet of the ground. But this form of mirage is essentially unsteady, for it involves an unstable state of equilibrium of the air. In many of Scoresby's observations, especially that of the solitary inverted image of his father's ship (then thirty miles distant, and of course far below the horizon), the *details* of the image could be clearly seen with a telescope, showing that the air must have been in equilibrium. The problem seemed to be one well fitted for treatment as a simple example of the application of Hamilton's *General Method in Optics*, and as such I discussed it. The details of my investigation were communicated in the end of that year to the Royal Society of Edinburgh, and will, I hope, soon be published. The paper itself is too technical for the general reader, so that I shall here attempt to give a sketch of its contents in a more popular form. But a curious little historical statement must be premised.

It was not until my calculations were finished that I found a chance reference to a great paper by Wollaston (*Phil. Trans.* 1800). I had till then known only of Wollaston's well-known experiment with layers of different liquids in a small vessel. But these, I saw, could not reproduce the proper mirage phenomena, as the rays necessarily enter and emerge from the transition strata by their *ends* and not by their lower *sides*. This experiment is by no means one of the best things in Wollaston's paper, so far at least as the immediate object of the paper is concerned. That so much has been written on the subject of mirage during the present century, with only a casual reference or two to this paper, is most surprising. It may perhaps be accounted for by the fact that Wollaston does not appear to have had sufficient confidence in his own results to refrain from attempting, towards the end of his paper, a totally different (and untenable) hypothesis, based on the effects of aqueous vapour. Be the cause what it may, there can be no doubt that the following words of Gilbert were amply justified when they were written, early in the present century:—"In der That ist Wollaston der Erste und Einzige, der die *Spieglung aufwärts* mit Glück zu erklären unternommen hat." For his methods are, in principle, perfectly correct and suffi-

ciently comprehensive; while some of his experiments imitate closely the state of the air requisite for the production of Vince's phenomena. Had Wollaston only felt the necessary confidence in his own theory, he could hardly have failed to recognise that what he produced by the extreme rates of change of temperature in the small air-space close to a red-hot bar of metal, could be produced by natural rates of change in some ten or twenty miles of the atmosphere:—and he would have deserved the credit of having completely solved the problem.

Six months after my paper was read, another happy chance led me to seek for a voluminous paper by Biot, of which I had seen no mention whatever in any of the books I had previously consulted. The probable reason for the oblivion into which this treatise seems to have fallen is a curious one. It forms a considerable part of the volume for 1809 of the *Mém de l'Institut.* But in the three first great libraries which I consulted, I found this volume to be devoid of plates. In all respects but this, each of the sets of this valuable series appeared to be complete. Without the figures, which amount to no less than sixty-three, it is practically impossible to understand the details of Biot's paper. The paper was, however, issued as a separate volume, "*Récherches sur les Réfractions extraordinaires qui ont lieu près de l'horizon*" (Paris, 1810), which contains the plates, and which I obtained at last from the Cambridge University Library. I have since been able to procure a copy for the Edinburgh University Library. Biot's work is an almost exhaustive one, and I found in it a great number of the results which follow almost intuitively from my methods:—such as the possible occurrence of *four* images, under the conditions usually assumed for the explanation of the ordinary mirage; the effects of (unusual) refraction on the apparent form of the setting sun; &c. But it seems to me that Biot's long-continued observations of the phenomena as produced over extensive surfaces of level sand at Dunkirk have led him to take a somewhat onesided view of the general question. And, in particular, I think that his attempted explanation of Vince's observations (so far as I am able to understand it; for it is very long, and in parts extremely obscure and difficult, besides containing some singular physical errors) is not satisfactory. His general treatment of the whole question is based to a great extent upon the properties of caustics, though he mentions (as the *courbe des minima*) the "locus of vertices" which I had employed in my investigations, and which I think greatly preferable. There can be no doubt, however, that Biot's paper comes at least next in point of importance to that of Wollaston:—though in his opinion Wollaston's work was complete only on the physical side of the problem. "Sous le rapport de la physique son travail ne laisse rien à désirer."

But, if the chief theoretical papers on the subject have thus strangely been allowed to drop out of notice, the case is quite different with several of those which deal with the observed phenomena. Scoresby's *Greenland*, his *Arctic Regions*, and his *Voyage to the Northern Whale Fishery*, are still standard works; and in them, as well as in vols. ix. and xi. of the *Trans. R.S.E.*, he has given numerous careful drawings of these most singular appearances. The explanatory text is also peculiarly full and clear, giving all that a careful observer could have been expected to record. It is otherwise with the descriptions and illustrations in Vince's paper (*Phil. Trans.* 1799). In fact the latter are obviously not meant as *drawings* of what was seen; but as *diagrams* which exhibit merely the general features, such as the relative position and magnitude of the images:—the details being filled in at the option of the engraver. That such was the view taken by Brewster, is obvious from the illustrations in his *Optics* (*Library of Useful Knowledge*), for while one of Scoresby's drawings is there *copied*, one of Vince's is treated in a highly imaginative style by the reproducer.

Scoresby's sketches are composite, as he takes care to tell the reader, so that in the reproduction below (Fig. 1) I have simply selected a few of the more remarkable portions which bear on the questions to be discussed. It is to be

Fig. 1.

remarked that the angular dimensions of these phenomena are always of *telescopic* magnitude:—the utmost elevation of an image rarely exceeding a quarter or a third of a degree.

Because the rays concerned are all so nearly horizontal, and (on the whole) *concave* towards the earth; and because they must also have on the whole considerably greater curvature than the corresponding part of the earth's surface, especially if they happen to have points of contrary flexure; it is clear that, for a preliminary investigation, we may treat the problem as if the earth were a plane. This simplifies matters very considerably, so that definite numerical results are easily obtained; and there is no difficulty in afterwards introducing the (comparatively slight) corrections due to the earth's curvature. But these will not be farther alluded to here.

Of course I began, as almost every other person who has thought of the production of the ordinary mirage of the desert must naturally have begun, by considering the well-known problem of the paths of projectiles discharged from the same gun, with the same speed but at different elevations of the piece. This corresponds, in the optical problem, to the motion of light in a medium the square of whose refractive index is proportional to the distance from a given horizontal plane. Instead, however, of thinking chiefly of the different elevations corresponding to a given range, I sought for a simple criterion which should enable me to decide (in the optical application) whether the image formed would, in any particular case, be a direct or an inverted one. And this, I saw at once, could be obtained, along with the number and positions of the images, by a study of the form of the locus on which lie the *vertices* of all the rays issuing from a given point. Thus, in the ballistic problem, the locus of the vertices of all the paths from a given point, with different elevations but in the same vertical plane, is an ellipse.

Its minor axis is vertical, the lower end being at the gun; and the major axis (which is twice as long) is in the plane of projection. Now, while the inclination of the piece to the horizon is less than 45°, the vertex of the path is in the *lower* half of this ellipse, where the tangent leans forward from the gun; and in this case a small increase of elevation *lengthens* the range, so that the two paths do not intersect again above the horizon. In the optical problem this corresponds to an *erect* image. But, when the elevation of the piece is greater than 45°, the vertex of the path lies in the *upper* half of the ellipse, where the tangent leans back over the gun; and a small increase of elevation *shortens* the range. Two contiguous paths, therefore, intersect one another again above the horizon. And, in the optical problem, this corresponds to an *inverted* image. In symbols, if the eye be taken as origin and the axis of x horizontal, there will be a direct image for a ray at whose vertex dy/dx and x (in the curve of vertices) have the *same* sign, an inverted image when the signs are different.

Hence, whatever be the law of refractive index of the air, provided only it be the same at the same distance from the earth's surface, (*i.e.* the surfaces of equal density parallel planes, and therefore the rays each symmetrical about a vertical axis) all we have to do, in order to find the various possible images of an object at the same level as the eye, is to *draw the curve of vertices for all rays passing through the eye, in the vertical plane containing the eye and the object, and find its intersections with the vertical line midway between the eye and the object.* As soon as this simple idea occurred to me, I saw that it was the very kernel of the matter, and that all the rest would be mere detail of calculation from particular hypotheses. Each of the intersections in question is the vertex of a ray by which the object can be seen, and the corresponding image will be erect or inverted, according as the curve of vertices leans from or towards the eye at the intersection. Thus, in Fig. 2, let E be the eye, and

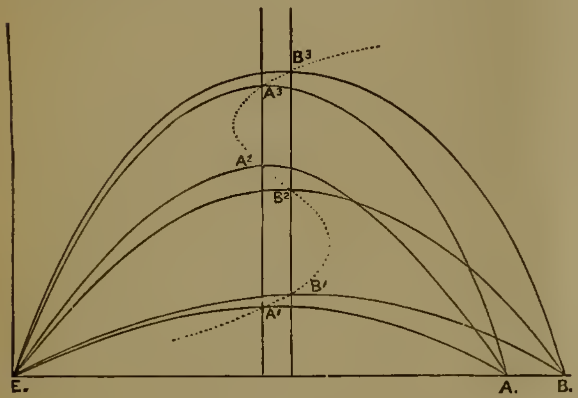


Fig. 2.

the dotted line the curve of vertices for all rays in the plane of the paper, and passing through E. Let A be an object at the level of the eye, $A^1 A^2 A^3$ the vertical line midway between E and A. Then A^1, A^2, A^3 are the vertices of the various rays by which A can be seen. If we make the same construction for a point B, near to A, we find that whereas the contiguous rays through A^1, B^1 and through A^3, B^3 do not intersect, those through A^2, B^2 do intersect. At A^1 and A^3 the curve of vertices leans *from* the eye, and we have erect images; at A^2 it leans back *towards* the eye, and we have an inverted image. And thus, if this curve be continuous, the images will be alternately erect and inverted. The sketch above is essentially the same as one given by Vince, only that he does not employ the curve of vertices. If the object and eye be not at the same level, the construction is not *quite* so simple. We must now draw a curve of vertices for rays passing through the eye, and another for rays passing through the object. Their intersections give all the possible vertices. (This construction of course gives the same result as the former, when object and eye are at the same level.) But the images are now by no means necessarily alternately erect and inverted, even though the curve of vertices be continuous. However, I merely note this extension of the rule, as we shall not require it in what follows.

I then investigated the form of the curve of vertices in a medium in which the square of the refractive index increases by a quantity proportional to the square of the distance from a plane in which it is a minimum, and found that (under special circumstances, not however possible in air) three images could be produced in such a medium. But the study of this case (which I could not easily explain here without the aid of mathematics) led me on as follows.

As the curvature of a ray is given by the ratio of the rate of change of index per unit of length perpendicular to the ray, to the index itself (a result which I find was at least virtually enunciated by Wollaston); and as all the rays producing the phenomena in question are very nearly horizontal:—*i.e.* perpendicular to the direction in which the refractive index changes most rapidly:—their curvatures are all practically the same at the same level. Hence if the rate of diminution of the refractive index, per foot of ascent, were nearly constant, through the part of the atmosphere in which the rays travel, the rays we need consider would all be approximately arcs of equal circles; and the curve of vertices would (so far as these rays are concerned) lean wholly *from* the eye; being, in fact, the inferior part of another equal circle which has its lowest point at the eye. Hence but one image, an erect one, would be formed; but it would be seen elevated above the true direction of the object. This is practically the ordinary horizontal refraction, so far as *terrestrial* objects on the horizon are concerned. The paths of the various rays would be of the form in Fig. 3 (the drawing

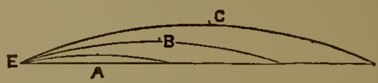


Fig. 3.

is, of course, immensely exaggerated) and the locus of vertices, A B C, obviously leans *from* the eye. But now suppose that, below a stratum of this kind, there were one of constant density, in which of course the rays would be straight lines. Then our sketch takes the form Fig. 4 (again exaggerated); each of the portions of the ray in

Fig. 4.

the upper medium being congruent to the corresponding one in the former figure (when the two figures are drawn to the same scale), but *pushed farther to the right as its extremities are less inclined to the horizon.* In its new form the curve of vertices A B C leans back *towards* the eye, and we have an inverted image. The lower medium need not be uniform as, for simplicity, we assumed above. All that is required is that the rate of diminution of density upwards shall be less in it than in the upper medium.

Those who have followed me so far will at once see that, as a more rapid decrease of density, commencing at a certain elevation, makes the curve of vertices lean back, so a less rapid decrease (tending to a "stationary state") at a still higher elevation will make the curve of vertices again lean forward from the eye. I need not enlarge upon this.

Thus to repeat:—the conditions requisite for the production of Vince's phenomenon, at least in the way conjectured by him, are, a stratum in which the refractive index diminishes upwards to a nearly stationary state, and below it a stratum in which the upward diminution is either less or vanishes altogether. The former condition secures the upper erect image, the latter the inverted image and the lower direct image.

In my paper read to the *Royal Society of Edinburgh* I have given the mathematical details following from the above statement; and have made full calculations for the effect of a transition stratum, such as must occur between two uniform strata of air of which the upper has the higher temperature. From Scoresby's remarks it appears almost certain that something like this was the state of affairs when the majority (at least) of his observations were made. When two masses of the same fluid, at different temperatures, rest in contact; or when two fluids of different refractive index, as brine and pure

water, diffuse into one another; the intervening layer must have a practically "stationary" refractive index at each of its bounding surfaces, and a stratum of greatest rate of change of index about midway between them. The exact law of change in the stratum is a matter of comparatively little consequence. I have assumed (after several trials) a simple harmonic law for the change of the square of the refractive index within the stratum. This satisfies all the above conditions, and thus cannot in any case be very far from the truth. But its special merit, and for my purpose this was invaluable, is that it leads to results which involve expressions easily calculated numerically by means of Legendre's Tables of Elliptic Integrals. This numerical work can be done once for all, and then we can introduce at leisure the most probable hypotheses as to the thickness of the transition stratum, the height of its lower surface above the ground, and the whole change of temperature in passing through it. I need not now give the details for more than one case, and I shall therefore select that of a transition stratum 50 feet thick, and commencing 50 feet above the ground. From the physical properties of air, and the observed fact that the utmost angular elevation of the observed images is not much more than a quarter of a degree, we find that the upper uniform layer of air must under the conditions assigned be about 7° C. warmer than the lower. Hence by the assumed law in the stratum, the maximum rise of temperature per foot of ascent (about the middle of the transition stratum) must be about 0°·2 C. per foot. Such changes have actually been observed by Glaisher in his balloon ascents, so that thus far the hypothesis is justified. But we have an independent means of testing it. The form of the curve of vertices is now somewhat like the full lines in the following cut, Fig. 5:—

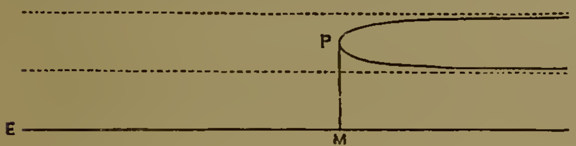


Fig. 5.

where E is the eye, and the dotted lines represent the boundaries of the transition stratum. It is clear that, if P M be the vertical tangent, there can be but one image of an object unless its distance from E is at least twice E M. This will therefore be called the "*Critical distance.*" If the distance be greater than this there are three images:—one erect, seen directly through the lower uniform stratum—then an inverted one, due to the diminution of refractive index above the lower boundary of the transition stratum—and finally an erect image, due to the approximation to a stationary state towards the upper boundary of that stratum. Now calculation from our assumed data gives E M about six miles, so that the nearest objects affected should be about twelve miles off. Scoresby says that the usual distance was from ten to fifteen miles. Thus the hypothesis passes, with credit, this independent and severe test. A slight reduction of the assumed thickness of the transition-stratum, or of its height above the ground, would make the agreement exact.

All the phenomena described in Vince's paper of 1799, as well as a great many of those figured in Scoresby's works, can easily be explained by the above assumptions. Scoresby's remarkable observation of a single inverted image of his father's ship (when thirty miles off, and of course far below the horizon) requires merely a more rapid diminution of density at a definite height above the sea. His figure is the second in Fig. 1 above. But Scoresby figures, as above shown, several cases in which two or more inverted images, without corresponding erect ones, were seen above the ordinary direct image. The natural explanation is, of course, a series of horizontal layers of upward diminishing density and without a "stationary state" towards their upper bounding planes. I find that, by roughly stirring (for a very short time) a trough in which weak brine below is diffusing into pure water above, we can reproduce this phenomenon with great ease. In fact, when temporary equilibrium sets in, the fluids are arranged in a number of successive parallel strata with somewhat abrupt changes of density.

But the mathematical investigation, already spoken of, shows that it is quite possible that there may be layers tending to a stationary state without any corresponding *visible* images.

This depends on the fact that, while the inverted image (due to the lower part of a stratum) is *always* taller than the object seen directly (though not much taller unless the object is about the critical distance); the numerical calculation shows that the erect image is in general extremely small, and can come into notice only when the object is not far beyond the critical distance. Thus there may have been, in *all* of Scoresby's observations (though he has only occasionally noticed and depicted them) an erect image above each inverted one, but so much reduced in vertical height as to have been invisible in his telescope, or at least to have formed a mere horizontal line so narrow that it did not attract his attention. The greatly superior number of inverted images, compared with that of the direct ones, figured by Scoresby may thus be looked upon as another independent confirmation of the approximate correctness of the hypothetical arrangement we have been considering.

To obtain an experimental repetition of the phenomena in the manner indicated by the above hypothesis, a tank, with parallel glass ends, and about 4 feet long, was half-filled with weak brine (carefully filtered). Pure water was then cautiously introduced above it, till the tank was nearly filled. After a few hours the whole had settled down into a state of slow and steady diffusion, and Vince's phenomenon was beautifully shown. The object was a metal plate with a small hole in it, and a lamp with a porcelain globe was placed behind it. The hole was triangular, with one side horizontal (to allow of distinction between direct and inverted images), and was placed near one end of the tank, a little below the surface-level of the unaltered brine, the eye being in a corresponding position at the other end. A little vertical adjustment of object and eye was required from time to time as the diffusion progressed. The theoretical results that the upper erect image is usually much less than the object, and that it is seen by slowly convergent rays, while the inverted image is larger than the object and is seen by diverging rays, were easily verified.

To contrast Wollaston's best-known experiment with this, a narrow tank with parallel sides was half-filled with very strong brine, and then cautiously filled up with pure water. (The strong brine was employed to make up, as far as possible, for the shortened path of the rays in the transition stratum.) Phenomena somewhat resembling the former were now seen, when object and eye were nearly at the same distance apart as before, and the tank about half-way between them. But in this case the disparity of size between the images was not so marked—the upper erect image was always seen by diverging rays, the inverted image by rays diverging or converging according as the eye was withdrawn from, or made to approach, the tank. In this case, the curvature of each of the rays in the vessel is practically constant, but is greatest for the rays which pass most nearly through the stratum of most rapid change of refractive index. Hence, when a parallel beam of light fell horizontally on the tank and was received on a sufficiently distant screen, the lower boundary of the illuminated space was blue—and the progress of the diffusion could be watched with great precision by the gradual displacement of this blue band

I propose to employ this arrangement for the measurement of the rate of diffusion, but for particulars I must refer to my forthcoming paper.

Wollaston's experiment with the red-hot poker was probably, his experiment with the long red-hot bar of iron almost certainly, similar to that above described with the long tank and the weak brine; and *not* to that with the short tank, though the latter is usually cited as Wollaston's contribution to the explanation of the Vince phenomenon. We have seen how essentially different they are, and that the latter does not correspond to the conditions presented in nature. P. G. TAIT

NOTES

THE Council of the Scottish Meteorological Society are soliciting subscriptions, however small, for the proposed Ben Nevis Observatory. It is essential to the success of this important national undertaking that the buildings should be erected during the present summer, and several thousand pounds are required before operations can be commenced. A considerable sum has already been received in liberal subscriptions from a few individuals, but not nearly enough for the purpose. We trust that many of our readers will send what they can to the Scottish Meteorological Society, Edinburgh.

DR. WILD, president of the International Circumpolar observation parties, announces that in conformity with the request of several Governments, the observations now going on round the Pole will not be prolonged beyond the time originally fixed, viz. September, and that all the parties, if not prevented by ice, will be back within that month.

A LETTER read at the Paris Geographical Society states that P. Vidal, French missionary to Samoa, has discovered the remains of La Perouse and his unfortunate companions.

THE Rev. S. J. Perry, S.J., has lately been elected a Corresponding Member of the Accademia dei Lincei.

DR. HENRY SCHLIEMANN has been elected an Honorary Fellow of Queen's College, Oxford.

LAST week we announced that a baronetcy had deservedly been conferred on Dr. William Chambers, and this week we regret to announce the death of the veteran publisher in his eighty-fourth year. As the head of the firm of Messrs. W. and R. Chambers, he has through a long life done splendid service in the spread of education, and of a knowledge of science. In his "Information for the People," his "Tracts," his text-books of science, among the first of their kind, and by various other means, he did good pioneer work in scientific literature and education.

IN reference to our note last week (p. 63), a correspondent writes that the American table at Naples is being used by its first occupant, Dr. E. B. Wilson, of the Johns Hopkins University, Baltimore. Dr. Wilson has been working during a part of the year at Cambridge on early mammalian embryology, and at Naples his work will probably be either on certain points in the development of some of the Cœlenterata or upon the embryology of the Dicegemidæ as available material permits. Williams College, Mass., which holds the American table, receives a brief course of lectures from each worker whom it appoints to the privileges of the Naples Station.

ON the evening of Friday last week several tornadoes swept over the states of Minnesota, Wisconsin, Illinois, and Missouri, which were exceptionally destructive to life and property even for that tornado-troubled region. It is reported that 83 persons have been killed and 340 injured, many of them fatally, and a very large number of houses reduced to ruins. Of these tornadoes the most terrible in its destructiveness would appear to have been the one which passed over Racine in the south-east of Wisconsin, killing 25 and injuring 100 persons, and wrecking 150 buildings. The path of the tornado was about 400 yards wide and half a mile long, and all buildings, particularly those in the central line of its path, collapsed into mere masses of ruins. Waggons and other movable articles were blown into Lake Michigan, over which the tornado passed on leaving the town, the whirling columns of clouds and the violent commotions of the lake presenting a grand and impressive spectacle. The recently published "Professional Papers of the Signal Service, No. VII." show that the region over which these tornadoes passed is comprehended within that portion of the United States where tornadoes are of most frequent occurrence.

MR. BRUNLEES, the President of the Institution of Civil Engineers, has sent out invitations for a *conversazione* at the South Kensington Museum on Wednesday, the 30th inst.

ON Saturday last, May 19, the Essex Field Club held its first meeting of the session. The party, nearly ninety in number, alighted at Theydon Bois Station on the Ongar branch of the Great Eastern Railway, and proceeded through Epping Forest to Ambresbury Banks, where they were met by Sir T. Fowell Buxton. The party was then conducted through the splendid park belonging to the Copt Hall Estate, and finally assembled at Warlies, Waltham Abbey, the seat of Sir Fowell Buxton, who had kindly invited the Club for the occasion. In the course of the evening a paper on "English Plant Names" was read by Mr. J. Britten, F.L.S.

THE Paris Aëronautical Exhibition will be opened at the Trocadéro on June 5 and close on the 18th. MM. Janssen, Berthelot, Paul Bert, and Hervi Mangon are among the members of the committee, as well as a number of senators and deputies. The festival will take place at Annonay on July 29, and statues of the two brothers Montgolfier will be erected on the public place of the city. A competition has been opened in Paris, and the works of competitors are on view at the Cercle de la Librairie. The jurymen, mostly members of the Academy of Beaux Arts, will give their award on Saturday next. The height of the monument and pedestal will be 7 metres. The prize is 3000 francs for the plaster model to be exhibited at Annonay on July 29, and 40,000 francs for the bronze. The marble for the pedestal will be given by Government. A public banquet will be given in Paris, M. Gaston Tissandier being in the chair.

ON April 29, at 10.30 p.m., a brilliant meteor was observed in Joudalen in Norway. It appeared in the east, and went in a southerly direction, where it passed out of sight. Its size to the eye was about the same as the moon's, while its shape appeared to be conic. The colour of its track was deep red, and it shone so brilliantly that the smallest objects could be seen on the ground. It lasted several seconds, and disappeared behind some mountains.

ON the 13th, at 8 o'clock in the evening, a large meteor was observed at Epinal, travelling from south-east to north-west; it had a disk which has been estimated at a decimeter. The tail was of a pinky colour; a noise from explosion was heard. It was also observed at Mulhausen.

THE Reports on the Public Gardens and Plantations in Jamaica are becoming yearly of more importance. That for the year ending September 30 last is now before us. Mr. Morris opens his report by bearing testimony to the liberality of the Steamship and Railway Companies in conveying plants free of charge to the different ports and railway stations. "By these means," it is stated, "districts, formerly beyond the reach of the Public Gardens, have been able to obtain plants as conveniently and as

cheaply as if they were in the neighbourhood of Kingston." It is stated that a drought, caused chiefly by the failure of the May rains coming after a succession of dry months with parching winds, had a prejudicial effect on all agricultural operations. Mr. Morris says: "It is a subject of common remark amongst old planters that the 'seasons,' or the periodical rains which have hitherto fallen with great regularity and copiousness during the months of May and October of each year, are becoming more and more uncertain and irregular, and the effects of these conditions are clearly shown in the precariousness of the agricultural products affected by them. These remarks apply chiefly to the southern slopes of the Blue Mountains, and to such other districts stretching south and west where coffee and provisions are being chiefly raised." Under the head of "Cinchona Plantation," the cultivation of which plants has become an object of special attention in Jamaica, Mr. Morris reports very fully. He says: "In order to test the commercial value of Jamaica grown bark, no better plan could be followed than to send it in lots to the open market and place it in competition with barks from other countries. That it has so satisfactorily stood this test and brought in a large return on the outlay, and, moreover, that the results of the sales have induced cinchona planting to be undertaken in the island by private enterprise with energy and success, are matters for which the Government no less than the general public are to be congratulated." "During the past year chief attention has been given to the successful introduction of *Cinchona Ledgeriana* and its establishment as a cultivated plant in Jamaica. In addition to the plants established on the Government plantations, several thousands have been distributed amongst private planters, and each lot of these will doubtless form a nucleus from whence seeds and cuttings may hereafter be obtained, and thus prove most valuable acquisitions to private plantations." A few plants of the now well known cuprea bark, *Remijia pedunculata*, have been raised from seed received from Bogota, and are being tried in order to test the value of the bark under cultivation. An attempt is about to be made to manufacture cinchona febrifuge in the island in a similar way to what is being now done so successfully in the East Indies. By this means a valuable and cheap preparation will be available for use among the poorer classes. Besides the cinchonas the cultivation of jalap and various other economic plants has received attention during the year, so that we have evidence that a good deal of really useful work is being carried on by Mr. Morris in Jamaica.

A WRITER in the *North China Herald* on the history of gunpowder in China asserts that this explosive was known in the seventh century of our era. The alchemists of the Han dynasty, and subsequently in the fourth and following centuries, worked with saltpetre and sulphur, as well as cinnabar, red oxide of lead, and other common compounds. But in the seventh century we find gunpowder used to make a crackling sound and to afford an agreeable sight to the court of Sui Yang-ti, the emperor of that time. The earliest exhibitions of fireworks mentioned in Chinese history belong to that date. The substances used in the composition of gunpowder are all native to China, and the writer appears to prove conclusively that the Arabs derived the art of firework making, as well as gunpowder, from the Chinese. The discovery once made, the Chinese alchemists, owing to the badness of their hypotheses and the futility of their aims, were slow at improvement. But the doctors of the Arab colonies in China carried to Bagdad the germs of the Chinese discoveries, and there they were elaborated into new forms. In short, in many arts and sciences the Arabs learnt from China, and, assisted by Nestorians, Jews, and Greeks, improved on what they learned. In course of years, cannon, matchlocks, and shells for use in sieges were brought to China from Mohammedan countries. There are faint traces in the eleventh century of rude firearms: in the twelfth and thirteenth centuries the records of their use in the Chinese wars become frequent and distinct. The Golden Tartars, in their wars with South China in the twelfth century, used cannon which they called "heaven-shaking thunder." In an iron tube was placed powder which was "set fire to, and would burn down half a square *li* of houses and pierce a coat of mail made of iron rings." It is expressly stated that Genghis Khan, the Mongol conqueror, used cannon in his wars. Kublai Khan also used these weapons at a siege celebrated in Chinese history—that of Siang-yang. Hearing, it is said, the sound of the explosion, which shook the sky, and seeing that the balls entered seven feet into the earth, the Chinese defenders of the city capitulated. It is clear that China owed its knowledge of artillery to the Mohammedans. In the fourteenth century commenced the European intercourse with China, which then abandoned the Arabs, and took the Portuguese as teachers in the construction of weapons of warfare.

NEWS from Iceland states that from the 12th to the 21st of March there were violent volcanic water eruptions.

A REUTER'S telegram from Hong Kong *viâ* San Francisco announces the completion of the telegraph line between Canton and that colony. This is the second great line in China, and appears to have been constructed wholly by native merchants in Canton, who found the want of early communication with western markets in their commercial transactions. Vigorous preparations are also being made for the most formidable undertaking of this nature that has yet been attempted in China, viz. a line connecting Peking with Canton. According to the latest information an expeditionary party has arrived at Shanghai to conduct the necessary surveys. It will proceed first to Soochow, and there, under the escort of 200 troops, will commence its work, proceeding southward.

A NUMBER of students at the Ecole des Mines of France will during the summer make an excursion to the Arctic regions. A steamer, in charge of a Norwegian Arctic hunter, will bring the party to Throndhjem and Hammerfest, and thence to Spitzbergen, which will be examined during a fortnight's stay. The Naturalistic Museum of Paris sends two *savants* with the party.

THE additions to the Zoological Society's Gardens during the past week include two Green Monkeys (*Cercopithecus callitrichus* ♂ ♀) from West Africa, presented by Mr. Thos. H. Dixon; a Long-eared Owl (*Asio otus*), British, presented by the Rev. H. D. Grantham; a Smooth Snake (*Coronella lævis*), European, presented by Mr. W. H. B. Pain; seven Black and Yellow Cyclodus (*Cyclodus nigro-luteus*) from Tasmania, presented by Baron Ferdinand von Mueller, C.M.Z.S.; a Proteus (*Proteus anguinus*), European, presented by Miss Maud Howard; a Sea Crayfish (*Palinurus vulgaris*), British Seas, presented by Messrs. Milestone and Staniforth; three Green-winged Doves (*Chalcophaps indica*) from India, a Herring Gull (*Larus argentatus*), British, deposited; a King Vulture (*Gypagus papa*) from Tropical America, purchased; a Cabot's Tragopan (*Ceriornis caboti* ♀) from North-West China, received on approval.

OUR ASTRONOMICAL COLUMN

THE COMET OF 1707.—The elements of this comet's orbit, as calculated by Lacaille and Struyck, bear a certain degree of resemblance to those of the comet discovered by De Vico at Rome on February 20, 1846 (1846 IV. of our catalogues), to which Van Deinse's definitive calculation assigns a period of revolution of 73 years. The interval between the perihelion passages in 1707 and 1846 would give two periods of 69·1 years; there is consequently a sufficient reason for examining how far the elements of the comet of 1707 represent the observations. It appears to have been discovered by Manfredi at Bologna on November 25, and the place given in the *Mémoires* of the Paris

Academy for that date was in R.A. 308° 25′, Decl. – 24° 17′. Pingré in his *Cométographie* mentions that according to Struyck this position is erroneous, and that ten minutes should be added to the declination and five to the right ascension as printed in the *Mémoires*, adding that if Lacaille has used the Bologna observation his orbit would be less accurate than that of Struyck. The Bologna observers Manfredi and Stancari found the comet on November 25, in the same field of view of an 8 foot telescope, with two stars, the distance between which they estimated at 9′. At 7h. 14m. 47s. apparent time the centre of the comet was in the right line joining these stars, and its distance from the northernmost star was one-third of the distance between them. It is easy to see from the rough position given, that the stars in question are Piazzi XX., 296 and 298, and carrying back his places, we have for the position of the comet referred to the mean equinox of 1708·0, R.A. 307° 49′·3, Decl. – 23° 44′·1. The equation of time was 12m. 37s. subtractive from apparent time, and hence the Greenwich mean time of observation was November 25·26163. The place calculated from Lacaille's orbit, first published in his "Leçons d'Astronomie," differs + 7′·2 in R.A. and + 5′·6 in Decl., so that it is evident he did not use the position as erroneously deduced in the *Mémoires*. The agreement of his elements with the Paris observation on December 17 is fairly good; there is a much larger deviation from the approximate places determined at Bologna, on January 13 and 17; but these observations of Manfredi and Stancari are probably affected with very material errors, as such is certainly the case with the deduced position for the night of discovery. So far as can be judged from this partial comparison of Lacaille's elements with observation, the hypothesis of identity of the comet of 1707 with that of 1846 is not supported, but the observations of the former may deserve further discussion.

THE TRANSIT OF VENUS.—Prof. C. A. Young has published his observations of all four contacts in the late transit of Venus, made at the Halsted Observatory, Princeton, N.J., with the 23-inch equatorial, and a power of 160. At the two internal contacts the aperture was diminished to 5⅗ inches, "in order to make the observations comparable as far as possible with those of the various government expeditions," but at the external contacts the full aperture was employed; a polarising helioscope was attached. We have compared the times given by Prof. Young with those calculated from the reduction-equations published in this column, in the formation of which it was the main object to get geometrical contacts. It has been previously mentioned that there was a close agreement between prediction and observation in the case of the results obtained at Harvard College, and the following are the small differences (calc.–obs.) for Prof. Young's:—

I. – 16s. ... II. + 3s. ... III. – 15s. ... IV. – 4s.

THE BRITISH ASSOCIATION CATALOGUE OF STARS.—We lately remarked, not without some surprise, that a copy of this work was priced in a continental list of second-hand books at the high figure of 12*l*. 10*s*., or about three times the cost at its publication in 1845. Such a fact naturally induces the query, Is there occasion for a new general catalogue of the principal fixed stars, or, say, of stars within the limit of naked-eye vision? It is a question upon which there will probably be a wide difference of opinion, and it is one that it would be of interest to discuss.

GEOGRAPHICAL NOTES

THE last number of the *Verhandlungen der Gesellschaft für Erdkunde* of Berlin contains a paper by Prof. Brauns, late of Japan, on the Island of Yezo. The writer agrees with Mr. Keane and other ethnologists that the Ainos are a totally different race from the Japanese. The number of these people in Yezo and the Kuriles is given by the Japanese Government as 18,000, but many authors place the number as high as 50,000. In Saghalin there are 10,000 to 12,000, and if those in the southern part of Kamschatka who are living under Russian rule are included, the total number of the race would probably be from 60,000 to 70,000. In the same issue the indefatigable explorer of the Philippines, Herr Jagor, describes briefly a recent journey through Luzon. An interesting communication also is a list of the papers published by the Geographical Society of Tokio in its volume for 1880. This Society is composed, we believe, almost wholly of natives, and its papers are printed in Japanese. There appear to have been in all thirty-eight communications of one kind or another; the writers or translators (for some of the papers are apparently translations from others in European languages) are in all cases Japanese. Among the papers are several on the history and geography of Okinawa, as the Japanese call the Loochoo group; the climate of Peking; Japanese intercourse with foreign countries in the middle ages; a journey to Vladivostock; the history of geography in Japan; history and geography of Persia by a Japanese who had travelled through the country; description of Australia; description of a voyage in the Persian Gulf; of a journey on the Khirgiz steppes; ancient Japanese geographical names; description of Saghalin; on the absence of precious stones in Japan, &c., &c. Some of these papers would hardly meet with a favourable reception from the Council of the Royal Geographical Society; but in Japan they are listened to and read afterwards in their printed form by hundreds of people who have never left their own country, and who possess but a very small geographical literature. When this is remembered, the list will appear not only a creditable one to the travellers, but also a most useful one for the spread of geographical knowledge in Japan, which after all is the purpose of the Society.

THE annual report of Mr. Tremlett, the British Consul at Saigon, contains some interesting geographical information about the northern and less known districts of the Indo-Chinese peninsula. The governor of Cochin China sent out an expedition to explore the country between the Meikong and Annam at about 14° latitude. The party left Peamchileng, on the Meikong, proceeding eastward. After passing the river valley the country became hilly and wooded, intersected with numerous watercourses. No difficulty was experienced until the arrival of the travellers near the Cambodian frontiers. As they proceeded the hostility of the people became yet more pronounced, and finally their passage towards Annam was closed altogether. They were finally compelled to retreat, losing all their baggage on the way, and after three months' absence they reached a friendly post. The Mois inhabit the wilds between Cambodia, Siam, Burmah, and China. Commerce, properly speaking, does not exist among them, and traffic is carried on by exchanges. The various roads and river are stopped up by the people themselves to prevent the passage of pillagers and enemies; as a result the people are very backward. Money is almost unknown or unappreciated among them. A native who will not work for a dollar a day will do so for a bell costing a few cents. The articles most valued by the Mois are buffaloes, red and white cottonades, glass ware, brass wire, utensils, salt, and salt fish.

FROM the same report we learn that an exploration of the upper waters of the Saigon river by Lieut. Gauthier shows that previous charts are incorrect; the names given in them being imaginary. There appears to have existed in this region in former times one or more states in an advanced stage of civilisation, as may be seen by ruins still remaining, probably offshoots of the famous Angker Wat. The present race of Mois claim no descent from their predecessors on the soil, and indeed it would be difficult to find a lower state than theirs. It is difficult to communicate with them, as the language is not easily picked up by the Annamites. They appear to be in a state of independence, paying no tribute to any of their neighbours, although the King of Cambodia is their nominal suzerain. The report concludes by saying that the French Government will have to send a much more serious expedition if anything is to be learned about these regions; two or three men can learn nothing.

THE great attention which France has given for many years past to the Indo-Chinese peninsula is shown by a return printed in the *Proceedings of the Société Académique Indo-Chinoise* of all the scientific expeditions despatched by the French Government to this region. These embrace archæological, ethnological, geographical, and other scientific objects, and up to 1881 they were seventy seven in number. They commence as far back as 1680, when the Jesuit Pallu visited the courts of Tonkin and Annam. Seventeen of these took place before the military occupation of any part of Cochin China by the French; thirty-three were sent by the Ministry of Public Instruction, chiefly for archæological purposes, while the remaining twenty-seven were sent by the Ministry of Marine and the Colonies, and were devoted principally to exploration. To understand the mass of scientific work done by the French in Indo-China, it must be remembered that these seventy-seven expeditions do not include the innumerable journeys and researches of the missionaries,

which commence as far back as the end of the fifteenth century, the various expeditions sent out by private enterprise, those despatched for military, naval, or diplomatic purposes, or, finally, the various hydrographic or geodetic surveys undertaken by the French authorities in Cochin China.

THE teachers at the school for the sons of Japanese nobles in Tokio appear to have hit upon a notable method of teaching physical geography. In the court behind the school building is a physical map of the country, between three and four hundred feet long. It is made of turf and rock, and is bordered with pebbles, which look at a little distance much like water. Every inlet, river, and mountain is reproduced in this model with a fidelity to detail which is wonderful. Latitude and longitude are indicated by telegraph wires, and tablets show the position of the cities. Ingenious devices are employed in illustrating botanical studies also. For example, the pine is illustrated by a picture showing the cone, leaf, and dissected flower, set in a frame which shows the bark and longitudinal and transverse sections of the wood.

IN No. 103 of the *Zeitschaft* of the Berlin Geographical Society will be found a fine series of new large scale maps by H. Kiepert on the region containing the ruins of Babylon, embodying the results of new surveys and explorations. In the same number Herr Karl Schneider has a long paper on the valley formations of the Eifel.

PROF. FRIES has written an interesting paper proposing that part of Greenland should be colonised by Lapps. He maintains that the country would be a paradise to the mountain Lapps, that it is no more inhospitable than their own country, that there would be no restrictions to their wanderings, and that in the interior in summer and on the coast in winter they would find abundant forage for their herds. Prof. Fries is of Nordenskjöld's opinion, that in the interior abundant reindeer pasture will be found. Moreover, as a Lapp can always follow where a reindeer leads, this would be an excellent plan of discovering the true nature of the interior; it seems certainly worth trying.

TWO gentlemen from Münster (Westphalia)—Dr. Bachmann and Dr. Friedrich Wilms—are about to start on a scientific tour to Southern Africa, the Transvaal to begin with, in order to make zoological and botanical researches. Their journey will extend over several years, and the travellers will endeavour to establish direct commercial relations between the South African colonies and Germany.

ELECTRICAL UNITS OF MEASUREMENT[1]

THE lecturer began by observing that no real advance could be made in any branch of physical science until practical methods for a numerical reckoning of phenomena were established. The "scale of hardness" for stones and metals used by mineralogists and engineers was alluded to as a mere test in order of merit in respect to a little understood quality, regarding which no scientific principle constituting a foundation for definite measurement had been discovered. Indeed it must be confessed that the science of strength of materials, so all important in engineering, is but little advanced, and the part of it relating to the quality known as hardness least of all.

In the last century Cavendish and Coulomb made the first advances towards a system of measurement in electrical science, and rapid progress towards a complete foundation of the system was effected by Ampère, Poisson, Green, Gauss, and others. As late as ten years ago, however, regular and systematic measurement in electrical science was almost unknown in the chief physical laboratories of the world; although as early as 1858 a practical beginning of systematic electric measurement had been introduced in the testing of submarine telegraph cables.

A few years have sufficed to change all this, and at this time electric measurements are of daily occurrence, not in our scientific laboratories only, but also in our workshops and factories where is carried on the manufacture of electric and telegraphic apparatus. Thus ohms, volts, amperes, coulombs, and microfarads are now common terms, and measurements in these units are commonly practised to within one per cent. of accuracy. It seems, indeed, as if the commercial requirements of the application of electricity to lighting and other uses of everyday life were destined to influence the higher region of scientific investigation with a second impulse not less important than that given thirty years ago by the requirements of submarine telegraphy.

[1] Abstract of lecture on "Electrical Units of Measurement," by Sir William Thomson, F.R.SS.L. and E., M.Inst.C.E., delivered on Thursday evening, May 3, 1883, at the Institution of Civil Engineers.

A first step toward the numerical reckoning of properties of matter is the discovery of a continuously varying action of some kind, and the means of observing and measuring it in terms of some arbitrary unit or scale division; while the second step is necessarily that of fixing on something absolutely definite as the unit of reckoning.

A short historical sketch was given of the development of scientific measurement, as applied to electricity and magnetism, from its beginning with Cavendish about 100 years ago, to the adoption of the absolute system of measurement by this country in 1869, at the instance of the British Association Committee on Electric Standards. The importance in this development of the originating works of Gauss and Weber was pointed out, as also of the eight years' labours of the British Association Committee. This Committee not only fairly launched the absolute system for general use, but also effected arrangements for the supply of standards for resistance coils, in terms of a unit, to be as nearly as possible 10^9 centimetres per second. This unit afterwards received the name of the ohm, which was adopted from a highly suggestive paper which had been communicated to the British Association in 1861 by Mr. Latimer Clark and Sir Charles Bright, in which some very valuable scientific methods and principles of electric measurement were given, and a system of nomenclature—ohmas, kilohmas, farads, kilofarads, volts, and kilovolts—now universally adopted with only unessential modification, was proposed for a complete system of interdependent electric units of measurement. At the International Conference for the Determination of Electrical Units held at Paris in 1882, the absolute system was accepted by France, Germany, and the other European countries; and Clark and Bright's nomenclature was adopted in principle and extended.

Gauss's principle of absolute measurement for magnetism and electricity is merely an extension of the astronomer's method of reckoning mass in terms of what may be called the universal gravitation unit of matter, and the reckoning of force, according to which the unit of force is that force which, acting on unit of mass for unit of time, generates a velocity equal to the unit of velocity. The universal-gravitation unit of mass is such a quantity of matter, that if two quantities, each equal to it, be placed at unit distance apart, the force between them is unity.

Here mass is defined in terms of force and space, and in the preceding definition force was defined in terms of mass, space, and time. Eliminating mass between the two, it will be found that any given force is numerically equal to the fourth power of the velocity with which any mass whatever must revolve round an equal mass, fixed at such a distance from it as to attract it with a force equal to the given force. And, eliminating force between the two primitive definitions of the universal gravitation system, it will be found that any given mass is numerically equal to the square of the velocity with which a free particle must move to revolve round it in a circle of any radius, multiplied by this radius. Thus, take a centimetre as the unit of length, and a mean solar second as the unit of time, and adopt 5·67 grammes per cubic centimetre as the mean density of the earth from Baily's repetition of Cavendish's experiment, and suppose the length of the seconds' pendulum to be 100 centimetres, and neglect the oblateness of the earth and the centrifugal force of its rotation (being at the equator only 1/289 of gravity), the result for the universal gravitation units of mass and force is respectively 15·36 French tons, and 15·36 megadynes, or 15·07 times the terrestrial surface-weight of a kilogram.

The ultimate principles of scientific measurement were illustrated by the ideal case of a traveller through the universe who has brought with him on his tour no weights, no measures, no watch or chronometer, nor any standard vibrator or spring balance, but merely Everett's units and constants, and a complete memory and understanding of its contents, and who desires to make for himself a metrical system agreeing with that which he left behind him on the earth. To recover his centimetre the readiest and most accurate way is to find how many wave-lengths of sodium light there are in the distance from bar to bar of a grating which he can engrave for himself on a piece of glass. How easily this is done, supposing the grating once made, was illustrated by a rapid experiment performed in the course of the lecture, without other apparatus than a little piece of glass with 250 fine parallel lines engraved on it by a diamond, and two candles and a measuring tape of unknown divisions of

length (only used to measure the *ratio* of the distance between the candles to the distance of the grating from either). The experiment showed the distance from centre to centre of consecutive bars of the grating to be 32 times the wave-length of yellow light. This being remembered to be $5\cdot89 \times 10^{-5}$ of a centimetre, it was concluded that the breadth of the space on which the 250 lines are engraved is 250. 32. 5·892. 10^{-5}, or ·4714 of a centimetre! According to the instrument-maker it is really ·5 of a centimetre! Five minutes spent on the experiment instead of one, and sodium flames behind fine slits, instead of open candles blowing about in the air might easily have given the result within one-half per cent. instead of $4\frac{1}{2}$ per cent. Thus the cosmic traveller can easily recover his centimetre and metre measure. To recover his unit of time is less easy. One way is to go through Foucault's experimental determination of the velocity of light.

But he must be imagined as electrically-minded; and he will certainly, therefore, think of "v," the number of electrostatic units in the electro-magnetic unit of electricity; but he will, probably, see his way better to doing what he wants by making for himself a Siemens' mercury unit (which he can do easily, now that he has his centimetre), and finding (by the British Association method, or Lorenz's with Lord Rayleigh's modification, or both), the velocity which measures its resistance in absolute measure. This velocity, as is known from Lord Rayleigh and Mrs. Sidgwick, is 9413 kilometres per mean solar second, and thus he finds, in mean solar seconds, the period of the vibrator, or arbitrary-unit chronometer, which he used in his experiments.

Still, even though this method might be chosen as the readiest and most accurate, according to present knowledge, of the fundamental data for recovering the mean solar second, the method by "v" is too interesting and too instructive in respect to elimination of properties of matter from our ultimate metrical foundations to be unconsidered. One very simple way of experimentally determining "v" is derivable from an important suggestion of Clark and Bright's paper, referred to above. Take a Leyden jar, or other condenser of moderate capacity (for example, in electrostatic measure, about 1000 centimetres), which must be accurately measured. Arrange a mechanism to charge it to an accurately measured potential of moderate amount (for example, in electrostatic measure, about 10 c.g.s., which is about 3000 volts), and discharge it through a galvanometer coil at frequent regular intervals (for example, ten times per second). This will give an intermittent current of known average strength (in the example, 10^5 electrostatic c.g.s., or about 1/300,000 c.g.s. electromagnetic, or 1/30,000 of an ampere), which is to be measured in electromagnetic measure by an ordinary galvanometer. The number found by dividing the electrostatic reckoning of the current, by the experimentally found electromagnetic reckoning of the same, is "v," in centimetres per the arbitrary unit of time, which the experimenter in search of the mean solar second has used in his electrostatic and electromagnetic details. The unit of mass which he has chosen, also arbitrarily, disappears from the resulting ratio It is to be hoped that before long "v" will be known within 1/10 per cent. At present it is only known that it does not *probably* differ 3 per cent. from $2\cdot9 \times 10^{10}$ centimetres per mean solar second. When it is known with satisfactory accuracy, an experimenter, provided with a centimetre measure, may, anywhere in the universe, rate his experimental chronometer to mean solar seconds by the mere electrostatic and electromagnetic operations described above, without any reference to the sun or other natural chronometer.

The remainder of the lecture was occupied with an explanation of the application of the absolute system in all branches of electric measurement, and the definition of the now well known practical units founded on it, called ohms, volts, farads, microfarads, amperes, coulombs, watts. The name mho, found by saying ohm to a phonograph and then turning the drum backwards, was suggested for a unit of conductivity, the reciprocal of resistance. The subdivision, millimho, will be exceedingly convenient for the designation of incandescent lamps.

The British Association unit has been found by Lord Rayleigh and Mrs. Sidgwick to be ·9868 of the true ohm (10^9 centimetres per second), which differs by only 1/50 per cent. from ·9870, the number derived from Joule's electrothermal measurements described in the British Association Committee's Report of 1867, with 772 Manchester foot-pounds taken as the dynamical equivalent of the thermal unit from the measurement described in his Royal Society paper of 1849, and confirmed by his fresh measurement of 20 years later, published in his last Royal Society paper on the subject.

It is satisfactory that, whether for interpreting old results, or for making resistance-coils anew, electricians may now safely use the British Association unit as ·9868, or the Siemens unit as ·9413, of the ohm defined as 10^9 centimetres per second.

U.S. NATIONAL ACADEMY OF SCIENCES[1]

THE annual meeting of this body was held in Washington during the last week, with an attendance of forty members. Scientific sessions were held on Tuesday, Wednesday, and Friday, in the large lecture-room of the National Museum, and business sessions on every day of the meeting.

Twenty-four foreign associates were elected as follows:—Astronomers: Prof. Otto von Struve, of the Imperial Observatory at Pulkowa, Russia; Prof. J. C. Adams, of Cambridge, Eng.; Prof. A. Auwers, Director of the Observatory at Berlin; and Prof. Theo. von Oppolzer, Director of the Observatory at Vienna. Mathematicians: Prof. Arthur Cayley, of the University of Cambridge, Eng.; Prof. J. J. Sylvester, of the Johns Hopkins University, Baltimore; and Prof. E. Bertrand, of Paris. Physicists: Prof. R. Clausius, of the University of Bonn; Baron H. von Helmholtz, Professor in the University of Berlin; Prof. Robert Kirchhoff, of the University of Berlin; Prof. G. G. Stokes, of the University of Cambridge, Eng.; and Sir William Thomson, Professor in the University of Glasgow. Chemists: Prof. J. B. Dumas, Secretary of the Academy of Sciences, Paris; and Professors M. Berthelot, Boussingault, Chevreul, and Würtz, all of Paris. Geologist: Freiherr von Richthofen, Professor in the University of Bonn, and President of the German Geographical Society. Botanists: Sir J. D. Hooker, Director of the Botanical Gardens at Kew, Eng.; Prof. A. de Candolle, of Geneva. Biologists: L. Pasteur, of Paris; Prof. T. H. Huxley, of London; Prof. R. von Virchow, of the University of Berlin; A. von Kölliker, Professor of Anatomy in the University of Würzburg. Prof. Struve, one of the newly elected foreign associates, who is on a visit to this country, was a regular attendant at the scientific sessions of the Academy, and read a paper.

In consequence of the death of Prof. W. B. Rogers, the President, it became necessary to elect his successor. On the first ballot, Prof. Wolcott Gibbs, of Cambridge, one of the founders of the Academy, was elected. He, however, firmly declined the honour, from a feeling, as he said, that he could not give the time necessary to the work. The Academy reluctantly acquiesced in the decision of Prof. Gibbs, and proceeded to a second ballot, when Prof. O. C. Marsh, of New Haven, the acting President, was elected by a handsome majority. The newly-elected President will hold office for six years.

The first act of the new President was to announce that he had received from Mrs. Mary A. Draper, widow of Prof. Henry Draper, the sum of six thousand dollars, accompanied by a deed of trust which fully specified the objects she had in view. He called upon Prof. Barker to explain the nature of the trust to the Academy. Prof. Barker first made some appropriate remarks, recalling Prof. Draper's interest in the Academy, and then read the deed, the substance of which is as follows:—The income of the trust is to be used "for the purpose of striking a gold medal which shall be called the 'Henry Draper Medal,' shall be of the value of two hundred dollars," and shall be awarded from time to time, but not oftener than once in two years, as a premium to any person in the United States or elsewhere who shall make an original investigation in astronomical physics, the results of which shall be deemed by the Academy of sufficient importance and benefit to science to merit such recognition. If at any time the income of the fund shall exceed the amount necessary for the striking of the medal, the surplus may be used in aid of investigations and work in astronomical physics to be made and carried on by a citizen of the United States.

The President appointed Messrs. G. F. Barker, W. Gibbs, S. Newcomb, A. W. Wright, and C. A. Young as a committee to have charge of the fund, to make rules to govern the award of the medal, and to suggest to the Academy for approval the names of those who may be considered worthy of the award.

The Treasurer announced that in accordance with the will of

[1] From *Science*, April 27.

the late Prof. James C. Watson the sum of about fourteen thousand dollars had been placed in his hands. When the estate is finally closed a further sum will be paid over to the Academy. The income of the Watson fund is to be used under the direction of three trustees—Messrs. J. E. Hilgard, S. Newcomb, and J. H. C. Coffin—for the purpose of aiding astronomical researches. In accordance with the recommendation of the trustees the Academy granted five hundred dollars from this fund towards defraying the expenses involved in observations of the total solar eclipse of May 6, 1883.

Later in the meeting Prof. Simon Newcomb of Washington was elected Vice-President, and Prof. Asaph Hall of Washington Home Secretary. Five new members were elected: Prof. A. Graham Bell of Washington, Dr. J. S. Billings, U.S.A., of the U.S. Army Medical Museum, Washington; G. K. Gilbert of the U.S. Geological Survey; H. B. Hill and C. L. Jackson, Professors of Chemistry in Harvard College. The whole number of members is now ninety-five.

On the afternoon of Thursday the Academy adjourned to take part by invitation in the ceremonies attending the unveiling of the statue of Prof. Henry in the grounds of the Smithsonian Institution. The time for these ceremonies was purposely fixed to coincide with that of the spring meeting of the Academy. Henry was preeminently a scientific man, and at the time of his death President of the Academy; and yet the members of the Academy were placed far down the line in the procession—after the Commissioners of the District of Columbia, and after officers of the army and navy. This fact must be regarded as evidence of a lack of appreciation of the relations existing between Henry and the Academy and of the true worth and dignity of science.

The exercises, which were in good taste, began with a short address by Chief Justice Waite. After this, at a signal, the covering was quickly drawn aside, instantly revealing the entire statue. Loud applause followed, those who were seated rose to their feet, and all hats were removed. The scene was highly impressive; and when the Philharmonic Society, accompanied by the full marine band, burst forth with Haydn's grand chorus, "The heavens are telling," the heart must have been a hardened one which did not experience a feeling of exaltation.

In the opinion of all, the statue is dignified and pleasing, and vividly calls to mind the honoured original. President Porter's oration, which was the principal event of the afternoon, was listened to with much interest. It dealt with the plain facts of the life of Henry, and was all that his best friends could have desired.

Among the pleasantest social features of the meeting was a reception given to the members of the Academy on Thursday evening by Prof. A. Graham Bell. There were present many well-known gentlemen, among them General Sherman, Chief Justice Waite, Senator Sherman, ex-Secretary Blaine, and the Japanese, Swedish, and Belgian ambassadors.

SCIENTIFIC SERIALS

Zeitschrift für wissenschaftliche Zoologie, Bd. xxxviii. Heft 1, February 20, 1883, contains:—On the vascular system and the imbibition of water in the Najadæ and Mytilidæ, by Dr. Hermann Griesbach (Pl. 1).—Researches among the Protozoa, by Dr. A. Gruber (Plates 2 to 4); describes and figures several new genera and species.—On the origin of the saliva (*Futter saft*) and the salivary glands in the bee, together with an appendix on their olfactory organ, by Dr. P. Schiemenz (Plates 5 to 7).—On the development of the red blood corpuscles, by Dr. W. Feuerstack (woodcuts).—Candid reply to my critics in the matter of the "Brain of Fishes," by G. Futsch.

Proceedings of the St. Petersburg Society of Natural History, Vol. xiii. Part 1, for 1882, contains: On the archæology of Russia, by Count Tivatkov (the Stone Period).—Notes of a journey on the Dnieper in 1844, by Dr. Kessler.—On *Capra caucasica*, Güld., by H. Dinnik.—Darwinism from the point of view of universal physical science, by A. Beketov.—A monograph of the Mysidæ to be found in Russia (Marine, Lacustrine, and Fluviatile), by Voldemaro Czerniavsky, fasc. 2. All the above articles are in Russian except the last, which is in Latin, and it is illustrated by four lithographic plates.

Journal of the Russian Chemical and Physical Society, vol. xv. fascicule 3.—On the hydrocarbon $C_{12}H_{20}$ obtained from the allyl dimethyl carbinol, by Prof. A. Zaytseff and W. Nicolsky.—On the hydrocarbon $C_{10}H_{18}$ obtained from the allyl dipropyl carbinol, by S. Reformatsky. It is a colourless liquid boiling at about 158° Celsius, insoluble in water, and easily soluble in alcohol and ether. It rapidly absorbs the oxygen of the air; density 0·787 at 0°, 0·774 at 16°, and 0·770 at 21°.—Chemical analysis of Kieff clays, by S. Bogdanoff. The white clay contains 96 per cent. of kaolins; the loess contains 83·5 per cent. of quartz, felspar, mica, and other silicates, 5·38 of kaolin, and 6·73 of carbonate of lime.—On the diisooctyl, by A. Alechin.—On the composition of the water which accompanies the naphtha and is discharged by mud-volcanoes of the Government of Tiflis, by A. Potylitzin (second paper).—An elementary demonstration of the pendulum-formula, and on a differential aërial calorimeter, by W. Preobrajensky.

THE *Archives des Sciences Physiques et Naturelles* for February, 1883, contains papers by C. E. Guillaume on electrolytic condensers; by Emile Yung, on the errors of the senses, a contribution to the study of illusions and hallucinations; by Ernest Favre, on the Geological Survey of Switzerland for 1882, concluded in the March number. To the latter C. de Candolle sends an interesting essay on the ripple marks formed on the surface of sands under water, and on other analogous phenomena.

THE *Journal de Physique théorique et appliquée* for March contains papers by Ph. Gilbert, on the experiments best suited for demonstrating the rotation of the earth; by G. Lippmann, on Helmholtz's theory of double electric layers as applied to electro capillary phenomena; by H. Pellat, on the same subject; by A. Rosenstiehl, on the definition of complementary colours; by Ch. Cros and Aug. Vergeraud, on a direct positive photographic paper.

SOCIETIES AND ACADEMIES

LONDON

Royal Society, March 15.—"On the Changes which take place in the Deviations of the Standard Compass in the Iron Armour-plated, Iron, and Composite-built Ships of the Royal Navy on a considerable change of Magnetic Latitude." By Staff-Commander E. W. Creak, R.N., of the Admiralty Compass Department. Communicated by Capt. Sir F. J. Evans, R.N., K.C.B., F.R.S., Hydrographer of the Admiralty.

The period comprised between the years 1855–68 was one of active research into the magnetic character of the armour-plated and other ships of the Royal Navy and iron ships of the Mercantile Navy.

Among other contributions to this subject a paper by F. J. Evans, Staff-Commander R.N., F.R.S., and Archibald Smith, F.R.S., was read before the Royal Society in March 1865, relating to the armour-plated ships of the Royal Navy, and containing the first published results of the system of observation and analysis of the deviation of the compass established four years previously.

From lack of observations in widely different magnetic latitudes the authors of that paper were unable to define the proportions of the semicircular deviations arising from vertical induction in soft iron and that arising from permanent or sub-permanent magnetism in hard iron.

During the last fifteen years vessels of all classes—except turret ships—have visited places of high southern magnetic inclination or dip, and the analysis of the deviations of their standard compasses has been made, showing the constants of hard and soft iron producing semicircular deviation.

The constants for soft iron provide a means of predicting probable changes of deviation on change of magnetic latitude for certain vessels of the following classes, and others of similar construction.

1. Iron armour-plated ships.
2. Iron cased with wood.
3. Iron troopships.
4. Iron and steel cased with wood.
5. Composite-built vessels.
6. Wooden ships with iron beams and vertical bulkheads.

These vessels were all in a state of magnetic stability previous to the observations which have been discussed, and their compasses have had the semicircular deviation reduced to small values, or corrected, in England by permanent bar magnets.

This correction may be considered as the introduction of a permanent magnetic force acting independently, and in opposition to the magnetic forces of the ship proceeding from hard iron.

It is now proposed to consider the effects of a change of magnetic latitude on the component parts of the deviation.

Semicircular Deviation

On semicircular deviation from fore and aft forces, time has but little effect, and the greater part of it is due to permanent magnetism in hard iron which may be reduced to zero for all latitudes, by a permanent magnet.

A second but small part of this semicircular deviation proceeds from sub-permanent magnetism in hard iron. It is subject to alterations slowly by time, from concussion, and from the ship remaining in a constant position with respect to the magnetic meridian for several days, and is more intensely affected by a combination of the two latter causes.

Deviations from sub-permanent magnetism which have temporarily altered in value as described, return slowly to their original value on removal of the inducing cause.

The principal cause of change in the semicircular deviation on change of magnetic latitude, in corrected compasses, arises from vertical induction in soft iron, which changes directly as the tangent of the dip.

In standard compasses judiciously placed with regard to surrounding iron this element of change is small and similar in value for similar classes of ships.

With very few exceptions, nearly the whole of the semicircular deviation from transverse forces is due to permanent magnetism in hard iron subject to the same laws as that proceeding from fore and aft forces.

In the exceptional cases alluded to there is a small part due to vertical induction in soft iron, changing directly as the tangent of the dip.

Quadrantal Deviation

This deviation is caused by induction in horizontal soft iron symmetrically placed, and it does not change with a change of magnetic latitude. Time alone appears to cause a gradual change in its value during the first two or three years after the ship is launched, when it becomes nearly permanent.

The diminution of the mean directive force of the needle which is common to all modern vessels of war, improves slowly at first by lapse of time, and finally assumes a permanent value.

Relative Proportions of Hard and Soft Iron

It has been found that the relative proportions of the hard and soft iron affecting the standard compasses of twenty-five vessels examined differ considerably, even in ships of similar construction.

This difference may be accounted for by the compasses not being placed in the same relative position in the ships, considered as magnets of various forms and containing numerous iron bodies introduced during equipment.

General Conclusions

The following general conclusions have special reference to the standard compass positions in the six classes of vessels previously mentioned.

1. A large proportion of the semicircular deviation is due to permanent magnetism in hard iron.

2. A large proportion of the semicircular deviation may be reduced to zero, or corrected, for all magnetic latitudes, by fixing a hard steel bar magnet or magnets in the compass pillar, in opposition to, and of equal force to, the forces producing that deviation.

3. A very small proportion of the semicircular deviation is due to sub-permanent magnetism, which diminishes slowly by lapse of time.

4. The sub-permanent magnetism produces deviation in the same direction as the permanent magnetism in hard iron, except when temporarily disturbed (1) by the ship's remaining in a constant position with respect to the magnetic meridian for several days, (2) by concussion, or (3) by both combined, when the disturbance is intensified.

5. To ascertain the full value of changes in the sub-permanent magnetism, observations should be taken immediately on the removal of the inducing cause.

6. In the usual place of the standard compass the deviation caused by transient vertical induction in soft iron is small, and of the same value (nearly) for ships of similar construction.

7. The preceding conclusions point to the conditions which should govern the selection of a suitable position for the standard compass with regard to surrounding iron in the ship.

Anthropological Institute, April 24.—Prof. W. H. Flower, F.R.S., president, in the chair.—The election of Mr. C. Roberts, F.R.C.S., was announced.—Mr. W. M. Flinders Petrie read a paper on the mechanical methods of the Egyptians. The author exhibited several specimens of ancient Egyptian work, and described the methods by which he believed them to have been produced.—Mr. F. C. J. Spurrell read a paper on some palæolithic knapping tools and modes of using them.

May 8.—Prof. W. H. Flower, F.R.S., president, in the chair.—Mr. Frederick Bonney read a paper on some customs of the aborigines of the River Darling, New South Wales. The tribes with which the author was most familiar are called Bungy-arlee and Parkungi. They inhabit a district within lat. 29°–34°. S., long. 141°–146° E. The country in its natural state was incapable of supporting a large population, being subject to protracted droughts, during which both food and water were scarce. There is a similarity in the typical features of all the Australian aborigines, but to a close observer each tribe has its own peculiarities. Though ugly and unprepossessing in appearance, they are most kind, gentle, and of quite average intelligence and morality. The aborigines of Australia are often spoken of as the lowest type of humanity, but the author considered this to be a libel on the whole of them, and was positive it is so as regards the tribes he knows best. Mr. Bonney then proceeded to give a description of the life-history of the above-mentioned tribes.—Lieut.-Col. H. H. Godwin-Austen, F.R.S., read a paper on the discovery of some worked flints, cores, and flakes from Blackheath, near Chilworth and Bramley, Surrey.—A paper by Admiral F. S. Tremlett, F.G.S., was read, on stone circles in Brittany, in which the author described three circles discovered by the late Mr. James Milne, in the commune of Carnac; they had presumably been places for cremating the dead, and also for depositing the urns; the greater part of the latter were found inclosed in cists of quartz covered over by a slab of schist, neither of which are to be found in the district.—Mr. W. Galloway exhibited a skull and a number of rubbed bones and other implements from the islands of Oronsay and Colonsay, forming part of a large collection exhibited by him in the Great International Fisheries Exhibition.

Physical Society, May 12.—Prof. Clifton in the chair.—New Member, Mr. A. W. Soward.—Mr. Woodward described an experiment illustrating motion produced by diffusion. A porous reservoir of clay containing air was suspended from one end of a weighted balance beam. A glass tube projected from it below and dipped into a vessel of water. A jet of hydrogen gas was allowed to play on the outside of the reservoir and the balance beam began to oscillate. This is an adaptation of Graham's well-known experiment, and is in fact a diffusion engine. Prof. Adams explained the action by the variation of pressure in the reservoir set up by diffusion.—Mr. W. Lant Carpenter read a paper on some uses of a new projection lantern. This lantern, of German make, is applied by Mr. Paterson, and is simple in construction, cheap, and gives a good image visible to a large audience. It can be used with a three-wick oil lamp or the limelight. Mr. Carpenter showed a number of objects on the screen. Mr. Lecky and Mr. Woodward offered some remarks, the latter deprecating a too frequent use of projection with students.—Dr. C. R. Alder Wright read a paper on the electromotive force of Clark's mercurous sulphate cell and the work done during electrolysis. He described the best mode of constructing Clark's standard cell. According to numerous tests, these cells vary in E.M.F. about 0·2 per cent. + or − among themselves. A cell properly made will keep its value for about two years. It is of great importance that the cell should not be worked or the current reversed through it, otherwise it may permanently deteriorate. The extraction of air from the paste is not very essential, and boiling it is unnecessary. It is more important that the solutions of zinc sulphate should be saturated. Dr. Wright described a cell in vacuum which is a good standard. He found the E.M.F. to vary 0·4 per cent. between 0° and 100° C. With regard to the work done in a cell, among other interesting deductions, he found that in a secondary battery the larger the plates the greater the economy. In the electrolysis of water the greater the surface condensing power of the electrodes for gas the less difference of potential is required to decompose the water. Thus with platinum electrodes a lower E.M.F. serves for the electrolysis than with gold electrodes.—Prof. Foster then took the chair, and Prof. Clifton read a paper on a complete determination of a double convex lens by lineal

measurements on the optical bench. This was a method (somewhat similar to that of Mr. Boys, previously described to the Society) for determining the four quantities of a lens on the bench by lineal measures, and without the use of the spherometer and prism. Experiments showed that it was about as accurate as the spherometer method.

Edinburgh

Royal Society, May 7.—Prof. Maclagan, vice-president, in the chair.—By request of the Council Prof. James Geikie gave an address on recent advances in the Pleistocene geology of Europe. The characteristic deposits of this period, which embraces the Palæolithic age of the antiquarians, were described in considerable detail—the terminal and ground moraines and other glacial remains, the fluviatile and lacustrine formations, and the cave deposits. The limits were indicated of the great Scandinavian ice-sheet, which pushed itself southward over North Germany and over the watershed of Central Russia, and westward across the German Ocean towards our islands, thereby modifying the trend of the native ice-streams that have left their traces all over our hills and round our coasts. As an indication of the great power of this agent it was mentioned that some portions of the brown-coal beds of Saxony which have been long worked are really not *in situ*, but have been pushed out of place by the ice-sheet. In describing the fluviatile deposits Prof. Geikie drew attention to a suggestion made by Darwin, that frozen snow accumulating in the valleys below the glacier limits might easily act as barriers and give rise to extensive flooding. The fauna and flora and the evidence of the interglacial beds were then touched upon, and the address ended with a general summary of results with special reference to the climatic peculiarities of the Pleistocene period. It thus appeared that Europe was subjected to great climatic changes, severe glacial periods alternating with times of peculiar equable climate in which temperate flora and fauna flourished side by side with forms which are now met with only in southern regions.

Sydney

Linnean Society of New South Wales, March 28.—Rev. J. E. Tenison-Woods, F.L.S., vice-president, in the chair.—The following papers were read:—Occasional notes on plants indigenous in the immediate neighbourhood of Sydney (No. 3), by Edwin Haviland. This paper refers chiefly to the genus *Lobelia*, its mode of fertilisation, and its domestication.—On tooth-marked bones of extinct marsupials, by Chas. W. de Vis, B.A., A large proportion of fossil marsupial bones from the Darling Downs, recently examined by Mr. de Vis, are considered by him to show more or less decided traces of the action of the teeth of carnivorous animals. The tooth-marks are ascribed to the agency partly of the native dog, partly of the *Thylacoleo*, and partly of an extinct species of *Sarcophilus* which was identified by a portion of a tibia.—On *Brachalletes palmeri*, an extinct marsupial, by Chas. W. de Vis, B.A. A femur from the Darling Downs differs so markedly from that of *Macropus* and *Halmaturus* in the less prominent character of the great trochanter that it is considered to belong to a new generic type, proposed to be named *Brachalletes*.—On the habits of the "Mallee hen" (*Leipoa ocellata*), by K. H. Bennett. This gives an interesting and detailed account from the author's own observation of the nidification and general habits of this very curious bird.—Mr. Macleay exhibited a specimen of *Dendrolagus dorianus*, a new species of Tree Kangaroo from Mount Owen Stanley, New Guinea, described by Mr. E. P. Ramsay at the January meeting of the Society. He pointed out that the hair on the body all turned the wrong way.

Berlin

Physiological Society, April 13.—Prof. du Bois Reymond spoke about a series of electrophysiological investigations which he began at the same time as his "Investigations in Animal Electricity," which have long since been incorporated in science, now forty years ago, and about which he has as yet not published anything, viz., about the secondary electromotor phenomena of muscles, nerves, and electric organs. These latter are distinguished from primary electromotor phenomena of nerves and muscles by the fact that the latter appear in quiescent organs and take place without being directly influenced by an external electric current, whereas the former appear only as a consequence of an extrinsic electrical current, and consequently are connected with the polarisation appearances in electrolytical conductors. When a current is led through a fluid electrolyte by means of metallic electrodes, a reverse (negative) polarisation current is, as is well known, produced between the electrodes by the accumulation of ions on the anode and cathode. In the year 1836 Peltier described a similar negative (in direction opposed to principal current) polarisation in masses of frogs' limbs through which an electrical current was being passed, and explained it in the same way by the development of ions on the electrodes. When Prof. du Bois Reymond repeated this experiment in the beginning of the forties, he found that an electromotive force was active not only at the electrodes, but that each piece of the preparation through which the current was passing had a negative electromotive reaction, and showed an opposite current to the polarising one in a galvanometer that was applied. On further study of this phenomenon, he found this "inner" polarisation in every porous conductor, which is soaked with a readily conducting electrolyte, and it was in all cases negative; on the other hand an outer positive polarisation exhibited itself on the line of contact of dissimilar electrolytes, *e.g.* when the current was led through a pad soaked with water into a salt solution. Fresh animal tissues of the most different kinds, when a current was led through them between pads soaked in common salt, accordingly showed an outer positive and an inner negative polarisation. Further, the lecturer studied an outer and an inner secondary (called forth by the current) resistance, of which the former was at least partially accounted for by the cataphorical action of the current. When afterwards (*i.e.* after the determination of the above-mentioned physical phenomena) the inner polarisation was studied on living muscles, secondary electromotor appearances of such irregularity and complexity manifested themselves that it was only after laborious investigations that were extended over many years that the simple law that the phenomena obey was discovered. It was discovered that when a current was passed through a muscle the inner polarisations might be positive as well as negative, that they depend on the density and length of duration of the polarising current, and that each of these polarisations can be altered in a different manner by these two factors. If the densities and duration of action of the primary current are properly graduated, the phenomena follow the following law:—With very weak polarising currents the inner polarisation is negative, and increases up to a certain limit with the duration of the current; with somewhat stronger currents, the inner polarisation is at first positive, but soon passes over into the negative, which goes on increasing with the duration of the current; with still stronger currents, the initial positive inner polarisation becomes stronger and longer lasting, and then again becomes negative with the longer duration of the primary current. If the density of the polarising current increases still more, the initial positive current becomes weaker and weaker, and finally disappears altogether, and gives way to a polarisation that is negative from the beginning. Accordingly there exists in the interpolar portion of a muscle that is traversed by a current, after a certain limit has been exceeded, a positive inner polarisation, which in a short time is replaced by a negative polarisation, and the deduction from these phenomena is that both secondary electromotive forces—those with the same and with opposite directions—are present in the portion of muscle traversed by the electrical current. These electromotive forces manifest themselves alternately, the predominance of the one and the other being conditioned by the several dependence of each upon the density and duration of the primary current. This indication of a positive inner polarisation, *i.e.* of secondary electromotor forces, which occasion a current in the same direction as the primary current, is a fact of fundamental import in the theory of animal electricity. The positive polarisation proved itself to be dependent upon the direction of the primary current, since it was stronger in the upper half of the muscle when the direction of the current was from below upwards, whereas it was stronger in the lower half with a descending current; furthermore it manifested itself in living muscles only, whereas the negative polarisation occurred also in muscles that had been boiled or otherwise killed; finally, the positive polarisation was less strong in active than in quiescent muscles. At the end of the fifties the lecturer had also succeeded in demonstrating a positive inner polarisation in nerves; it showed the same regularity as was afterwards, with finer appliances, quantitatively estimated in muscles; that is to say, with small current-densities a negative polarisation only was manifested; with greater current-densities and very short duration of closing a purely positive polarisation was manifested,

which passed over into a negative polarisation with the longer duration of the primary current. Here also the different manifestations of the nerve polarisation led, as in muscles, to the recognition of two simultaneous electromotive forces, which behave differently to the intensity and duration of the primary current. And as in muscle the direction of the primary current influenced the strength of the positive polarisation, similarly in nerves the direction had an influence upon the positive polarisation predominating in the motor nerve-roots when the current was a descending one, and conversely in the sensory nerve-roots when the current was an ascending one; consequently both times the direction of the physiological nerve-wave predominated. Finally, Prof. du Bois Reymond gave an account of his experiments by which he has demonstrated quite analogous secondary electromotor phenomena in the electric organs of the electric fish (Malapterurus). In the theoretical discussion of the results of these experiments that were carried on for so many years the lecturer pointed out in conclusion that the inner polarisation, the positive polarisation in particular, could scarcely be otherwise explained except by the hypothesis that in the above-mentioned organs (the muscles, nerves, and electric organs) electromotor molecules preexisted during life, which, being turned by the polarising current, became the occasioned causes of the electromotor phenomena.—Prof. Rosenthal of Erlangen spoke about the experiments he had made to ascertain the electric conductivity of living tissues. He dwelt on the difficulty of exactly measuring its amount, which he could only overcome by using alternating currents, of which, by the help of a particular apparatus, currents of one direction only acted upon the galvanometer of the Wheatstone's bridge. On the living man he found the resistance of the epidermis so great that he regards it as an excellent insulator which permits the electrical current to pass through to the deeper organs only through the medium of the canals (the pores) that ramify through it and that are filled with fluid. The measurements of the conductivity of living animal tissues are not yet quite completed.

PARIS

Academy of Sciences, May 7.—M. Blanchard in the chair.—M. Loewy explained his new method for determining at any moment the relative position of the instrumental equator in relation to the real equator. This method is analogous to that already given for right ascensions, being founded on the observation of the stars near the pole, and on the variations in the relations of the coordinates due to the deflection of the instrument. M. Loewy demonstrates mathematically that his plan combines all the theoretical and practical conditions required for the complete solution of the problem. It is based on the theorem here demonstrated that when the track described by a star in apparent distance from the pole coincides with its distance in relation to the instrumental plane, the angle may be exactly determined which is formed by the terrestrial axis with the line of the instrumental poles, by means of the variation observed between the apparent polar distance and the distance in relation to the instrumental plane. The method is independent of any possible variations in the state of the instrument during a period of twelve hours, and it excludes the cause of systematic error due to refraction. It is moreover capable of extreme accuracy, which, by multiplying the points, may be carried as far as is desirable.—M. Tresca submitted some remarks on the observations made last year by Prof. Lemström in Lapland on various circumstances connected with the phenomenon of the aurora borealis, which have been reported in NATURE.—M. Th. du Moncel presented a paper by M. E. Semmola on the annual variation of temperature in the waters of the Bay of Naples, showing the results of observations made during the summer of 1879 and January, 1880, with a Negretti and Zambra thermometer. The observations were generally taken during calm weather between the hours of 11 a.m. and 3 p.m., in depths of 30 or 40 feet, and at some distance from the coast. They showed that on the whole the Bay of Naples is only a few degrees warmer than the Mediterranean, which, from the observations made in the August of 1870 by the English expedition under Prof. Carpenter, was found to be 25° C. at the surface, 15°·5 at a depth of 180 feet, 14° at 230, 13° at 620, and nearly the same down to 10,000 feet. In the bay the temperature varied from 13° on the surface in winter to 27° in summer, showing a mean of about 20°, or 3° higher than the city of Naples. This result also agrees with the mean annual temperature of the Mediterranean, which, according to Mohn, lies between 16° and 19° in the west, and 21°–23° in the east.—Other papers were contributed by M. Lecoq de Boisbaudran on the extremely sensitive character of salts of iridium, rendering them most useful in detecting the presence of the smallest particles of iridium in compound substances; by G. A. Hirn, continuing the *résumé* of the meteorological observations made during 1882 at four points of the Upper Rhine and Vosges highlands; by Th. Schwedoff, on the form of the great comet of September, 1882, with two cuts showing its appearance on October 12 at Lyons, and on October 17 and November 7 at Odessa; by E. de Jonquières, on the identities presented by the reductions belonging respectively to the two "modes" of continuous periodical fractions. By "the two modes" of continuous fractions the author understands, on the one hand the ordinary continuous fractions ("first mode"), on the other those in which the numerators differ from unity ("second mode").—Papers were also submitted by M. Vieille, on the specific heats of some gases at high temperatures; by C. Resio, on the electrodynamograph, an instrument constructed for recording the work executed by machinery; by J. A. Le Bel, on the amylic alcohol developed in alcoholic fermentation; by M. Gonnard, on the staurolites and regular groupings of the felspar crystals in the siliceous porphyry of Four-la Brouque, near Issoire (Puy-de-Dôme); by J. Thoulet, on the elasticity of rocks and minerals; by P. Mégnin, on the direct reproduction of tænia in the intestines of the dog and man; by B. de Chancourtois, on a common meridian and measurement of time in view of the universal adoption of a complete decimal system, with a planisphere showing two proposed initial meridians passing through Behring Strait and the Azores; by Ch. Contejean, on some special cases of distribution in the Italian flora.

CONTENTS

THURSDAY, MAY 31, 1883

HUMAN FACULTY AND ITS DEVELOPMENT

Inquiries into Human Faculty and its Development. By Francis Galton, F.R.S. (London: Macmillan and Co., 1883.)

AMONG all his anthropological brethren Mr. Francis Galton has no competitor in regard to the variety and versatility of his researches. So various and versatile, indeed, have these researches been, that, with the exception of "Hereditary Genius" and "English Men of Science, their Nature and Nurture," we have become accustomed to regard them as disconnected pieces of work, which from time to time were thrown off like sparks from the flame of an active mind. But in the present volume he has collected in one series most of the investigations which he has separately published during the last ten years, and this collection when read in the light of a considerable amount of additional matter, clearly shows that the sundry investigations which were separately published were not separately conceived, but have throughout been united by the bond of a common object. This object, as the title of the book indicates, is that of inquiry into Human Faculty and its Development. And it is evident, when this fundamental note is supplied, that it serves to join not only the researches contained in the present volume, but also those of its above-named predecessors, into one harmony or design.

But although there is one harmony pervading this work, the changes of theme are so numerous that we shall not be able to touch upon them all, and must therefore restrict ourselves to considering the more important.

The book begins with an essay on "Variety of Human Nature," as to features, bodily qualities, energy, sensitivity, special senses, &c. In the course of this chapter the leading results of the author's well-known investigations on composite portraiture are brought in, the audibility of high notes in different individuals, as well as in different species of animals, &c. Next there follows a chapter on "Anthropomorphic Registers," which is mainly directed to showing the desirability of keeping family records of the anthropometry of children until they are old enough to continue the records for themselves. To facilitate this process—which he deems to be one of much practical importance in view of all that is now known touching the potency of hereditary influences—Mr. Galton urges that anthropometric laboratories should be established where all the needful periodic portraiture and other observations on the life-history of children should be made and preserved on the payment of small fees by the parents. Without such systematic observation any one may pass through life without knowing that he presents so strongly marked a peculiarity as that of colour-blindness; while the benefit to the race, a few generations hence, of a large mass of statistics of such consecutive anthropometry of numerous families would probably be of the utmost value. Indeed this suggestion as to anthropometric laboratories may be taken as the foundation of Mr. Galton's proposed science of "eugenics," to a tracing of the main principles of which his work on "Human Faculty" is chiefly concerned.

After a chapter on "Statistical Methods," we come to a consideration of "Character." So far as sex is concerned, "one notable peculiarity in the character of the woman is that she is capricious and coy, and has less straightforwardness than the man . . . and there can be little doubt as to the origin of the peculiarity. . . . The willy-nilly disposition of the female in matters of love is as apparent in the butterfly as in the man, and must have been continuously favoured from the earliest stages of animal evolution down to the present time. It is the factor in the great theory of sexual selection that corresponds to the insistence and directness of the male. Coyness and caprice have in consequence become a heritage of the sex, together with a cohort of allied weaknesses and petty deceits, that men have come to think venial and even amiable in women, but which they would not tolerate among themselves."

The type of character which leads to criminality is next discussed, and is shown by statistics to be strongly inherited. After a few pages on the allied topic of insanity, Mr. Galton passes on to consider the gregarious and slavish instincts, where he shows from first-hand observations on wild or but partly domesticated animals the immense utility of these instincts. We ourselves inherit from our savage ancestry instincts of the same kind, and thus it is that the less intellectually developed among us are so prone to submit ourselves, like sheep, to the guidance of a leader, and even to the tyranny of a despot.

Passing on to intellectual differences, a long and interesting account is given of mental imagery, the main points of which are already known to the readers of NATURE. It is remarkable that men of science, and of hard thinking generally, are for the most part totally deficient in this faculty. The discussion of mental imagery naturally leads to the resemblance which Mr. Galton has previously pointed out between his composite photographs and general ideas; each alike are "generic images," and in many matters of detail the analogy, or, as we should prefer to call it, the illustration, holds good.

Next we come to a chapter on Psychometric Experiments, which is devoted to an account of interesting experiments on the association of ideas. The influence of early association and sentiment is shown by these experiments, and by considerations drawn from them, to be much greater than is generally supposed.

One of the most interesting chapters in the book is that which next follows on the History of Twins. It will be remembered that the main fact elicited by this inquiry is that nature counts for much more than nurture; for it is shown that "instances exist of an apparently thorough similarity of nature, in which such difference of external circumstances as may be consistent with the ordinary conditions of the same social rank and country do not create dissimilarity. . . . The twins who closely resembled each other in childhood and early youth, and were reared under not very dissimilar conditions, either grow unlike through the development of natural characteristics which had lain dormant at first, or else they continue their lives, keeping time like two watches, hardly to be thrown out of accord except by some physical jar. . . . The effect of illness, as shown by these replies, is great, and well deserves further consideration. It appears that the constitution of youth is not so elastic as we are apt to think; but that an attack, say of scarlet fever, leaves a permanent

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mark, easily to be measured by the present method of comparison."

The essay which follows on the "Domestication of Animals" is not so interesting, because not so original, as the rest of the book; all its points are obvious to any one who has thought about the subject at all.

A consideration of the Possibilities of Theocratic Intervention next leads the way to a reappearance of the author's paper on the Objective Efficacy of Prayer. Here the logic is unexceptionable as far as it goes, but it is not such as to leave no loophole of escape for orthodox belief. The argument is that if prayer is of any avail in an objective sense, it ought to admit of being shown by the statistical method to be so. But, as the present writer pointed out nine years ago when considering this essay, the statistical method applied to such a case is of doubtful validity. To show this we may quote one paragraph from our previous criticism:—

"What, then, is the whole state of the case? To illustrate it most fairly, we shall take the strongest of the examples supplied by Mr. Galton, viz. that of the Clergy. As Mr. Galton truly observes, in no other class are we so likely to obtain men of Prayer. Suppose, then, for the sake of calculation, that one-half of the clergy are sufficiently prayerful to admit of their petitions influencing the course of physical phenomena. Next, let us suppose that one-half of their successful petitions for physical benefits are offered on behalf of individuals other than themselves: this is equivalent to reducing the number of the prayerful clergy to one-fourth. Here we ought to add that in whatever degree this section of successful prayers may influence the prayerless classes of the community, in that degree is the comparison still further vitiated. Neglecting this point, however, let us lastly suppose that one-half of the petitions for physical benefits offered on the petitioner's own behoof are answered by physical benefits of some other kind; . . . this is equivalent to reducing the original number to one-eighth. Now I do not think any of these suppositions are extravagant. Let us see the result of applying them to Mr. Galton's tables. According to these tables, the clergy as a class live, on an average, two years longer than men of any of the other classes quoted, notwithstanding we are repeatedly told that, as a class, they are the most poorly constitutioned of all. Now, neglecting the last-mentioned point, and also the fact that all clergymen do not pray for long lives; still, even on the above data, an average of two additional years over all the clergy allows, when concentrated into one-eighth of their number, an average of sixteen additional years of life to every pious divine. Of course this illustration is not adduced in order to prove that prayer has in this case been observably effectual. The greater length of life enjoyed by the clergy may be conceded due to the cause assigned by Mr. Galton—viz. the repose of a country life—or to any other cause, without in any way affecting the present argument. All we are engaged in showing is that the statistical method is not a trustworthy instrument wherewith to gauge the physical efficacy of prayer; and the above illustration has been adduced to show that even if the petitions of the pious clergy for lengthened days were somewhat more effectual than those of Hezekiah, statistics would still be so far unable to take cognisance of the fact that the observable average increase of two years over the entire body of the clergy might reasonably be attributed to other causes. Yet length of days is perhaps the most conspicuous, and therefore the most easily tabulated, of all physical benefits for which it is possible to pray."[1]

After some well considered remarks on Enthusiasm, or "to what degree the strong subjective views of the pious are trustworthy," the book begins to draw towards its final object, which is virtually that of marking out the lines of what may appropriately be called a new religion. We have of late had so many manufactures of this kind that the market is somewhat glutted, and therefore it is very doubtful how far this new supply will meet with an appropriate demand; but we can safely recommend Mr. Galton's wares to all who deal in such commodities as the best which have hitherto been turned out. They are the best because the materials of their composition are honesty and common sense, without admixture with folly or metaphor. He says: "We may not unreasonably profess faith in a common and mysterious whole, and of the laborious advance, under many restrictions, of that infinitely small part of it which falls under our observation, but which is in itself enormously large, and behind which lies the awful mystery of all existence." Having, then, this faith in the seen, and observing that, whatever the far-off divine event may be to which the whole creation moves, the whole creation is certainly moving in an upward course of evolution, Mr. Galton submits that man has now reached a level of intelligence which should enable him, not merely to know these things, but to do them. He ought to "awake to a fuller knowledge of his relatively great position," and begin to regard it as his high prerogative to cooperate with the unknown Worker in promoting the great work. He may infer the course that evolution is bound to pursue, and might therefore "devote his modicum of power, intelligence, and kindly feeling to render its future progress less slow and painful. Man has already furthered evolution very considerably, half unconsciously and for his own personal advantages; but he has not yet risen to the conviction that it is his religious duty to do so deliberately and systematically."

Several directions in which such assistance might be yielded are pointed out in the concluding pages of the book, especially in the way of "eugenics"; and there can be no question that, if the idea of promoting evolution could become generally, or even largely, invested with a feeling of obligation, the prospects of the race would be greatly brightened. The most important field of human activity under such circumstances would obviously be that of improving the race by selection, and Mr. Galton throws out several well considered suggestions as to the way in which this might be done without violating so precious a product of evolution as the moral sense, or seriously interfering in any other particular with the ordinary usages of civilised life.

We have said enough to show that in respect of its matter "Human Faculty" is an unusually interesting work; but we should not do it justice were we to conclude this brief notice without alluding also to its manner or style. There is a strand of humour woven through the serious texture of the whole, which, together with the ingenious cast of thought and the ingenuous cast of feeling, affords a most pleasing and instructive study, unconsciously presented, of the nature and nurture of an English man of science.

GEORGE J. ROMANES

[1] Burney Prize Essay on "Christian Prayer and General Laws," pp. 265-6 (Macmillan and Co., 1873), where other and more important considerations against this application of the statistical method are given. [I may observe that this essay was written on a thesis which was set by the Vice-Chancellor of Cambridge, and I still think that, upon its given basis of Christian belief, all the more important of its arguments hold, both as regards prayer and miracles.—G. J. R.]

THE GEOLOGICAL HISTORY OF BRITAIN

Contributions to the Physical History of the British Isles; with a Dissertation on the Origin of Western Europe and of the Atlantic Ocean. Illustrated by 27 Coloured Maps. By Edward Hull, F.R.S., &c. (London: Stanford, 1882.)

IF Geology may be correctly described as a history of the earth, then a geologist is in the first place and essentially a historian. His function is to trace back the gradual growth of the world, organic as well as inorganic, and to show through what successive stages the present conditions of geography and of life have been reached. His materials, like those of the historian of human progress, become fewer and less reliable in proportion to their antiquity. More and more as he pilots his way into the records of the remoter past is he driven to piece together their evidence with conjecture, until at last evidence of every kind fails him, and he is reduced to mere speculation. There is undoubtedly a strong temptation to minds of a particular order to indulge in wide excursions into the unknown realms of primeval cosmogony. The fewer the facts that may serve as guide-posts the greater the scope for the fancy. So long as the picture does not appear to outrage our established conceptions of physical law its enthusiastic limner considers himself within the safe limits of fact or, at least, of legitimate inference. He does not stop to consider whether his restoration may not in itself be flagrantly improbable, or whether enough may not be already known on the subject to show that it is quite untenable. In this way much harm has been done to the progress of sound geology.

The attempt to restore former aspects of the globe, or at least of different areas of its surface, may be made with fair measure of success up to a certain point. As the geologist goes beyond that point he leans more and more on conjecture. It is very desirable, for his own sake as well as for that of the subject, that the actual data on which he proceeds should be definitely stated. His readers ought to know exactly where ascertained fact ends and restoration begins. Yet he may be so convinced of the truth of his restoration that, until challenged to set down in definite form the amount of evidence actually at his command, he may honestly have come to regard some of his deductions as well-established truths. He cannot, however, be too careful to draw a clear and sharp line between what he knows and what he infers, when it is his object to write geological history.

One of the most attractive branches of this history is that which deals with the gradual growth of a country or continent. Many interesting and important memoirs on this subject have appeared, more especially in England, where it has long been a favourite study. Sketch-maps have been published indicating in a somewhat vague way what the authors believe to have been the probable distribution of sea and land at former geological periods. Among those who by their original researches have contributed materials towards the restoration of ancient geographical conditions in Britain, Prof. Hull, the Director of the Irish Geological Survey, deserves honourable mention. His papers upon the changes that occurred during Carboniferous, Permian, and Triassic times, and upon the south-eastward attenuation of the Jurassic series in this country are well known to geologists. He has now, however, attempted a much more ambitious task than any one has yet ventured upon in this department of science. He has published a series of maps representing what he conceives to have been the successive geographical phases through which the region of the British Islands has passed from the earliest geological times. Without discussing the question whether the information at the disposal of geologists is yet sufficiently ample and precise to warrant an attempt of this kind, one may at least demand that every care should have been taken to show precisely what is actually known fact and what is inference. But Mr. Hull gives us scanty guidance in this respect. There is not one of his restorations that does not prompt the question on what grounds its details have been put together. The position of former areas of sea is usually sufficiently definable, but it is by no means so easy to say what was land, and still more difficult to assign even the most conjectural outlines to the shores. The author doubtless thinks his geographical boundaries vague enough; we are inclined to regard them as a good deal more definite than the actual evidence in many cases warrants. To take as an illustration his map of Britain during the Upper Silurian and what he terms the "Devono-Silurian" periods; we should like to know on what grounds he makes Wales, the Lake Country, the north-west of Ireland, and much of the Highlands of Scotland elevated land at that time. The evidence, so far as we are aware, is rather in favour of these areas having been under the Upper Silurian sea; at least we know of no proof that they formed high lands, even after the plication and metamorphism he refers to. Nor is there any information as to why the author marks the area from the mouth of the Humber to the middle of Norfolk as part of his continental land. He mixes up in a curiously unintelligible way his "Devono-Silurian" and Lower and Middle Devonian formations, some of the estuaries or lacustrine areas being placed with the older group of strata, others with the younger, in accordance with certain theoretical ideas which he has already published.

According to Prof. Hull's maps, most of the high grounds of Britain have been elevated dry land since the Lower Silurian period. No one, however, who has seriously studied how the land is continuously denuded, can believe this representation to be even approximately true. Our mountains must have been many times, and probably for long intervals, under water. Even if no large amount of sedimentary material were laid down upon them, their submergence would at least protect them from the degradation which would otherwise have worn them down. How does Prof. Hull know that Ireland, which was almost if not entirely under water during the Carboniferous period, did not remain more or less in the same condition through several succeeding ages? The presence of Permian and Triassic deposits in the north-east of the island shows that considerable denudation of the Carboniferous rocks had taken place there before these red strata were laid down. But surely it is rather a large inference from these slender data that all the rest of the country was land, with high grounds where we see them still. How can he tell that Ireland was not entirely submerged beneath the Jurassic sea? Had it not been

for the protecting sheets of basalt in Antrim, probably no fragment of Lias or Oolite would now have been left in the island. Prof. Hull submerges his country a little more in the Cretaceous period, but still keeps the high grounds as islands. Can he produce any evidence that they were so? Has he sounded the Cretaceous Ocean about which he is so precise? The denudation of Ireland has been unquestionably enormous, but had the country been above water as long as the Director of its Geological Survey imagines, we fear that every geological formation would have been worn off its surface down to the very platform of its fundamental or Laurentian gneiss. In fact the continued survival of the country above water could only have been maintained by repeated uplifts that in some measure at least compensated for its superficial degradation.

The chapters accompanying the maps furnish the reader with some of the information he requires to be able to estimate the extent of the data on which the restorations have been constructed. But they do not give him nearly enough of it. Some of their statements moreover will provoke criticism not less than the maps themselves. The author asserts, for instance, as if it were an established fact, that what he regards as the "essentially oceanic" conditions under which the Chalk was formed prevail from Ireland to the shores of the Caspian, and from Belgium to North Africa. We can hardly suppose him to be ignorant of the fact that the Chalk is but a local development of calcareous matter confined to the western part of the European area. Yet the author not only spreads the Chalk across most of Europe and into Africa and Asia, but proceeds to infer from this asserted extension that "according to all the laws of terrestrial mechanics" the site of much of the North Atlantic must have been then dry land. In other words, he first infers a wide deep ocean, and then creates a continent to keep it company.

One of the chapters, with the sounding title of "The Genesis of the North Atlantic Ocean," will be read with amazement by those who have watched the progress of recent research on this question. The author begins it by the following oracular announcement: "I date the genesis of the North Atlantic Ocean, properly so called, from the close of the Carboniferous period; and, consequently, from the same period, that of the British Isles and Western Europe." One is disposed at once to ask what may be his "exquisite reason" for this extraordinary statement, and he frankly volunteers it. It appears to be somewhat as follows:—The Carboniferous rocks of Western Europe were much disturbed at the close of the Carboniferous period, being thrown into east-and-west ridges. Similar movements took place over the eastern States of North America, the direction of the ridges being there more nearly north and south. It may be concluded, therefore, that the formation of the basin of the Atlantic Ocean formed part of these terrestrial movements!

In his Preface the author tells us how he had long entertained the idea of preparing such a series of maps as he has now published, and how he was deterred by the cost of publication. At last, in what we venture to think was an evil hour for his reputation, the Royal Dublin Society generously agreed to bear the expense. The maps were therefore prepared and published in the Society's *Transactions*, and a fresh impression has been printed off from the plates for the volume just issued. Fortune would have been kinder to one whose long services entitled him to gentle treatment at her hands had she induced him still to keep his restorations in the privacy of his own portfolio, at least for some years to come, or, if they must be published, had she insisted on greater accuracy in the statement of what is known and greater precision in the expression of what is conjectured.

OUR BOOK SHELF

Die Verwandlungen der Tiere. Von Dr. Otto Taschenberg, Privat-dozent in Halle. Pp. 268, with 88 Illustrations. Small 8vo. (Leipzig: G. Freytag, 1882.)

THIS forms the seventh volume of the series known as "Das Wissen der Gegenwart," the object of which is to give, in an attractive and popular form an outline of the "science of the day." Metamorphosis and development are always interesting subjects, and we are of opinion that Dr. Taschenberg has contrived to place them before his readers in a specially clear manner by choosing a few types in each class of the animal kingdom upon which to dilate, leaving the blanks to be filled in by more advanced students than those for whose instruction this elementary treatise is intended. The author goes in this manner through the entire animal kingdom, and so far as we can see he is well posted up in most of the latest discoveries and theories bearing upon his subject; we miss, however, any allusion to the disputed position of *Limulus*, although the metamorphoses of that remarkable animal are not entirely overlooked. A work such as this is naturally to a large extent a compilation, and in all such works the good or bad influence exercised depends upon the acumen of the author in his choice of subjects and authorities. In the present instance our author seems usually to have consulted the best and most modern authorities. The numerous illustrations are mostly very good; some of them are superlatively so. In these, as in the text, various works have been laid under contribution; and probably to no work is the author under greater obligation than the text-book on embryology by the lamented F. M. Balfour, but due acknowledgment is always made.

The concluding chapter is devoted to a sketch of the "evolution of species," in which, in a few pages, the author has contrived to give succinct historical information, winding up with a definition of "protoplasm," in connection with which a German translation from well-known English lines is given, and perhaps the definition was so modelled as to fit the lines. We reproduce them, just to show what latitude may be allowable in translation:—

"Der grosse Cäsar tot und Lehm geworden
Verstopft ein Loch wohl vor dem rauhen Norden.
O dass d e Erde, der die Welt gebebt,
Vor Wind und Wetter eine Wand verklebt."

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications.*

[*The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to insure the appearance even of communications containing interesting and novel facts.*]

Natural Selection and Natural Theology

I AM very glad to find from Prof. Asa Gray's last communication (NATURE, vol. xxviii. p. 78) that the result of our "amicable discussion" has been that of coming to an agreement on all

points save one, which, as he truly observes, is "seemingly capable of settlement by scientific inquiry." This point simply is as to whether variation in plants and animals is promiscuous (not "lawless") or is restricted to beneficial lines.

Now with reference to this point, I observed in my first letter (NATURE, vol. xxvii. p. 362) that if variation is promiscuous it is only the favourable variations that are able to survive, and hence the sole ground of entertaining natural selection as an agency in the process of evolution; but that, on the other hand, if it could be shown that variations always take place exclusively in the directions required for a development of the adaptations, so as to leave no room for the operation of natural selection, then the evidence of design as deduced from the theory of evolution would become comparable with that evidence as deduced from the theory of special creation. But I also pointed out that "the burden of proof lies with the natural theologian to show that there *has* been some such intelligent guidance of the variations, not with the evolutionist to show cause why there *may not* have been such guidance." And now I understand Prof. Gray accepts this as a correct statement of the case, observing in his last letter that, if variation is promiscuous, "then no doubt the theory of natural selection may be 'the substitute of the theory of special design,' so as to efface that evidence of underlying intelligence which innumerable and otherwise inexplicable adaptations of means to ends in nature was thought to furnish. If it is not so, then the substitute utterly fails."

It is most satisfactory to me that the issue has thus been clearly reduced to a simple matter of scientific observation, and I may add that I am much interested to find that a naturalist of such high standing as Prof. Gray still holds to the view that, "so far as observation extends, it does not warrant the supposition of omnifarious and aimless variation." Of course, if I had not believed in "aimless variation" as of universal occurrence in organic nature, I should never have supposed that the theory of evolution by natural selection could in any way touch the theory of special design; but finding that my fundamental belief in this matter is still open to question by so esteemed an authority as Prof. Gray, and observing that we are here upon the ground of a purely scientific question, I should like to say a few words in justification of this belief.

No one has attended to the subject of variation with a tenth part of the care that was bestowed upon it by Mr. Darwin, and no one has been gifted with a better judgment in matters of this kind. I shall therefore restrict myself to giving a brief outline of his matured opinion upon the subject.

Everywhere he speaks of variation as promiscuous or aimless, but never as "lawless," and only under a carefully guarded meaning as accidental. That is to say, he has no doubt that every variation is due to causes, though not of a teleological kind. Of these causes he regards changes of environment as highly important; but nevertheless he is inclined to lay less weight on these "than on a tendency to vary due to causes of which we are quite ignorant."[1] But with reference to variations not taking place exclusively in beneficial lines he says: "As man has domesticated so many animals and plants belonging to widely different classes, and as he certainly did not choose with prophetic instinct those species which would vary most, we may infer that all natural species, if exposed to analogous conditions, would, on an average, vary to the same degree. . . . We have abundant evidence of the constant occurrence under nature of slight individual differences of the most diversified kinds; and we are thus led to conclude that species have generally originated by the natural selection of extremely slight differences; . . . although each modification must have its own exciting cause, and though each is subjected to law, yet we can so rarely trace the precise relation between cause and effect, that we are tempted to speak of variations as if they arose spontaneously. We may even call them accidental, but this must be only in the sense in which we say that a fragment of rock dropped from a height owes its shape to accident. . . . If an architect were to rear a noble and commodious edifice without the use of cut stone, by selecting from the fragments at the base of a precipice wedge-formed stones for his arches, elongated stones for his lintels, and flat stones for his roof, we should admire his skill and regard him as the paramount power. Now the fragments of stone, though indispensable to the architect, bear to the edifice the same relation which the fluctuating variations of organic beings bear to the varied and admirable structures ultimately acquired by their modified descendants. . . . The shape of the fragments at the base of our precipice may be called accidental, but this is not strictly correct; for the shape of each depends on a long sequence of events, all obeying natural laws; . . . but in regard to the use to which the fragments may be put, their shape may be strictly said to be accidental. . . . Can it be reasonably maintained that the Creator intentionally ordered, if we use the word in any ordinary sense, that certain fragments of rock should assume certain shapes, so that the builder might erect his edifice? If the various laws which have determined the shape of each fragment were not predetermined for the builder's sake, can it be maintained with any greater probability that He specially ordained for the sake of the breeder each of the innumerable variations in our domestic animals and plants;—many of these variations being of no service to man, and not beneficial, far more often injurious, to the creatures themselves? Did He ordain that the crop and tail-feathers of the pigeon should vary in order that the fancier might make his grotesque pouter and fantail breeds? Did He cause the frame and mental qualities of the dog to vary in order that a breed might be formed of indomitable ferocity, with jaws fitted to pin down the bull for man's brutal sport? But if we give up the principle in one case,—if we do not admit that the variations of the primeval dog were intentionally guided in order that the greyhound, for instance, that perfect image of symmetry and vigour, might be formed,—no shadow of reason can be assigned for the belief that variations, alike in nature and the result of the same general laws, which have been the groundwork through natural selection of the formation of the most perfectly adapted animals in the world, man included, were intentionally and specially designed. However much we may wish it, we can hardly follow Prof. Asa Gray in his belief 'that variation has been led along certain beneficial lines,' like a stream 'along definite and useful lines of irrigation.'"[1]

I could give a number of other quotations to the same general effect from the writings of Mr. Darwin, but I think these are enough to show, as I have said, that if there is any evidence of variations being determined in special and beneficial lines, it now lies with the teleologist to adduce such evidence. If this could be done it would be a matter of immense importance, both from a scientific and a speculative point of view, seeing that on the scientific side it would be subversive of the whole theory of natural selection, and on the speculative side would therefore leave us where we were before the publication of the "Origin of Species." But at present the whole weight of such scientific evidence as we have appears to me unquestionably opposed to Prof. Gray's statement that, "so far as observation extends, it does not warrant the supposition of omnifarious and aimless variation."

GEORGE J. ROMANES

[1] "Origin of Species," 6th edition, p. 107.

[1] "Variations of Animals and Plants under Domestication." Second edition, vol. ii. pp. 401-2, 410, 416, 426-8.

Carson Footprints

IN NATURE (vol. xxvii. p. 578) which I have just seen, the Duke of Argyll calls your attention to the so-called human footprints uncovered in the prison yard at Carson, Nevada. I have carefully examined these tracks, and read a paper on the subject before the California Academy of Science, August 27, 1882. Unfortunately the *Proceedings* of the Academy have not yet been published, though copies of the several papers on this subject have been printed and privately distributed. Perhaps a brief account of these tracks will be interesting to your readers.

The nearly horizontal strata in which they occur consist of beds of sandstone with thin layers of fine shale. The track layer, which is one of these latter, has been uncovered over an area of nearly two acres, and forms the floor of the prison yard, while the stone removed has been used to build the prison. In the course of the excavation a number of fossils have been found, among which the most important are the jaws and teeth of an elephant, probably *E. Americanus*, and two species of horse, *Equus Pacificus* and *occidentalis*; some freshwater shells, all of recent species, have also been found. The age of the deposit seems to be that of the "Equus beds" of American geologists, which by some are put in the uppermost Pliocene, and by others in the lowest Quaternary. It is probably a transition between the two.

The whole surface of the shale exposed in the prison yard is literally covered with tracks of many kinds, but the mud was so soft when the tracks were made that the nature of many of them can only be guessed. Some were probably those of a horse; some probably of a wolf; some certainly of a deer;

many were those of long-legged wading birds. But the most interesting are those of the Mammoth and the problematical so-called human tracks. About the Mammoth tracks there can be no doubt. Some of these were uncovered by blasting in my presence; round basin-shaped impressions, 5 inches deep and 22 inches across, and occurring in regular alternating series, the hind-foot tracking almost perfectly with the fore-foot. The nature of the so-called human tracks, however, is far more doubtful. These occur in several regular alternating series of 15–20. In size they are 18–20 inches long, and 8 inches wide. In shape they are many of them far more curved than the human track, especially in soft mud. The stride is 2½ to 3 feet, and even more. The outward turn of the track is in many cases greater than in human tracks, especially in soft mud. But the most remarkable thing about them on the human theory is the straddle, *i.e.* the distance between the right and left series. This I found to be 18 and even 19 inches, which was fully as great as that of the mammoth tracks. This is probably the greatest objection to the human theory. On the other hand, the great objection to the quadrupedal theory is the apparent singleness of the tracks, and the absence of claw-marks. But it must be remembered that the tracks are deep, and the outlines somewhat obscure, and also that the mammoth tracks, on account of tracking of hind with fore-foot, are in most cases, though not always, single.

After careful examination for several days, the conclusion I came to was that the tracks were probably made by a large plantigrade quadruped, most likely a gigantic ground-sloth, such as the Mylodon, which is found in the Quaternary, or the Morothenium, which is found in the upper Pliocene of Nevada. The apparent singleness, the singular shape, and the large outward turn of the tracks I attribute to the imperfect tracking of hind and fore-foot on the same side, while the absence of claw-marks was the result of the clogging of the feet with mud.

This view seems to me most probable,[1] but many who have seen the tracks think them human, and I freely admit that there is abundant room for honest difference of opinion. On any theory the tracks are well worthy of scientific attention.

Berkeley, California, May 12 JOSEPH LE CONTE

[1] Views similar to my own have recently been expressed by Prof. Marsh and by G. K. Gilbert.

Cloudiness of Aquarium

CAN you tell me the reason why the water in my fresh water aquarium will not remain clear, but becomes cloudy throughout in a few days after filling.

The aquarium in question holds about twelve gallons of water. It stands in a window facing north. I have in the water two or three water-plants, among them a water-aloe. At the bottom are small gravel stones, which have been thoroughly washed before using. Floating on the surface for the benefit of a few newts is a piece of virgin cork, on which is placed some carpet moss. I had a dozen minnows and four newts to begin with, but nine of the minnows and two of the newts have died, manifestly from the fouling of the water.

The framework of the aquarium is iron, with a slate floor. The glass sides are fixed with red lead. There is a copper tube for overflow purposes, which was inserted when a fountain was used in the centre. This has now been removed and the water is stagnant.

It is now some years since I have kept an aquarium, and I cannot divine the reason for the above-mentioned cloudiness of the water. I shall be much obliged if you or some of your correspondents will help me. X.

May 9

So far as I can judge from "X's" description, the cloudiness of the water in his aquarium is due to the abnormal development of some unicellular algal (Palmellaceæ) or to the prolific spore-production within it of one of the filamentous forms (Confervaceæ). This may be obviated by screening the back of the tank from the access of light. Possibly "X" may find on examination that the cistern whence he obtains his supply has been left uncovered, and that the intruding algal has established itself and entered upon the reproductive process in that position. In that case he should either isolate the water he requires in a dark place for a week or so, when the spores will die, or obtain his supply from a purer source. An investigation with a high power of the microscope of the turbid water complained of will speedily determine whether the explanation here suggested is the correct one. By way of illustration, I may mention that the water of the ornamental pond in the centre of the Horticultural Gardens, supplied clear and bright shortly before the opening of the Fisheries Exhibition, had assumed within a few days and still retains the colour and consistency of green-pea soup through the rapid development, under the action of light, of a unicellular cryptogam in the manner above described. W. SAVILLE KENT

Singing, Speaking, and Stammering

REFERRING to the letters in NATURE (vol. xxvii. p. 580) on my classification of vowel sounds, allow me to explain:—

The classification given in the "Principles of Elocution" (4th ed., 1878) was retained from the earlier editions of that work, because of the difficulty, or impossibility, of exhibiting the complete vowel system of visible speech without V.S. symbols. For the purposes of the book on Elocution, the latter were not required; but a *note* (on p. 36) immediately preceding the "General Vowel Scheme" explains the basis of the complete classification developed in visible speech.

As you have given an abstract of my classification, quoted by Dr. Stone from "Principles of Elocution," I shall be glad if you will show your readers the following abstract of the visible speech classification:—

Classification of Vowels in Visible Speech

Nine Lingual positions yield 9 Primary vowels ... ...
Each Primary vowel yields a "Wide" variety by faucal expansion = 9 Wide vowels ... ... } = 18 Lingual vowels.
Each Lingual vowel yields a "Round" variety by labial contraction ... ... } = 18 Labio-lingual vowels.
} = 36 Normal vowels.
Each Normal vowel yields a possible variety by *higher*, *lower*, *broader*, or *narrower* formation = 36 + 144 = a total of 180 vowels.

The mutual relations of the different sounds may be exhibited in this way:—

LINGUAL.

	Primary.			Wide.		
	Back.	Mixed.	Front.	Back.	Mixed.	Front.
High	7	4	1	7	4	1
Mid	8	5	2	8	5	2
Low	9	6	3	9	6	3

LABIO-LINGUAL.

	Primary.			Wide.		
	Back.	Mixed.	Front.	Back.	Mixed.	Front.
High	7	4	1	7	4	1
Mid	8	5	2	8	5	2
Low	9	6	3	9	6	3

In this arrangement, each No. 1, No. 2, No. 3, &c., in the four sets is formed from one and the same lingual position. These relations are plainly exhibited in the symbols of visible speech. They cannot be shown by ordinary letters, but the use of *numbers*, as above, may make the arrangement clear to those who are not acquainted with visible speech.

Washington, D.C., May 12 ALEX. MELVILLE BELL

On the Cold in March, and Absence of Sunspots

I WAS travelling when Dr. Woeikof's letter appeared in NATURE (vol. xxviii. p. 53), and could not sooner reply to his criticisms on my communication (vol. xxvii. p. 551), "Unprecedented Cold in the Riviera—Absence of Sunspots." Let me first remark that I do not go so far as to "ascribe (as Dr. Woeikof says that I do) the great cold of March, 1883, at the

Riviera, to the absence of sunspots." My observations prove only the *coincidence* of a sudden and unprecedented visitation of cold, with an absence of sunspots (the more remarkable as occurring during a *maximum* sunspot period); and the further *coincidence* of a progressive rise in temperature with the return of the sunspots; but I add, "These observations are too few and too imperfect to warrant any decided conclusions; but they add to those already made in evidence of the connection between the absence of sunspots and the diminution of terrestrial heat; and I trust they may be followed by further and more exact investigations, to determine the influence of our great luminary on the weather and climate of the world."

It does not appear to me that Dr. Woeikof has succeeded in establishing a parallel between Cannes and Suchum-Kale on the Black Sea; which, however sheltered locally, must, far more than Cannes, be liable to chilling influences in the cold winds from the lofty mountains and vast elevated steppes to the north, extending even to the Arctic regions. Therefore the fall of 31° below average in March, 1874, might not be extraordinary, even in a *year* with a considerable number of sunspots. It is not stated that the spots continued in this particular month.

The case of Cannes may be thus stated: With a climate usually so mild in winter that frost and snow are of rare occurrence; and this winter, with slight frost only three times before February, and none at all in that month, the average minimum being 44°,—on March 7 minimum fell to 36°, with a heavy fall of snow; and on the 8th, 10th, 11th, and 12th, the minimum fell further to 27°·7, 27°, 24°·1, 25°·7. The sunspots, which had been observed by my friend, Mr. Campbell, of Islay, to be large and active until February 26, suddenly disappeared, and on February 28 and March 3 I found no spots; on the 10th and 11th only one or two small spots. On the 12th they began to appear in numbers, with a large oval facula. From that day they continued to increase, and the temperature gradually rose to the ordinary average.

I will not occupy space with further arguments, but I will merely state some more facts with regard to the extraordinary intensity and universality of this invasion of cold, and my further observations of the sunspots. At my villa at Cannes, which is favourably placed in position and shelter, the register did not fall so low as in other parts. At Dr. Frank's villa, Grand Bois, more open to the north (thermometer in louvred box, a metre above ground), the minima were: March 7, 27°; 9, 25°·2; 10, 21°; 11, 21°; 12, 20°; 13, 25°. At Villa Beaulieu, more sheltered (therm. also in louvred box), minima were: March 7, 29°; 9, 27°; 10, 25°; 11, 26°; 12, 28°. Dr. de Valcourt's minima are somewhat higher; but he adds this note: "La période de froid du 7 au 14 Mars, 1883, a été très remarquable; elle est unique, depuis que les observations regulières ont été recueillis à Cannes." Where instrumental records are wanting, we refer to the report of the "oldest inhabitants," and learn that there has not been a cold so severe or destructive to oranges and olives since the year 1820.

Extraordinary and intense as was this invasion of cold, it might have been supposed due to local or regional causes only, had it been confined to Cannes and its neighbourhood. In my former paper I stated that I was not informed how far the cold had extended to other countries and latitudes. We still need further exact information on this point, but what has already reached us goes far to prove that the cold was universal, and not limited to a region. In England, Mr. Thomas Plant writes to the *Times* from Moseley, Birmingham:—"After one of the mildest winters registered in the Midland Counties, the month of March, which is generally expected to be the beginning of spring, has been colder this year than any corresponding month for 38 years." "When we consider the power of the sun in March, as compared with December, January, and February, then we can realise some idea of the prolonged and most abnormal cold of the month now ended." By private information I learn that at the same time, in Stockholm, Centigrade's thermometer fell 13°, and at St. Petersburg 18°, below freezing. Unusually intense cold in March is also reported from Canada. In the south we hear of snow and frost in South Italy, Sicily, Algeria, Egypt, and even Nubia. Later still there have been reports of snow on the mountains of Madeira and California, where it had never been seen before.

Since March 19, the date of my former letter, I have been able to make sketches of the sun's position on 49 days.[1] Of these observations the following summary may suffice. In number the spots varied from 3 to 18; the larger showing, more or less, holes or clefts of central umbra, with fringe of penumbra. Faculæ, or clouds of whiteness, were often seen around the larger spots. The spots varied in number and form from day to day; and although the same large spots and even groups could be traced for several successive days, they never retained the same aspect during the whole period of the sun's semi-rotation. On April 17 the spots were at their maximum; in number 18, in three groups. During this period, from March 19 to April 19—thirty-one days—the mean minimum temperature was 46°·2, mean maximum 57°·9.

From April 20 to May 7 there was considerable diminution of the spots; numbers not exceeding 8; and on May 7 there was only one large spot, with surrounding facula. The mean temperature of these seventeen days was—minimum 49°·8, maximum 60.

From May 8 to 16 spots were few, from 2 to 8; but two of them were very large, with umbra and penumbra and sometimes adjoining faculæ. The mean temperature of these nine days was—minimum 52°·7, maximum 63°·8.

Here my observations terminate, as I left Cannes on the 16th, and have no means of observing in London, even if the atmosphere permitted. But I conclude by strongly commending the attentive study of the sun not only to astronomers and physicists, but also to practical meteorologists, as an interesting and not difficult addition to their work of observation, and one likely to supply information concerning the most important factor in the problems of weather and climate. C. J. B. WILLIAMS

47, Upper Brook Street, May 25

[1] I use only a modest achromatic of 32 inches focus, and 2¼ inches aperture, which, projecting the solar image on a white card, exhibits the spots with umbra and penumbra, and the faculæ, sufficiently for this purpose. Of course a more powerful instrument would show a great deal more, both in number and in construction of the spots. For instance, on April 17, when I made out 18 spots, Mr. Campbell's solar image exhibited 104, with marvellous variety in the larger spots, and in the dome-like expansion of the adjoining faculæ. But these details, so deeply interesting in heliography, are not wanted for meteorological purposes.

The Soaring of Birds

MY thanks are due to Mr. R. Courtenay for the notice he has taken (NATURE, vol. xxviii. p. 28) of my letter on the Soaring of Birds (vol. xxvii. p. 592). It is a great satisfaction to me to find my general conclusion supported by his observations. As to the possibility of a soaring bird utilising a downward current of air, I stand corrected. There is no difficulty in agreeing with Mr. Courtenay that the bird, finding itself in a downward current "will descend swiftly so as to acquire the necessary impetus for a rapid escape;"—that is to say, it will seek to make the best of a bad bargain. But it is not so easy to see that the bird, in a current approaching the perpendicular, will "acquire an impetus much more than compensating for the slight loss of elevation;"—that is, will actually make a profit out of a seemingly adverse condition.

This paradox, however, becomes more acceptable by the aid of an illustration:—A marble held lightly just within the rim of a hemispherical bowl, if let drop, will barely reach the opposite rim, but, if struck sharply downward, will run up the opposite side and leap up above the opposite rim. In like manner a bird, struck by a downward current as by a hammer-stroke, may speedily acquire a downward velocity greater than that due (under gravity) to the height through which it has descended; and may therefore rise, if it can escape from the downward current into a horizontal (or *a fortiori* into an upward) current, to a greater height than if it had fallen from the same starting-point through still or horizontally-moving air.

I am very much obliged to Mr. Courtenay for pointing out this interesting result. It gives completeness to the theorem, which now stands thus: that any alternations in the strength or direction of air-currents can be so utilised by birds as to enable them to soar. HUBERT AIRY

Woodbridge, May 25

The Zodiacal Light

THE phenomenon to which your correspondents allude, under the head of zodiacal light, was seen by me in the month of April, 1852. At the time I wrote a letter to the *Times*, in which I suggested it might be caused by the reflection of the sunlight at the surface of two masses of air of different densities, however irregular the bounding surface might be, in the same manner as the line of light seen reflected between the observer

and the sun across the sea. One of your correspondents has suggested a more probable origin, viz. particles of ice in the air. From other correspondents it seems that the sun column is not always vertical, which might be the result of the general flame of the reflecting surface not being parallel with the earth's surface.

In the June number of the *Philosophical Magazine* there was a notice of a sun column as seen at Orkney by the Rev. C. Clouston, who at that period made meteorological observations for that publication. He says that in the month of April of that year the drought was unprecedented, the atmospheric pressure great, and the temperature high. I believe two of these characteristics belonged to the recent month of April if not the third, the high temperature. He says it was seen six times, and once or twice before he noted the date, and also before sunrise.

Saltburn, May 21 E. R. TURNER

Sheet Lightning

MAY not this be an auroral phenomenon, at times, at least, and hence the differences of opinion as to its nature? Reading Wilkes's "Narrative of the U.S. Expedition," I find the following:—"On the 7th February (1840) the weather had become less boisterous, and having reached latitude 49° S., longitude 155°·23 E., the aurora Australis again appeared. It was first seen in the north, and gradually spread its coruscations over the whole heavens; the rays and beams of light radiating from nearly all points of the horizon to the zenith, when their distinctive outlines were lost in a bright glow of light, which was *encircled by successive flashes, resembling those of heat lightning on a summer's night.* These formed a luminous arc in the southern sky, about 20° in altitude, from the upper part of which rays were continually flashing towards the zenith. Light showers of rain finally shut it out from view." FRED. PRATT

Clapton Park, May 25

Pocky Clouds

FOR twenty years I was constantly observing the forms and appearances of the clouds, as clues to the weather and its changes. I observed this form on a very great number of occasions, and from experience always came to the conclusion, "no rain today," and I can only remember two occasions on which the conclusion was not justified. I saw it again a few days ago, with the same result of good weather.

I always termed it the "bubble" cloud till I saw Dr. Clouston's work. It seems to me to be a body of vapour the upper surface of which is being acted upon by an upper current of very dry and rarefied air, causing a great and rapid evaporation, and thence a gradual and unequal cooling and shrinkage of the under surface in the detached globules from which it takes its name. I have seen a very simple illustration while passing through the laundry, and observing a neglected trough of soap-suds cooling down and nucleating in the exact form presented by the pocky cloud, and with the same gradations of tint.

This kind of cloud is generally observable at periods most probable for storms and electric condensations, the which, acting at a distance, would influence outlying areas of upper atmosphere and cause this form of cloud condensation in the way explained. In my observations I have generally found the cloud revert to uniform sheet stratus rather than to disappear in cloudlets in the upper air. FRED. PRATT

Clapton Park, May 25

Clerk Maxwell's "Devil on Two Sticks"

IN the very interesting life of Clerk Maxwell which has lately appeared there are frequent references to a philosophical toy, from which he seemed to derive endless amusement. He calls it the "devil on two sticks." Can you give your readers any account of it? The editors take it for granted that the apparatus is well known, but I cannot find any one here who can tell me what it is. DENNY LANE

72, South Mall, Cork

The Centres of a Triangle

CONTINUING my suggestion in your number of May 3 (p. 7), I propose not only to call the circle circumscribing a triangle the *circumcircle*, but also to call its centre the *circumcentre*, and in the same way to speak of the *incentre*, the three *excentres* (namely, the *a*-excentre, the *b*-excentre, and the *c*-excentre), and the *midcentre*.

The line joining the circumcentre to the orthocentre, on which the masscentre and the midcentre lie, may be appropriately called the *central line* of the triangle.

Similar abbreviations would apply to the radii of these circles; they might be spoken of as the *circumradius*, the *inradius*, the a-*exradius*, the b-*exradius*, the c *exradius*, and the *midradius*.

May 25 W. H. H. H.

THE ROYAL GEOGRAPHICAL SOCIETY

THE annual meeting of the Royal Geographical Society on Monday was of rather more than usual scientific interest. Sir Joseph Hooker was presented with the Royal Medal which the Society has awarded him, Mr. Colborne Baber being the recipient of the Patron's Medal; while among the speakers at the dinner, besides Sir Joseph Hooker, were Mr. Spottiswoode and Prof. Huxley. From the address of the President, Lord Aberdare, it is evident that geographical research, and especially exploration, has been as active as ever during the past year, yet, as the speakers we have named pointed out, the discovery of new countries must have a limit, and in time must come to an end. Still there will be plenty of work for geographers to do in the wider acceptation of the term geography, implied in the presentation of the Royal Medal to so distinguished a botanist as Sir Joseph Hooker. In the words of Mr. Spottiswoode, and as we have frequently pointed out in these pages, geography in its modern acceptation includes "an accurate delineation of the earth's surface, and an exact account of its inhabitants and of their habits, of the animal and vegetable life, and its distribution over the face of the globe." In this direction the Society has a long and brilliant career before it But as Prof. Huxley humorously pointed out in replying for the "other societies," these societies "were all growing a little dull. He did not say this in the way of reproach. The progress made in research and accuracy in methods of procedure involved that consequence. So long as there were large regions of knowledge which the methods of modern science had not penetrated, so long was it possible to go to meetings of societies, and to hold brilliant discussions. Looking at the means which now existed for the diffusion of information, he had been led to think that in many cases where the field of knowledge had been extensively explored the utility of societies was constantly diminishing, and that sooner or later it would be necessary to devise other means of effecting the results now attained by meetings of societies. But there was one thing which would not be reached at any period of time by any other organisation than that of societies, and that was the stimulus which was given by their meetings to investigators; and the reward they found for their toils and sacrifices in such a welcome as had been given that night to his long-tried friend Sir J. Hooker."

The prosperity of the society continues to be maintained. Mr. Clements Markham read the annual report, which showed that during the year the number of Fellows elected was 163, besides three honorary corresponding members, and the total number of Fellows on the list (exclusive of honorary members) was 3392. The total net income for the financial year ending December 31, 1882 (exclusive of balance in hand and 1005*l.* sale of Exchequer Bills) was 7937*l.*, of which 5652*l.* consisted of entrance fees and subscriptions. The net expenditure during the past year was 8779*l.*, including 1135*l.* spent on expeditions. The sale of 1000*l.* of Exchequer Bills was rendered necessary to meet the Society's contribution to the Eira Relief Expedition, but this sum had since been generously presented to the Society by Mr. Leigh Smith. The investments and assets of the Society on December 31, 1882, exclusive of the map collection and library, amounted to 39,831*l.*

THE TRUE ORBIT OF THE AURORAL METEOROID OF NOVEMBER 17, 1882

AFTER many fruitless efforts to conciliate the apparently widely diverging data, given by the numerous observations of this most interesting phenomenon; and after having been many times on the same point as Mr. H. D. Taylor (vol. xxvii. p. 434), who has given the first approximate calculations of this orbit, namely, "to give up the reconciling of such contradictory evidence," I have devoted my Easter holidays to new research on the true orbit. Besides the encouraging letters received from some of the English observers, I found still another motive in the observation of Mr. Julius Dupire at Laon (France, $\beta = 49° 34'$), who had the kindness to give me ample information, for which I offer him my sincere thanks, and in the communication of the following citation, kindly given me by Prof. Ch. Montigny, of Brussels, taken from the *Bulletin de l'Observatoire de Bruxelles*, November 18, 1882: "À 6h. 23m. un énorme rayon d'un blanc vif s'éléva à l'horizon E.N.E.; il traversa le ciel en passant le zénith et alla s'éteindre à l'horizon O.S.O." A similar phenomenon has been observed by Dr. F. Terby at Louvain. The great attraction of the Laon observation consisted in the fact that the meteor's apparent path was there seen at the north side of the zenith, this being in harmony with the Brussels zenith observation, and promising a good determination of the sought orbit.

In the first place I took the following apparent orbits from the numerous given observations. They can or must be taken as great circles, and must, in this case, fulfil the condition of intersecting one another in two opposite points of the sphere. In fact their intersections are contained within a small space and gave me an approximate position to one of these two points, $\alpha = 70° 30'$, $\delta = + 14° 30'$.

These five apparent orbits, the only ones given completely, are the following:—

No.	Place of observation.	Pages of NATURE, vol. xxvii.	Data of observation.	Deduced horizontal direction.	Local time of max. elevation.	Observer.
1	York, $\beta = 53° 58'$.	87, 140, 434	The centre was 6° or 7° below the moon's centre (given not directly after observation); 30° elevation in meridian (probably a mistake or a printer's error, being in contradiction with the other data).	E. 20° N.–W. 20° S. (nearly); deduced by the observer.	6h. 4 or 5m.	H. D. Taylor.
2	Clifton (Bristol), $\beta = 51° 28'$.	85	8° from Saturn, to the right, in a line inclined 45° to the horizon.	E. 20° N.–W. 20° S.	6h. 4m.	A. M. Worthington.
3	Old Windsor, $\beta = 51° 30'$.	87	First seen a little S. of Aldebaran; moves across the moon's disk.	E. 20° N.–W. 20° S.	6h. 6m.	John L. Dobson.
4	Utrecht, $\beta = 52° 5'$.	296	Aldebaran and two points in the equator at 110° and 290° R.A.	E. 20° N.–W. 20° S.	6h. 24m.	Prof. J. A. C. Oudemans.
5	Zonnemaire (near Zierickzee), $\beta = 51° 42'$.	296	Aldebaran and β Pegasi (α Pegasi on p. 296 was a printer's error).	E. 20° N.–W. 20° S.	6h. 21m.	P. Zeeman.

Tracing these five apparent orbits on a celestial globe they gave the intersection point above mentioned. It is clear that this point, joined with the eye of the observer, gives the direction of the true path. This point lying further, the globe being placed on the different latitudes and hours, not far from the point E. 20° N. of the eastern horizon (at Utrecht 7° above the horizon), it is evident that the lines of intersection, formed by the plane of the mean horizon with the planes of the apparent orbits must be nearly parallel to this direction. That these lines of intersection cannot be true parallels follows from the observations of the four students at Cooper's Hill (p. 97), from that of Mr. Joseph Clark at Street, communicated by Mr. J. E. Clark, at York; and from that of Mr. A. S. P. at Cambridge (p. 87), who saw the phenomenon disappear in the S.W., S.W. and S.S.W. Further the *Revue Mensuelle* of M. C. Flammarion (2me Année, p. 72), containing a short report of Mr. Dupire's observation, mentioned above, gives also an observation made at Ploërmel ($\beta = 47° 55'$, λ 2° 23′ W. Greenwich), where the phenomenon disappeared in the west.

Now I have drawn a stereographic map on a large scale, and brought the intersection of the vertical plane through Brussels, with the bearing E. 20° N. It is clear that the true orbit must lie in the vertical plane. Further I have constructed the angles formed by the planes of the apparent orbits with the respective horizons, correcting, if necessary, for the curvature of the earth, and after much trouble found the following path, being a straight line having the properties given in the table on p. 106, that enables us at the same time to compare the results of my construction with the data of the different observations given in order from E. to W.

I hope that the observers will be content with the degree of harmony between their observations and my results. I believe that a small change in the direction of the orbit's plane will give still more harmony between calculation and observation, but the orbit found satisfies the chief observed facts, and gives the greatest divergence, where the observations have the smallest sharpness. I believe I have proved by this research that there existed, with the aurora of November 17, 1882, cosmic dust, passing through the upper strata of our atmosphere with great velocity, and giving, according to the most interesting observation of Mr. Rand Capron (p. 84), "the usual green line" of the aurora spectrum. Thus nature itself has been so kind as to give an experiment that till now, and perhaps for ever, is beyond human

Places of observation.	Height of path in kilos.; meteoroid at max. alt. above horizon.	Max. altitude of the apparent path: Deduced from the found path.	Max. altitude of the apparent path: Deduced from observation.	Comparison of the position with regard to the moon: Constructed.	Comparison of the position with regard to the moon: Observed.	Horizontal bearings of the apparent path: Constructed.	Horizontal bearings of the apparent path: Observed.	Observers.	Remarks.
I. Utrecht.	159·5	54°	54°	—	—	E. 22° N.	E. 20° N.	Prof. J. A. C. Oudemans.	The inclination on the horizon, from observation 6° to 7°, from construction 7°.
II. Zonnemaire (near Zierickzee).	150·6	56	62	—	—	E. 22° N.	E. 20° N.	P. Zeeman.	The altitude 62° uncertain, according to the observer's letter.
III. Brussels.	149·1	90	90	—	—	E. 20° N.	E. 22° 30′ N.	(?)	From the *Bulletin* of the Observatory. The horizontal bearing was given E.N.E.
IV. Laon.	140·9	51½	60 to 65	—	—	W. 17° S.	W.	Jul. Dupire.	The maximum altitude seems to have been determined by estimation.
V. Ipswich.	135·1	—	—	The centre 1° above the moon's centre.	Exactly across the moon.	E. 21° N.	E. 10° N. (?)	Hub. Airy.	The eastern horizon cloudy, the width of the meteoroid 5°.
VI. York.	134·3	—	—	The centre 10° below the moon's centre when nearest.	The centre 6° to 7° below the moon's centre when nearest.	E. 37° N.	E. 20° N. (p. 434) given by the observer.	H. D. Taylor.	The angle below the moon's centre is given from memory (p. 140), and was difficult to estimate. I find the horizontal bearing of the apparent path, given by the observer, E. 25° N. The small inclination to horizon makes it very uncertain.
VII. Cambridge.	133·5	—	—	—	—	W. 51° S.	S.S.W., or W. 67° S.	A. S. P.	It disappeared below the horizon in the S.S.W. (p. 87).
VIII. Greenwich.	132·1	—	—	The centre 5° above the moon's centre.	A little above the moon (p. 83).	About E.N.E., W.S.W.	E.N.E., W.	W. H. M. Christie.	P. 83.
IX. Lincoln's Inn Fields (London).	132	—	—	Across the moon's disk.	Across the face of the moon.	—	—	Edw. Pollock.	P. 141.
X. Cooper's Hill (near Windsor).	130·6	—	—	Nearly above the moon, the centre about 2° above the moon's centre.	Just below the moon.	S. 58° W.	S. 45° W.	Messrs. Sykes, Wildeblood, Thornhill, and Wackrill.	P. 99. Communicated by Mr. Herb. McLeod. The meteoroid being seen from Windsor and from Lincoln's Inn Fields, to the north of Cooper's Hill *before* the moon, it could not *possibly* be *below* the moon, when seen from Cooper's Hill. There *must* be a mistake in the communication or in the observation. The being *before* the moon is naturally a *fact*, where a mistake is *impossible*.
XI. Old Windsor.	130·5	33	30½	Across the face of the moon.	Across the face of the moon.	—	—	John L. Dobson.	P. 87. The apparent path not being very sharply given, the difference in the observed and constructed max. altitude is very small.
XII. Ramsbury (near Hungerford).	127·9	—	—	Across the moon. The centre 2° below the moon's centre.	It seemed to pass directly over the moon.	—	—	Alfred Batson.	P. 100. The given position of the moon (p. 141) seems to be geocentric. I find an altitude of the moon of about 27° at Greenwich.
XIII. Clifton (Bristol).	126·2	28½	25	The centre 6° beneath the moon's lower cusp (measured vertically). The upper boundary of the meteor 4° beneath the lower cusp.	Passing close under the moon, the centre 1½° beneath the moon's lower cusp.	E. 32° N.	E. 20° N.	A. M. Worthington.	The apparent path having but little inclination on the horizon, and its eastern extremity being not without some doubt, the intersection point is very uncertain.
XIV. Street.	126·2	—	—	—	—	S.W. by W.	And went right across the heavens to the S.W.	Joseph Clark.	P. 84. Communicated by Mr. J. Edm. Clark at York.
XV. Clevedon.	126·2	—	—	—	—	W. 32° S.	W. 20° S. (?).	Stephen Saxby and another careful observer (p. 86).	Pp. 86 and 100. The western horizon covered by trees. The altitude in the meridian, given by two observers, is 22°. The true path gives 22½°.
XVI. Ploërmel (Bretagne).	123·9	—	—	—	—	W. by S.	W.	—	"Elle s'avance rapidement vers l'Ouest." Given by the *Revue Mensuelle* of Mr. C. Flammarion.

power, for our means are not sufficient to throw projectiles with several thousand metres velocity; and it is very remarkable that this experiment comes at the same time as the interesting experiment of Prof. Lemström, showing that electric currents are able to give a development of light in our atmosphere, possessing the same number of undulations in a second as the auroral light. Now our meteoroid being a part of an aurora, it gives a stronger proof of the origin of that phenomenon than Prof. Lemström's experiment, the greatest attraction of which is that we are able to repeat it arbitrarily and with our own means. Further, I have always maintained that electricity, excited easily by friction, must be one of the causes of the auroral light ("Théorie Cosmique de l'Aurore Polaire," *Journal des Spectroscopistes Italiens*, 1878, vol. vii. chap. ii.), and it seems to me very plausible that cosmic matter, approaching the earth, induces electric currents through the air. Therefore I think that the results of Prof. Lemström are in full harmony with the idea of a cosmic origin of auroræ.

The orbit found does not reach the surface of the earth, being at its nearest approach still 123·9 kilometres (1 mile = 1609·3 metres; 1 German geogr. mile = 7420·4 metres) or 16·7 geogr miles from that surface. The length of the orbit from the Utrecht perpendicular line to the Utrecht horizon is 1,483,070 metres, and this line being run over in 60 seconds,[1] the mean relative velocity was 24,673 metres, 15·3 miles, or more than 3 German geogr. miles.

The dimensions of the "cosmic cloud" (length 40°, width 5°, as seen from Ipswich) are: length = 182,594, width = 21921 metres. By these dimensions, probably too great from irradiation, it must show at Utrecht an apparent length of 50°; but the extremities were tapered and therefore the length strongly influenced by the transparency of the air. It is therefore not strange that the apparent length at Utrecht was during some few seconds 90 degrees.

To conclude, I will remark that the proved existence of a cosmic cloud, preserving its pretty sharp sides during so long a path as that from Sweden to the Atlantic Ocean, notwithstanding its velocity of 247 kilometres, proves its particles to be nearly spherical. Otherwise these particles should necessarily have diverged sideways from the orbit and spread into space. In connection with the fact observed by Mr. P. Zeeman (p. 297), that auroral clouds gave interference-phenomena, when coming before the moon's disk, and these latter phenomena requiring (Dagnin, "Traité de Phys.," iv. p. 446) the presence of nearly equal particles of dusty matter, Mr. Zeeman's observation proves the same property in the particles of the auroral cloud. Being nearly equal, but not perfectly, the tangential atmospheric resistance must throw the smallest particles backward, and this explains the oblong shape of the cloud.

In presenting my results and reasonings to the readers of this journal, I hope that they will remember that this paper has been written in a shorter time than the author had wished. H. J. H. GRONEMAN

Groningen (Netherlands), April 7

THE AURORA BOREALIS[2]

II.

THE Aurora Borealis at Sodankylä.—Although the aurora borealis often appeared with considerable intensity, it did not boast many varieties. It began generally with a faint arc in the northern sky, which soon developed into a sharp arc, with streamers and a kind of luminous "drapery" spreading from east to west. The colour of this luminous drapery was not very changeable, so that the spectroscope only returned the usual yellow-green line. Generally it was of a yellow-whitish colour, with a slight shade of green.

There was, however, an observation made of far greater interest, viz. that the spectroscopic "reaction,"[1] *i.e.* $\lambda = 5569$, on several occasions was returned from every quarter of the horizontal plane, even from the zenith, *without any aurora being visible.* As this reaction was obtained while the ground was still bare, there can be no question of its being a reflection, but that this place was at the moment within the sphere of an auroral discharge, but of such a weak character that it did not appear in the form of aurora borealis. This observation was therefore precisely similar to the one made in 1871 in Lapland, described above.

These observations were chiefly made by my assistant, Herr Biese, who made another remarkable discovery. Nearly due south-east from the Observatory, he received on several occasions a spectral reaction from a narrow belt of the sky, *although no aurora was visible.* This observation, which was very difficult to effect, as the eye had to be kept entirely away from all light for fully five minutes before the reaction could be traced, I had myself several opportunities of corroborating. In this direction were situated some mountains 300 metres high, about 30 kilometres distant, and in my opinion the reaction was due to the above-described phosphorescent flames, which were seen around the mountain-tops in Lapland and Spitzbergen. All observations were of course made after *every trace* of daylight had disappeared.

The Phosphorescent Luminosity.—On several occasions the attention of travellers in the Arctic regions has been attracted to a peculiar soft light or "shine" during the night. But, as the change from day to night is very gradual in the Polar regions, as compared with that of southern climes, a certain amount of exertion of the mind is required in order to take cognisance and retain the features of this phenomenon. As, however, attention has been once drawn to the same, it will always be observed. Already in October I noticed it at Sodankylä, and directed the attention of my assistants to it. I give subjoined some extracts from my diary concerning this phenomenon:—

December 9, 1882.—The Polar night shows sometimes a peculiar phosphorescent "shine" or diffused luminosity, which possesses several phases, but the general character of which is a luminosity of a yellow-white colour, which renders the night as light as the moon with a thick hazy air. I take here the appearance and disappearance of the light on two nights when its intensity was greatest.

On December 6 I was on a journey between Orajärvi and Sodankylä. The phenomenon became then apparent at 7 o'clock p.m. When daylight had completely disappeared, there seemed to remain a faint light in which the outlines of objects around could only with difficulty be discerned. At 7.40 this increased, however, so rapidly that in a few moments every object around stood out clearly in a yellow-white hazy phosphorescent luminosity of quickly-shifting intensity. I had unfortunately no photometer by me by which I could determine the same. It lasted in this form until about 10 o'clock.

December 8, at 5 p.m., I walked from the Observatory to the church near it, in order to observe from its steeple some fire-signals from Oratunturi. On the way, I noticed that a yellow-white luminosity of shifting intensity filled the entire horizon, while twenty minutes after it had increased greatly in intensity, and was now strongest in the north, whence it gradually faded to the south, where it had least intensity. Near the horizon it was difficult to discern the stars. Higher up it was, however, easier, and from 60° to the zenith the sky was clear, of a mauve colour. It was exceedingly interesting to compare the light with the Milky Way. The yellow-white light

[1] This number is stated also by the sharp determination given by the Astronomer Royal, Prof Christie.

[2] Continued from p. 63.

[1] [By this term Prof. Lemström of course refers to the *characteristic line in the spectrum* of the aurora. The term might be justified by analogy with the "reactions" characteristic of the presence of the various chemical elements.—ED.

contrasted sharply with that of the latter, particularly where the Milky Way stood out of the same. In the yellow-white light it was difficult to make out the Milky Way. This phenomenon lasted far into the night. Later in the evening, between seven and nine, there appeared an aurora of great intensity, of which I shall speak below. This luminosity gave no reaction in the spectroscope at our disposal, but no doubt it would have been obtained had this been less absorbing. Thus, for instance, the larger Wrede's spectroscope (four prisms) did not give the reaction of the auroral phenomenon at Oratunturi, whereas the smaller, as stated above, really showed the line.

There is not the least reason for assuming that this luminosity is of any but an auroral nature, and the result of these observations is *that the whole of northern Lapland is during most winter nights illuminated by a phosphorescent luminosity*, whose intensity varies greatly according to period and place, *but which is undoubtedly of an auroral nature.*

On the same day, viz. December 8, the expedition was enabled to make the first measurement in the magnetic meridian of the elevation of the auroral arc. The wire, which was laid out north and south for the study of the terrestrial current, was used as a t lephone line, and the observations thus made by signals. Two theodolites with the necessary instruments were employed, viz. one at Sodankylä and the other about 4·5 kilometres distant to the north, near the mouth of Kälujoki. The observations at the observato y were made by Herr Biese, and at the northern end of the telephone line by Herr Petrelius. The auroral arc appeared in the north and shone with a quiet, subdued light, while a streamer now and then shot forth into the sky. Six measurements were made with the following result :—At the northern station the line of sight formed, with the under rim of the arc and the horizontal plane, an angle of 9°, and at the southern station one of 12°, *i.e.* an angle 3° larger at the southern than at the northern station ! Even assuming that both observers saw *the same* arc, the result is absurd, as however great the distance between the two might be, the difference of the angle would be very small indeed, and, if a difference at all, the angle of the *northern* station should have been the greatest. As, however, the reverse was the case, I have come to the conclusion *that the two observers did not see the same aurora.* A corroboration of this opinion is that on one occasion Herr Biese telephoned, "Turn the instrument to where the *red* column is," while at the northern station no such colour could be traced. This was proved still further during the return journey from Kultula to Sodankylä by the following circumstance. At Köngäs, 60 kilometres north of Sodankylä, on January 3, 1883, at 4 p.m., the whole horizon was flooded with a yellow-white luminosity of great intensity. At the same time an auroral arc formed in the south about 25° over the horizon, and a similar one was at the same moment observed at the same elevation in the north from Sodankylä. The departure from Köngäs took place just after 4 p.m., and during the journey this arc gradually disappeared, while the luminosity and the arc seen at Sodankylä were seen all through the evening. Here there was an opportunity of measuring the elevation of the auroral arc, but as I was convinced that the two phenomena were not the same, I did not attempt it.

It was clear that we were within an auroral discharge which extended considerably east and west, but the main strike of which was north and south. It is very probable that the electric current which caused this light some thousand metres above the surface of the earth also produced the above-described intense luminosity in a layer some 20 metres in depth, running parallel with the earth. It was this layer which was projected from both points into the sky in the shape of an arc. But it is clear that the auroral "drapery" did not penetrate far into the horizontal plane, but as it is generally produced in the centre of a weak discharge of great penetration its appearance from various places in the line north and south would be very variable according as the layer lends its light to the drapery.

The measurements and results described above exactly correspond with those of Mr. Fritz in Greenland (*Bulletin de la Commission polaire Internationale; Mittheil. der Internationalen Polarcommission*, Heft 3), where he obtained an auroral drapery of 650 feet, 1700 feet distant from the observer, and another one of 170 feet, 350 feet distant.

Without further discussing this question here I must state that I consider that *all* measurements of the height of the aurora, calculated on those with a long base north and south, are always erroneous, as the two observers *never* see the same aurora. And even those calculations which are based on the measurments of the height and length of an arc from *one* point, and the hypothesis that the arc extends around the magnetic pole, must be considered very unreliable, as no satisfactory answer can be given as to what results would have been obtained a little further north or south. This is also the case with auroræ with long bases east and west, as only on a shorter distance is it possible to say if it is the same phenomenon which is seen.

That the height of the aurora borealis is very variable I fully admit, but in my opinion it has been greatly overestimated.

Researches with the Terrestrial Current.—During my expedition to Lapland in 1871, we examined the terrestrial current in two places, viz. Kittilä, lat. 67° 40′, and Enare Vicarage, lat. 68° 55′, with wires 1½ kilometres long—east and west, north and south—of copper 0·4 mm. in diameter, and finishing in platina disks 10 cm. by 5 cm., buried in the earth at a depth of 0·7 to 0·9 metre. The deflexion was measured by a galvanometer with astatic needles with telescope and scale (Weber's magnetometer, of Edlund's improved construction). The remarkable result obtained here was that the galvanometer at Kittilä, with the current east and west, gave a deflexion equal to 60 to 100 parts of the meter scale, whereas the current at Enare only gave *a fraction of one part of the meter.* With the current north and south, the difference was not so great, although even here the deflexions were smaller at Enare. It was unfortunately impossible to ascertain if this remarkable phenomenon was due to *latitude* or *season*, the researches at Kittilä being made in October, and those at Enare in the latter half of November, while on the former occasion the ground was not frozen, which it was on the latter.

The Finnish expedition this year to Sodankylä has also examined the terrestrial current, viz. during certain periods of the phenomenon every five minutes, at other times once every hour, with a wire 5 kilometres long, terminating in small platina disks in the earth. During my visit to Kuttala—Decem er 22 to January 4—I also tested the terrestrial current, but with a wire only 1 kilometre in length, running east and west. Here, too, no deflexion was shown, while in Sodankylä the current was just as strong as ever. At Kuttala the galvanometer was certainly not so sensitive as at Sodankylä, still, the experiments of 1871 are even in this respect not without importance.

I have, therefore, from these researches drawn the inference that, while the condition of the ground is of some influence, *the terrestrial current ceases at a certain latitude.* In 1871 already I maintained that the terrestrial current was caused chiefly by the electricity which descends from the atmosphere in the belt around the Pole, in which the aurora borealis attains its maximum, and my recent researches at Sodankylä have greatly confirmed this theory.

I now intend to discuss the conclusions I have come to from the above detailed researches.

Although the general belief as to the nature of the aurora borealis certainly is that it is of electric origin,

other theories have been advanced, as for instance by Grönemann, *Astr. Nachr.*, 1874-75, and the reason of this is, I believe, that hitherto no direct proof had been obtained demonstrating its true nature.

But the experiments at Luosmavaara in 1871, *and at Oratunturi and Pietarintunturi in* 1882, *clearly and undeniably prove that the aurora borealis is an electric phenomenon.*

The science of the physical conditions of the globe has hitherto, particularly as regards the electric and magnetic ones, simply advanced by observing the effects of these great forces of nature, without however any successful attempt having ever been made to influence or call them forth either directly or indirectly. My experiments now, however, prove that *aurora borealis may be produced in nature* by a simple contrivance assisting the electric current flowing from the atmosphere to the earth. And although the efforts of man must always be limited in comparison with the grand products of nature, the conclusions which may be drawn from the same are not the less instructive.

In a question wherein the theoretical deductions, supported only by a few indirect proofs, have but slowly advanced, *absolute certainty* has now been obtained, and this result should induce future students of the aurora borealis not to devote attention to the "light" phenomenon itself, but to the investigation of those wonderful forces of nature the existence of which it so "lucidly" demonstrates. We have, of course, much to learn from the light also, but far more, I believe, from the electric forces which create it.

It is, however, far from my intention to insist that the apparatus invented by me is the best or that the method followed may not be improved on; still it has certainly one advantage, viz. that of being effective. It is, of course, evident that the drawbacks under which the experiments suffered—as, for instance, weak wires and defective insulators—must be remedied, and it appears to me that the theory which is the basis of M. Mascart's insulator would be particularly suited to the apparatus. The galvanometer should also be altered so as to consist of a *great* number of well insulated coils, in order better to regulate the deflexions, and the experiments should be made in a warm room. As the electrometrical method hitherto used gives only the electric tension at a certain point, it would, it appears to me, form a good meter for measuring the electric state of the surrounding atmosphere. The galvanometer deflexions depend certainly on the electric potential, as well as on the variable conducting power of the air; but it can, as will be seen from my experiments, be measured and even divided by using a constant galvanic element. The electric condition thus measured will give us *an idea* of the strength of the electric current, which in a certain place descends to the earth, and of the electric changes which take place in the atmosphere.

From the experiments with the terrestrial current described above it seems very probable that the current is closely related to the electricity in the auroral belt. The terrestrial current is, as is generally known, related to the magnetic variations, which is most conclusively shown by Mr. Airy's curves (*Phil. Trans.*, vol. cxxxviii. p. 465). In Sodankylä disturbances of the terrestrial current were always followed by a magnetic one. The exact result has of course not yet been calculated, but a glance at the figures returned is sufficient to show this. Mr. Airy's researches have caused these questions: (1) Are the variations in the terrestrial currents more numerous than the corresponding magnetic ones? (2) Do the terrestrial variations occur about half an hour from the corresponding magnetic disturbances?

We have from the experience gained attempted to explain these peculiarities, viz. by the hypothesis that the earth forms, so to say, the core in a flexible bobbin, represented by the terrestrial current circulating around her. In the first place, many of the changes to which the terrestrial current is subject could not affect the magnetic moment of the core, *i.e.* the earth; and, in the second place, the current acts directly on the instruments whereby the magnetic variations are measured; and in these circumstances we must find the explanation of the first-named peculiarity. With regard to the very remarkable difference in time of about *half an hour*, this is the exact time elapsing before the variations of the terrestrial current can affect the magnetic moment of the earth. It is, by the bye, only necessary to compare the duration of induction currents produced in bobbins with different iron cores, to observe that *half an hour* might well pass before the current became perceptible, *if the earth constituted the core.* In Polar regions the electric current descending from the atmosphere to the earth may also contribute to the variations which are measured by our instruments.

In accordance with this theory, therefore, the *electricity* which descends into the auroral belt is the *primary* cause of the greatest part of the terrestrial current, and, through this, of the many variations of the magnetic elements. There are also others, as the diurnal changes in the temperature on the earth's surface, but *the chief* cause is, in my opinion, the electric current from the atmosphere.

In my belief, therefore, the possibility of explaining the peculiarities of this phenomenon lies in a thorough and complete knowledge of the current from the atmosphere.

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(*To be continued.*)

THE FLORA OF ANCIENT EGYPT[1]

THE discovery made by Emil Brugsch Bey on July 6, 1881, in the vault of a king of the twentieth dynasty is of the greatest importance to botany in consequence of the large number of species of plants contained in the offerings and funeral repasts and in the wreaths which adorned the illustrious dead. Among them are several which were not known to belong to ancient Egypt. I have begun the study of the remains of these plants taken from the breasts of the most celebrated kings of Egypt and of such inestimable value to science. Deputed by Mr. Maspero to arrange these relics for the Egyptological Museum of Boulak, I have classified them according to the high personages for whom they were intended. On the eight cardboards which I have the honour to send you in the name of Mr. Maspero, you have a part of the funeral wreaths belonging to Ramses II., Amenhotep I., and Aahmes I.

The wreaths of Ramses II. were renewed towards the end of the twentieth dynasty (1100 or 1200 B.C.), or at the time of the twenty-first dynasty (1000 B.C.). The king of that period, according to records inscribed on the coffins and translated by Mr. Maspero, caused a new coffin to be made for the great Ramses, the one in which he had first been placed having been accidentally destroyed. In this new coffin were several yards of wreaths, which Mr. Maspero handed to me. I have examined them all and ascertained their composition.

The wreaths of Ramses II. are formed of the leaves of *Mimusops Schimperi*, Hochst., either folded or torn in

[1] "Memoir on the Discovery at Deir-el-Bahari in Relation to the Ancient Flora of Egypt," by G. Schweinfurth. [This article, written in French, was communicated to Sir Joseph D. Hooker by Dr. Schweinfurth, together with a set of the wreaths, flowers, &c., described therein. These objects were exhibited at the annual *soirée* of the Royal Society on the 25th ult., and are now on view in No. 3 Museum, Royal Gardens, Kew. With regard to the orthography of the names of the Egyptian kings, that employed by some of the leading Egyptologists of this country has been adopted in this translation. Thus Amenhotep has been substituted for what looks like Amenhotpan in Dr Schweinfurth's manuscript.—W. B HEMSLEY.]

See "La Trouvaille de Deir-el-Bahari." 20 photogr. par M. E Brugsch. Texte par G. Maspero. (Le Caire: chez F. Maurès et Cie., 1881.)

two and stitched together, and serving as clasps for the sepals and petals of *Nymphæa cærulea*, Savi, and *Nymphæa Lotus*, Hook., the whole strung on strips of the leaves of the date palm. Besides the wreaths, there were in the coffin at the side of the body, and fastened between the bands encircling the mummy, whole flowers of *Nymphæa cærulea* on stalks eighteen or twenty inches long. The water-lilies thus scattered separately on the mummy were all of the blue-flowered species. An examination of these entire flowers and the sepals and petals in the wreaths, whether of the white or of the blue-flowered species, leaves no doubt whatever respecting their identity with the living plants so common in ditches at the present day, especially in Lower Egypt, where they blossom from July to November.

The *Nymphæa cærulea*, Savi, which figures on all the ancient monuments of Egypt and among the offerings painted on the walls of the temples is often recognisable from the blue colour of its petals. In the temple of Ramses II. at Abydos the colour is remarkably well preserved, and be-ides there is always a leaf associated with each cluster of flowers, clearly demonstrating by its entire (not toothed) margin that the species represented is *N. cærulea* and not *N. Lotus*. The latter, whose sepals and petals occur abundantly in the wreaths taken from the coffins of Ramses II. and Amenhotep I., has not been found by me on the ancient monuments, though Unger records an instance at Beni Hassan where the white flower could be recognised. With regard to the question to which of the species the old name Lotus properly belongs, I have been able to ascertain the following facts. No design on the ancient monuments is referable to *Nelumbium;* neither the fruits nor the leaves, so easily characterised, are recognisable. Further, no remains of *Nelumbium* have been found either in the coffins or among the offerings and funeral repasts deposited in the vaults of the Pharaohs. The Lotus was not referred to *Nelumbium* until a very much later epoch. This plant has not been found among the wild plants of any part of Africa. It is eminently Asiatic, and was perhaps not introduced into Egypt before the Persian invasion. At the time of Ramadus it was probably cultivated every-

FIG. 1.—Portion of a Funeral Wreath from the tomb of Ramses II. (1000 to 1200 B.C.), composed of the folded leaves of *Mimusops Schimperi* and the petals of *Nymphæa cærulea*, Savi, stitched together with strips of the leaves of the Date Palm. A separate leaf of *Mimusops Schimperi*.

where in Egypt, for we often find it in the mosaics, sculptures, &c., of that period, associated with papyrus and animals characteristic of the Nile, and easily recognised by its fruit.

The most ancient writer who treats of the Egyptian Lotus in such a way as to leave no doubt that he meant the *Nelumbium*, and not a species of *Nymphæa*, is Herodotus (lib. ii. cap. 92); after him Theophrastus ("Hist. Plant." lib. iv.), and then Strabo, while Pliny (lib. xiii.) clearly alludes to a *Nymphæa* in a comparison of the fruit with the capsule of a poppy.

The *Mimusops* was evidently a sacred tree to the ancient Egyptians. The fruits, or the stones of the fruits, which had been eaten, are often found in the funeral repasts in the vaults; and the leaves not only occur in the wreaths of the ancient empire but likewise in those of later times, even down to the Græco-Roman epoch, as specimens in the Leyden Museum testify.

The fruit of *Mimusops* found in Egyptian tombs[1] exactly resembles—except that the stones are a little thicker—that of *M. Kummel*, Bruce, a species spread throughout Abyssinia and the region of the Upper Nile; yet no species of the genus is found wild in Egypt. The leaves forming the wreaths in question should belong to the same species as the fruits found in the tombs. Nevertheless, in comparing them with numerous specimens of *Mimusops Kummel*, I did not meet with the perfect identity one would have expected from the resemblance of the fruits. In Central Africa, and especially in Abyssinia, an allied species, *M. Schimperi*, exists, the leaves of which are much more like those of the wreaths. A longer, and especially a slenderer, weaker petiole, and a more acute, less abruptly acuminate blade characterise these leaves. With regard to the fruit of *M. Schimperi*, I have not had an opportunity of studying it. Moreover the two species under consideration are not sufficiently established as distinct species. But an anatomical character came to my aid. Dr. Westermaier of Berlin has ascertained that the leaves of *Mimusops Schimperi* and of *M. Elengi*, L., have a double layer of epidermal cells, a character they possess in common with the leaves from the ancient tombs; whereas in the leaves of *M. Kummel* there is only a single epidermal layer of cells.

[1] The ancient fruits, however, have usually a thicker stone, the three angles of which appear to be more prominent than in that of *M. Kummel*, Bruce.

Should this distinctive character be constant in the two African species, there is a double reason for naming the ancient *Mimusops M. Schimperi*. The fruit of *M. Elengi* is very distinct from that found in the tombs. I think it very likely that this species, of which we so often find the fruits and leaves in the tombs of the ancient Egyptians, may be the *Persea* of the old authors, which modern botanists have erroneously referred to *Balanites* and *Diospyros mespiliformis*.[1] The latter has not hitherto been found in the ancient tombs; neither does it occur depicted on the monuments. Diodorus (i. p. 34) has transmitted to us a valuable tradition concerning the *Persea*. He states that it was introduced into Egypt with the first colonists coming from Ethiopia, which clearly implies that the ancient authors regarded it as having been introduced from the regions of the Upper Nile and not as belonging to the indigenous flora. *Balanites*, however, grows wild in the valleys of the Eastern Thebaid and on the borders of the Red Sea, and in Nubia this shrub is of general dispersion. True its fruit has been found in the funeral repasts in the tombs, yet that of the *Mimusops* has been found much more frequently, and, in support of my hypothesis, the thick leaves of the *Balanites* are always wanting in the wreaths.

According to Theophrastus, the *Persea* had a black wood, and he compares the flowers with those of the apple-tree. I do not know the wood of the *Mimusops* sufficiently, but with regard to the flowers it must be admitted that no ancient authors ever made a more unmistakable comparison, while the flowers of the *Balanites* have nothing in common with those of the apple. Pliny (lib. xiii. p. 9) does not speak of the *Persea*, but of the *Persica*, and the only surprising thing in it is that he treats it as indigenous in Egypt. He mentions, too, the peculiarity of the Egyptian variety of the peach-tree, which consists in its persistent foliage. Even now in the middle of winter we see the peach-trees in blossom while still carrying their leaves. The same author (lib. xv. p. 13) expressly points out the difference between the *Persica* and the *Persea*. On Egyptian monuments we often see a tree diagrammatically represented, though the distichous, elliptical, acute leaves are evident. This tree, sacred to Hathor or Isis, and often drawn with these divinities, probably represent the *Mimusops* in question. The fruit of *Mimusops Kummel*, of Central Africa, resembles in appearance as well as in taste that of the wild rose; and it may be that under cultivation a still more palatable fruit could be obtained. Indeed, the fruit of specimens of this species collected in Abyssinia appears to be much more pulpy.

All the wreaths of the find at Deir-el-Bahari are of one and the same pattern. The leaves are folded lengthwise in the middle,[1] then folded again in the contrary direction over a string or strip about $\frac{1}{8}$ in. wide, of a leaf of the date-palm. In the fold of each leaf, single flowers, or parts of flowers (sepals and petals), are inserted in

FIG. 2.—Portion of a Funeral Wreath from the tomb of Amenhotep I. (1300 to 1700 B.C.), composed of the folded leaves of *Salix safsaf* and the flower-heads of *Acacia Nilotica* strung together with strips of the leaves of the Date Palm. A separate leaf of the *Salix* (the teeth represented too sharp) and a flower-head of the *Acacia*.

such a manner that they are fixed in the leaf as in a pair of pincers. Then with a finer strip of the date-leaf than the central one, they are stitched through and securely fastened together in long rows side by side, and all pointing in the same direction. These wreaths are arranged in semicircles on the breast of the mummy, so that their disposition is like one sees in the necklaces of the present day. Their thinness rendered them suitable for using in large numbers, and sometimes they occur in several layers one above the other, filling up the limited space between the mummy and the lid of the coffin.

It is probable that it is to this kind of wreath that Pliny alludes (lib. xxi. p. 2) as the "so-called Egyptian wreaths," of which Plutarch and Athenius praised the beauty. Unfortunately these wreaths, which, with ordinary care, might have been removed entire from the mummy when the coffin was first opened, were broken and reduced to powder in several places. The specimens I send you attached to cardboard are the most perfect that I could procure after those selected for the Museum of Boulak. On placing them in boiling or cold water, according to the species, the leaves, &c., recover their original flexibility, especially in *Nymphæa cærulea*; and with proper precaution one succeeds in spreading them out and drying them again effectually. The fragility of these objects is only due to the extreme state of dryness they have reached during the thirty to thirty-five centuries they have lain in the tombs. It is at the same time the principal factor in their wonderful preservation.

The wreaths of the other kings of this vault I have at present only partially examined. From their general appearance, however, as well as from the flowers and leaves of which they are composed, which also indicate a different season[2] of the year, one would be justified in attributing them to a different period from that during which the wreaths of Ramses II. were renewed. If they really date from the time when the bodies of the kings of the eighteenth dynasty were first deposited in the vault, we have here to do with specimens four or five centuries older than the wreaths of Ramses II. In any case these objects are at least contemporaneous with the time commonly assigned to the Trojan war, if not several centuries more ancient.

The wreaths of Amenhotep I. (who was found during

[1] Kunth took the stones of *Mimusops* found by Passalacqua to be this plant.

[It may be mentioned that Kunth published his determinations of the relics found by Passalacqua in the *Annales des Sciences Naturelles*, viii. (1826) p. 418. Unfortunately it is not known to what period they belonged. Among them were seeds of a palm, *Areca* (?) *Passalacquæ*, Kunth, which was subsequently identified by Unger with *Hyphæne Argun*, Mart., a palm which inhabits some of the valleys of the Nubian desert in the bend of the Nile between Korosko and Abou Hammed.—W. B. H.]

[1] Or when they were too large they were torn in two.

[2] The records to which I have alluded indicate the day and the month; and these flowers will one day serve to fix the season with which the month of that epoch coincides. The Carthamus could only be had from the end of March to the middle of May; the Water-lilies from July to November while the young leaves of Salix indicate the spring. The Acacia and Sesbania flower at all seasons.

the twentieth dynasty still intact in his coffin, and who, according to Brugsch, preceded Ramses II. by three centuries) are more varied. Among them are some composed, like those of Ramses II., of the leaves of *Mimusops* and the sepals and petals of the two species of *Nymphæa;* while others are formed of the leaves of *Salix safsaf*, Forsk., which serve as clasps for the little balls of flowers of *Acacia Nilotica*, Del., portions of the heads of flowers of *Carthamus tinctorius*, L., or the separate petals of *Alcea ficifolia*, Cav.

Nobody could recognise either the *Salix* or the *Alcea* among the hundred Egyptian species of plants enumerated by Pliny, or in the writings of other ancient authors; whereas the *Acacia* and the *Carthamus* occur under the names of *Acanthos* and *Cnicus*. Concerning the former, Pliny (lib. xiii. p. 19) mentions the employment of its wood in boat-building, the use of its gum, of its pods in tanning; he speaks of the spines, even, which are found on the leaves; in short he indicates the distinctive feature of the species, adding that the flowers are effective in wreaths. Several of the old authors treat of this tree. With regard to the *Cnicus* or *Knekos* (Pliny, xxi. p. 53) it is only recognisable by the indication that it is spiny, that its large white seeds yield an oil, and that there are in Egypt both wild and cultivated species, which is true. The flowers of *Carthamus* found in the wreaths of Amenhotep I. have retained their red colour, and resemble those of the species cultivated everywhere in Egypt at the present day. The colour, as in recent herbarium specimens, has changed from cadmium red to a brownish red or orange. In water the colouring matter is rapidly excreted, and we behold these flowers of some thirty to thirty-five centuries ago intensely colouring the liquid in the phial containing them.[1] All four of the plants which I have just mentioned have now for the first time been actually found in an ancient Egyptian tomb. The leaves of *Salix safsaf*, which form the greater part of the wreaths of Amenhotep I. and Aahmes I., do not differ in the least from those of the present day, and the species is common in Egypt. They are young—that is to say small and pale—thus indicating an early season of the year. In this respect they are in contradiction with the blue and white petals of *Nymphæa* found in the same coffin, though not, it should be stated, in the same wreaths as the *Salix*, but in the wreaths with leaves of *Mimusops*. The latter very closely resemble those found on the mummy of Ramses II. Perhaps at the time of the removal of the kings of the eighteenth and nineteenth dynasties from one vault to another, and finally to the place of concealment at Deir-el-Bahari, when a new coffin was made for Ramses II.—perhaps, I say, they renewed a part of the wreaths of the other kings, or having ascertained the condition of the mummies (whether under the twentieth or under the twenty-first dynasty), they added some new wreaths to the original ones. This would explain the presence in the same coffin of flowers belonging to different seasons of the year.

Salix safsaf, which occurs in a wild state on the banks of the Nile in Nubia, is in Egypt proper only a riverine fugitive, like many other plants, whose real home is in the south. Away from the river it only exists on sufferance, chiefly near wells and canals. To my mind it is an example of the wild flora which agriculture has caused to disappear. *Alcea ficifolia*, Cav., is now found in Egypt only in the ancient Arabian gardens of Cairo and other towns—that is to say, in gardens dating before the introduction of European horticulture by Barillet in 1869, where it grows almost wild as a weed. I have found it in a wild state in Syria and the Lebanon. Boissier, in his "Flora Orientalis," has not clearly defined it, and gives one or two other forms (*A. lavateræfolia*) as distinct species, which they are not. The petals of the *Alcea* contained in the wreaths of Amenhotep I. leave no doubt that they belong to the species named. Their shape, the distribution of the veins, and especially the hairy callosity on the inner surface of the claw, as well as the size even, confirm the identity of the species. Moreover one perceives in the petals of the ancient wreaths traces of a purplish tint corresponding to the crimson of the living plant. The ancients probably esteemed this plant alike for its beauty and its medicinal properties.

I have examined a head of flowers of *Acacia Nilotica* coming from one of the wreaths, and I found that the flowers agreed in the minutest details with fresh ones, with the characters of which I am sufficiently familiar. The proportions of the peduncle, the position of the annular bract, the shape of the bracteoles, the calyx, the petals, and stamens of each flower do not exhibit the slightest differences. This tree, which is planted or tolerated by man all over Egypt, is nowhere completely wild except on the White Nile between 11° and 12° N. lat., where it constitutes large riverine forests.

The wreaths which were found in the coffin of Aahmes I., the great founder of the eighteenth dynasty (1700 B.C., according to Brugsch), are the most varied, and astonish the eyes with the bright colours they have retained. They are partly composed of leaves of the Egyptian willow (*Salix safsaf*), containing separate flowers of *Delphinium orientale*, Gay, of *Sesbania Ægyptiaca*, Pers., petals of *Alcea ficifolia*, or flower-heads of *Acacia Nilotica;* and partly of the leaves of *Mimusops*, serving as clasps for the petals of the two species of *Nymphæa*, like the wreaths of Ramses II. and Amenhotep I. The *Delphinium* and the *Sesbania* had not hitherto been authenticated from ancient Egypt. The colours of their flowers are admirably preserved, the deep violet of the former being especially striking, but the specimens I have communicated to you in a phial of alcohol have lost their colour, just as fresh flowers of our time would. *Delphinium orientale* is now spread over a very wide area of the Mediterranean region. The two nearest localities to Egypt where it has been found are Algeria and Northern Syria, near Raldoun. It is not impossible that it still occurs in some parts of Egypt, while it is equally possible that it was cultivated by the ancient Egyptians as an ornamental plant. In the event of our being able to prove that some of the wreaths of Aahmes I. and Amenhotep I. were removed at the time of the twentieth dynasty, together with those of Ramses II., we should be justified in the assumption that this plant and *Alcea ficifolia* were introduced through the conquest of Syria. A minute analysis of the flowers, and comparison with those from various localities, leaves no doubt that they are of the species mentioned; and if I had had access to a larger number of flowers of the plant of the present period, I am certain that I should have been able to have exactly matched the ancient ones. The differences that I was able to detect between the ancient flowers and recent ones from Algeria, the Caucasus, Phrygia, and Lycia, kindly supplied by Mr. E. Boissier, may be set forth in a few words. In the first place there are two narrow linear bracteoles exceeding the peduncle in length, and reflexed; then the ovary is less pubescent, and the sepals are narrower and less acute. With regard to the bract, the thickened peduncle, the shape, number, and disposition of the stamens, the stigma, and especially the single petals, I have seen recent flowers in which these organs are absolutely identical. It will be seen that the characters in which they differ are only of individual value. Further, the species in question, commonly cultivated at the present time, comprises a considerable range of forms. Thus there are varieties in which the single petal is merely three-lobed, whilst in others the intermediate lobe is again divided. Both conditions occur in the ancient flowers. These flowers are so well

[1] Unger ("Botanische Streifzüge," p. 113) mentions that a chemist named Thomson had proved that the red dye in the mummy bandages was derived from *Carthamus*.

preserved that under the influence of boiling water the spur of the posterior sepal is easily separated from that of the petal projecting into it. That is to say, the latter may be extracted without injury. The numerous details of the petal, its intricate venation, the coloured glands on the margins, the claw with two lateral folds—all correspond to recent specimens. The colour of the ancient flowers is rather a deep bluish violet than a reddish violet, as in the plant of our time.

I have also carefully analysed the flowers of *Sesbania Ægyptiaca*, from the wreaths of Aahmes I. They belong to the typical form of the shrub, which still springs up on the borders of cultivated fields and on roadsides in Egypt, though it is not really spontaneous below the Soudan. The flowers are so perfectly preserved that the minutest detail did not escape my scrutiny. Submitted to the action of boiling water they scarcely differed from flowers taken from my herbarium. One circumstance shows how hurriedly these funeral wreaths were made. The flower torn from its pedicel and pinched with the finger nails always retains only a part of the calyx cut through the middle.

In the find at Deir-el-Bahari other objects besides the wreaths were found for the first time. Thus in the coffin of the priest Nibsoni, of the twentieth dynasty, the leaves of *Citrullus vulgaris* were scattered between the body of the mummy and the sides of the coffin; and flowers of *Nymphæa cærulea* were found fixed beneath the outer bandages of the same mummy. The Egyptian Museum of Berlin already possessed seeds of this *Citrullus* in the collection of Passalacqua, though the epoch to which the collection belongs is unknown. *Citrullus vulgaris* is found wild in the greater part of Central Africa,[1] and its fruit is smaller than that of the cultivated race, and less palatable, though otherwise like it. Among the broken remains in question I found one whole leaf, which enabled me to fully study its specific characters. Placed in cold water it recovered its original flexibility, so that it could be spread out flat and dried again. The chlorophyll was perfectly preserved, and what was curious, it was absorbed by the water to such a degree, that the glass of water in which the leaf and portions of leaves were placed became of an intense green colour. The problem to solve was whether the leaves were those of the water-melon or those of the colocynth, a species spread over the whole desert region, and only differing from the former, which has long hairs on the young fruit, by the complete nudity and spongy nature of its bitter fruit with a hard rind, and by the seeds. The leaves of the water-melon often very closely resemble those of the colocynth, especially in the variety called *Gjurma* (*Gyurma*) in Egypt, which bears fruit no larger than that of the colocynth, though it is always sweet. Nevertheless the large leaves of elongated outline and having less numerous lobes, are rare in the colocynth, and only in places well watered by rains. There is an association of characters in the leaves from the mummy of Nibsoni, that enable one to refer them to varieties of the cultivated water-melon, rather than to the wild colocynth. I have compared them with a long series of specimens of the water-melon from all parts of the Nilotic region, and with a no less numerous series of specimens of the colocynth; and I have come to the conclusion that they may be regarded as belonging to the former species. The uses of the two species would render them equally admissible in a coffin of ancient Egypt. As a funeral offering an alimentary plant might serve as well as a medicinal one. Still the fact that there are seeds of the water-melon in the Berlin Museum from an ancient tomb supports my first supposition. The leaves found on Nibsoni are about a palm long, and of a pinnatisect form, with obtuse lobes. If these leaves were distinctly hairy there would be no doubt of their belonging to the water-melon. Yet, as already mentioned, there is a variety widely spread in Egypt which has not the long and numerous hairs attached to the tubercles with which the leaves are covered, but merely short bristles, which is also the case in the colocynth.

This variety of water melon, which I have named *colocynthoides*, is the *Gyurma* of the Egyptians, and is cultivated in dry neglected ground in Upper Egypt. It is probably the primitive condition of the species before it had reached its present state of perfection. The leaves of the *Gyurma* are sometimes hairy, as in the water-melon, sometimes only provided with short deciduous bristles, as in the colocynth. The leaves from the coffin of Nibsoni exhibit only the latter condition. It may be that they have lost a great part of these deciduous hairs during the long period that has elapsed. I found one character, however, that the *Gyurma* has in common with those in question. There are on the petiole, and especially on the under surface of the leaf in the middle, among the round tubercles with which it is beset, other tubercles or callosities of an elongated linear form and arranged in rows corresponding to the secondary veins. On these leaves, as well as on those of the *Gyurma*, these elongated tubercles are much more prominent than they are in the colocynth. Moreover the numerous specimens that I have compared of the last have all of them leaves more densely furnished with the round tubercles than is the case with those of the water-melon, of the *Gyurma*, and the ancient leaves.

The secret vault of Deir-el-Bahari, besides the coffins of so many illustrious kings, also contained numerous funeral offerings deposited there by the later kings of the twenty-first dynasty who used this collective tomb, so well concealed by the topographical conditions. Among these offerings I was able to recognise dates, raisins, and pomegranates. There was also a basket filled with a lichen (*Parmelia furfuracea*, Ach.) which at the present day is sold in the bazaar of drugs in every town of Egypt. It is now called "Chêbi" (Sheba), and is used to leaven and flavour the Arabian bread. Medicinally, also, it is in great request. The presence of a lichen of solely Greek origin, mixed with the species named, and which also occurs in the modern drug, excludes all doubt as to its being a commercial product. *Ramalina Græca*, Muell., Arg., which was mixed with the *Parmelia*, has only been found in the islands of the Greek Archipelago, and the Arab merchants regard that country as the source of their drug. As there is no locality in Egypt where *Parmelia furfuracea* could grow, the only explanation of its presence in the offerings of the twenty-first dynasty (1000 B.C.) is that it was derived from Abyssinia or Greece. In the latter case the find at Deir-el-Bahari would prove the existence of commercial intercourse with Greece at about the time of the Trojan war. Among the *Parmelia* (which was perhaps the *Sphagnos* of Pliny) were fragments of *Usnea plicata*,[1] Hoffmg, and the straw of a grass (*Gymnanthelis lonigera*, Anders.) of Nubia, which at the present day is used by the natives as a remedy against affections of the chest and stomach. On searching through the copious remains of this plant I succeeded in finding a few well-preserved flower-spikes, which I carefully examined and determined beyond doubt to belong to the species mentioned. In Arabic it is called "*máhareb*." The odour even of this grass was preserved to a certain extent in the mixture of the offering. The fragrant secretion is of the same nature as that of the allied section *Schœnanthus* of *Andropogon* of India. Besides the lichens and the grass, this offering contained the hairy buds of some *Compositæ*, probably an *Artemisia*, with pinnatisect leaves; tendrils of some *Cucurbitaceæ*; seeds of the coriander; and numerous berries and seeds of the eastern Juniper (*Juniperus Phœnecia*). Inasmuch as we have here to do with plants coming from opposite regions of Africa and from Europe or Asia, it was

[1] I have gathered it in that state in the islands of the White Nile.

[1] Dr. J. Mueller of Geneva

not an easy matter to pronounce an opinion on the *Cucurbitaceæ* and the *Compositæ* mentioned. The coriander is a plant of early cultivation in Egypt, being mentioned by Pliny as one of the best products of the country. The berries and seeds of the juniper (the latter free in consequence of the decomposition of the former) could only have been derived from Syria or the Greek Islands. I carefully compared them with the allied species, including the Abyssinian *Juniperus excelsa* (which has larger berries and much thicker seeds, to the number of six), and there can be no doubt that they belong to *J. Phœnicea*, L. Kunth had previously determined this species in the collection of Passalacqua.

Among the fragments of the offerings and repasts found scattered on the floor of the vault of Deir-el-Bahari when it was first inspected by Brugsch Bey (some of the objects had already been disturbed by Arab robbers) was a tuber of *Cyperus esculentus*, L., some specimens of which from ancient Egypt are also preserved in the Berlin Museum. It is common in a wild state, and generally cultivated in the country.

In bringing this enumeration to a close I have only to mention the finding of a bundle of the grass called *Halfa* by the Egyptians (not the *Halfa* of Tripoli and Algeria), *Septochloa bipinnata*, Hochst., syn. *Eragrostis cynosuroides*, Retz. This bundle probably formed part of an offering representing the productions of the black and fertile soil of the valley of the Nile, of which this grass was a good sample.

ON THE CHEMICAL CHARACTERS OF THE VENOM OF SERPENTS

DRS. WEIR MITCHELL and E. T. Reichart, of Philadelphia, are now engaged in an inquiry into the chemical composition and characters of snake poison, which promises to yield important results and to supply information long wanted on an aspect of the subject which has made little progress since Prince Louis Lucien Bonaparte published his discovery of an active principle in viper venom, which he considered to be the sole cause of its toxic properties, and to which he gave the name of Echidnine or Viperine. He described the mode of separation of this principle in a paper read before the "Unione degli Scienziate Italiani" at Lucca in the year 1843.

The investigations of Drs. W. Mitchell and Reichart relate chiefly to crotaline snake poison, but include a partial analysis of some dried cobra (colubrine) poison sent to them by Mr. V. Richards from India.

Difference in the mode of action of the colubrine and viperine virus was pointed out by me many years ago in India, when I observed that viperine poison destroys the coagulability of the blood in animals, causes hæmorrhage, and has peculiar effects on the nervous system differing from the cobra's (colubrine) venom, which does not destroy the coagulability of the blood, nor cause so much hæmorrhage.

Dr. Wall of the Bengal Medical Service has added much to our information on the subject, and has defined the different modes of action of the venom of the principal Indian poisonous snakes.

The Philadelphia observers came to the conclusion that the venom of the crotaline snakes with which they have chiefly operated can be subjected to the action of the boiling temperature of water without completely losing its poisonous power. The toxicity of the venom, however, of the *Crotalus adamanteus* seems to be destroyed by a temperature below 176° F. Mitchell some years ago showed that the venom of *Crotalus durissus* is not destroyed by boiling, and they remark on the curious fact that the venom of *C. adamanteus* should thus differ from the venom of other snakes.

The symptoms caused by the venom of the different snakes with which they have operated do not, they say, differ radically save in degree, but there are certain symptoms which they think make it probable that further investigation will enable them to point out certain differences by which it will be possible to discriminate one form of poisoning from the other. This is in accordance with what has already been done by observers in India, and notably by Dr. Wall.

The investigations of Drs. Weir Mitchell and Reichart so far, lead them to conclude that the poison of the cobra is the most active, next the copperhead, then the moccasin, and lastly the rattlesnake; but their researches on this head are not yet complete.

They are unable to confirm the statement of Gautier of Paris that an alkaloid resembling a ptomaine exists in cobra poison; or that of Prof. Wolcott Gibbs, that the poison of crotalus yields an alkaloid; but they have satisfied themselves that the venom contains three distinct proteid bodies, two of which are soluble in distilled water, one which is not soluble. These bodies have certain properties and reactions, which are detailed in their monograph on the subject.

Hitherto observers have regarded the venom of different snakes as each representing a single poison, but it appears from these researches that, of the three proteids before mentioned, one is analogous to peptone and is a putrefacient poison, another is allied to globulin, and is a most fatal poison, probably attacking the respiratory centres and destroying the power of the blood to clot, while the third resembles albumen, and is probably innocuous. The separation of the two poisons necessitates a long and elaborate series of researches, the results of which will be subsequently reported.

They have also ascertained that the poison of the Rattlesnake (*Crotalus adamanteus*), Copperhead (*Trigonocephalus contortrix*), and Moccasin (*Toxicophis piscivorus*), are destroyed by bromine, iodine, hydrobromic acid (33 per cent.), sodium hydrate, and potassium permanganate. It is to be hoped that these important and valuable researches will be continued until the true chemical nature of these poisons be completely made known.

J. FAYRER

NOTES

AT a meeting of the subscribers to the Balfour Memorial Fund, held at Cambridge on the 26th inst., it was stated that 8309*l.* had been promised, all except 100*l.* of which had been paid. Of this 8078*l.* had been invested, yielding an annual income of 284*l.* 10*s.*, which it was hoped further subscriptions would raise to 300*l.* Among the regulations agreed to were the following:—The income of the fund shall be applied (1) to endow a Studentship the holder of which shall devote himself to original research in biology, especially animal morphology; (2) to further by occasional grants of money, original research in the same subject. The Student shall not necessarily be a member of the University, and during his tenure of the Studentship shall devote himself to original biological inquiry, and shall not systematically follow any business or profession or engage in any educational or other work which in the opinion of those charged with the administration of the fund would interfere with his original inquiries. The place and nature of the studies of the Student shall be subject to the approval of the managers provided that the Student shall be bound to pursue his studies within the University during at least three terms during his tenure of the Studentship, unless the managers shall, with the approval of the Board, dispense with this requirement for special reasons. The managers shall take such steps as they may think necessary to satisfy themselves as to the diligence and progress of the Student, and may require from him any reports or other information on the subject of his studies which they may think desirable. The Studentship

shall be tenable for three years, but it may be continued over a second term of three years (but no longer) to the same person if the managers and Board decide that it would be clearly in the interests of biological research. The balance of the income of the fund, after providing for the Studentship and for any necessary expenses connected with the election, shall be devoted to the furtherance of original research in biology, especially animal morphology. Grants may be made for this purpose either to the holder of the Balfour Studentship or to any other person engaged in research.

THE subscription list for the memorial bust of Prof. Henry Smith, to be placed in the University Museum, will be closed at the end of the present term. It would be convenient if subscribers would, as soon as possible, pay their subscriptions into the Old Bank, or send cheques to any of the following gentlemen :—Mr. W. Little, Queen Anne's Mansions, S.W.; Mr. R. L. Nettleship, Balliol College, Oxford; or Mr. E. Chapman, Frewen Hall, Oxford.

DR. JULIUS VON HAAST has been created a Companion of the Order of St. Michael and St. George.

TWO statues which have been erected in front of the Berlin University to the Brothers Alexander and Wilhelm von Humboldt were unveiled on Monday with great ceremony. The Emperor and some of the members of the Imperial family witnessed the proceedings from the Royal Palace, which immediately faces the University, and the Emperor afterwards went on foot to inspect the statues.

DR. GABRIEL GUSTAV VALENTIN, one of the most eminent professors of the University of Berne, and a distinguished physician, died at that city on May 24. Dr. Valentin was born at Breslau in 1810, graduated in 1832, and began practice in his native town in the following year. In 1835 he published a handbook of the history of evolution ("Entwicklungsgeschichte"), and in 1836 was appointed Professor of Physiology in the University of Berne, a position which he held until 1881, when ill health compelled him to resign. He stood very high in his profession, and was the author of many scientific works, two of which were written in Latin, "De phænomeno generali et fundamentali motus vibratorii continui" and "De functionibus nervorum cerebralium et nervi sympathici libri quatuor." He wrote also a "Text-Book of Physiology," a book entitled "Groundwork of Human Physiology," a "Repertory of Anatomy and Physiology," an "Examination of the Effects of Polarised Light on the Life of Plants," an elaborate work on the "Adaptation of the Spectroscope to Physiological and Medicinal Purposes," and several others which attest his vast knowledge and untiring industry.

THE following remarks by our American contemporary, *Science*, on the subject of the Canadian meeting of the British Association in 1884, are deserving of attention :—"It is to be observed that in the present year the meeting of the American Association, at Minneapolis, is early (August 17); while that of the British Association, at Southport, which is, besides, in the immediate vicinity of Liverpool, is unusually late (September 19). This will allow members of the American Association to attend both meetings, and it is stated that the retiring President of the American Association, and possibly others of its members, may avail themselves of this privilege. This may possibly permit arrangements to be made which might substantially unite the meetings of the two Associations in 1884, and so prepare for an international meeting in the future. If the meeting of the American Association for 1884 can be fixed for some northeastern city, sufficiently near to Montreal, and can be timed so as to occur a week before or after that of the British Association, there can be no doubt that a great number of the members of the latter body would take advantage of the opportunity to enjoy the companionship of their American *confrères*, while, on the other hand, many of these would gladly spend a few days at the meeting of the British Association. In this way it would seem that a greater benefit to science might result than even from an international meeting. There would be time for the complete transaction of the business of both Associations. Neither would suffer, either pecuniarily or in the value of its proceedings; and there would be the best possible opportunity for interchange of ideas between the scientific men of the United States, Great Britain, and Canada. Nor is it unlikely that some scientific workers from the continent of Europe and elsewhere may be attracted by a combination so unusual. It may thus be hoped that the proposed meeting of the British Association in Canada may not only be one of the most successful that this mother of Associations has held, but may inaugurate an epoch of renewed activity and progress in the widely-spread scientific work of the two great Associations of the English-speaking race."

THE New Parkes Museum of Hygiene at 74*a*, Margaret Street, Regent Street, was opened on Saturday under favourable and distinguished auspices. The Duke of Albany presided and formally opened the Museum, and gave besides a sensible and thoughtful speech. "Hygiene," His Royal Highness said, "as we now understand it, is a branch of knowledge of modern growth. It is one of the natural results of the great advance of science which this century has witnessed, and might, I fancy, not inaptly be defined as the application of scientific principles to the varying conditions under which we are called upon to live. Thanks to the labours of many eminent men, we have now advanced some way towards an accurate knowledge of the conditions which are necessary for health; and most of these conditions have long been familiar to the few. One object of the Parkes Museum will be to make them familiar to the many. We have learned, and are daily learning, that many of the luxuries and conveniences of modern life may become sources of danger to us if they be ignorantly used. London would be almost uninhabitable were it not for its wonderful system of sewers; but while enjoying the blessing of effective sewerage, we have had to encounter the difficulty of keeping the air of the sewers out of our dwellings. We all appreciate the brilliant light which is given by a gas lamp; but its wholesome use, we are now beginning to find, involves questions of ventilation which scarcely troubled those who were content with the comparative dimness of a candle. Again, the open coal fire has long been regarded as one of the chief luxuries of the Briton, but the collected smoke of the fires of 4,000,000 of people has become a nuisance too grievous to be borne, and one for which a remedy must be sought. It is notorious that many of our public and private buildings in this country have been constructed without due attention, or, indeed, any attention, to those details which alone make a dwelling wholesome. The experiences of my own family in this matter have indeed been singularly hard. We hope that this museum will tend to hasten the end of this state of things, and that henceforward 'healthiness' will be considered as an essential condition of true architectural beauty. For the healthiness of our dwellings we have to depend, not only upon the master mind which furnishes the plan, but even to a greater extent upon the intelligent hands of those who are called upon to carry out the details. Unless the work of these latter be done with intelligence and faithful honesty, the schemes of the wisest architect avail us little. The instruction which has been and will be given here to the artisans who carry out the sanitary details of our houses must be productive of good results. At least, let us hope that some of the specimens of defective workmanship to be found upon our shelves will impress upon them that death, disease, and sorrow may be the results of ignorance or carelessness on their part." Among the other speakers

were Sir Charles Dilke, Prof. Tyndall, and the Archbishop of York. It is to be hoped that the public, and especially those on whose skill and honesty our sanitary arrangements are dependent, will take ample advantage of the opportunities offered by the new museum.

THE seventh Congress of Russian Naturalists and Physicians will be held this year at Odessa, from August 30 to Sept. 9.

THE district of Pergamos in Asia Minor is now so infested with sparrows that application has been made to the Turkish Government for aid against them. It will be remembered that this district is subject to occasional invasions of rodents.

THE Marine Excursion Committee of the Birmingham Natural History and Microscopical Society announce that, in response to a wish expressed by many members, they have arranged a second excursion to Oban and the West Highlands of Scotland, similar to that which proved so successful in the year 1881. The party will leave on Friday, June 29 next, to reach Oban about 5 p.m. on Saturday. The screw steam yacht *Aerolite*, of about sixty tons, has been hired of Messrs. Ross and Marshall of Greenock for a week, commencing Monday, July 2; facilities will thus be afforded for dredging excursions not only in the district previously worked, but also in distant localities. Arrangements are being made for excursions to several places of interest in the neighbourhood of Oban.

THE sixth annual meeting and *conversazione* of the Midland Union of Natural History Societies will be held at Tamworth on June 12 next. Excursions have been arranged for that day and the 13th. The Darwin Gold Medal for 1882 will be presented to Prof. A. M. Marshall and W. P. Marshall, for their paper on the Pennatulida.

THE additions to the Zoological Society's Gardens during the past week include a Malbrouck Monkey (*Cercopithecus cynosurus*) from West Africa, presented by Mr C. D. Gordon; two Grisons (*Galictis vittata*) from South America, presented by Mr. Percy Kenyon Slaney; two Sloth Bears (*Melursus labiatus*) from India, presented by Mr. F. A. Curteis; a Surucucu or Bushmaster (*Lachesis mutus*) from Pernambuco, presented by Mr. J. Y. Barkley; a Common Chameleon (*Chamæleon vulgaris*) from North Africa, presented by Mr. Henry W. Weguelin; a Chimpanzee (*Anthropopithecus troglodytes* ♂) from West Africa, two Welsh Sheep (*Ovis aries*) from Wales, a Goffin's Cockatoo (*Cacatua goffini*) from Queensland, five Margined Tortoises (*Testudo marginatus*), thirteen European Pond Tortoises (*Emys europæa*), South European, deposited; a Common Seal (*Phoca vitulina*) from British Seas, a Grey-headed Porphyrio (*Porphyrio poliocephalus*), a Conical Worm Snake (*Gongylophis conicus*) from India, purchased; a Hybrid Tapir, ♀ (bred between *Tapirus roulini* ♂ and *Tapirus americanus* ♀), born in the Gardens.

OUR ASTRONOMICAL COLUMN

THE MINOR PLANET, ANDROMACHE.—Among the small planets mentioned in the last volume of the *Berliner Astronomisches Jahrbuch* as having been observed at one opposition only, though several oppositions have taken place since their discovery, is No. 175, detected by the late Prof. Watson of Ann Arbor, U.S., on October 1, 1877, and named *Andromache*. The orbit has a considerable eccentricity, and the planet recedes to a greater distance from the sun at aphelion than is the case with any other member of this now numerous group so far calculated; indeed at this point of its orbit it is distant from the sun 4·723 (the earth's mean distance being taken as unity), and only 0·594 from the orbit of Jupiter. There should be no great difficulty in recovering this planet during the month of June or July. According to the most accurate elements calculated by Prof. Watson it will be in perihelion about July 25, and in opposition a fortnight earlier, its computed intensity of light being equal to that of a star of fully the ninth magnitude. Its considerable south declination will give an advantage to a search at one of the observatories of southern Europe. To facilitate its reobservation we subjoin positions deduced from the orbit last published:—

At Greenwich Midnight

	R.A. h. m.		Decl. ° ′		Log. Distance from Earth.		Sun.
June 4	... 19 40·3	...	−27 23	...	0·1547	...	0·3635
12	... 19 39·0	...	27 44	...	0·1368	...	0·3622
20	... 19 36·0	...	28 7	...	0·1222	...	0·3610
28	... 19 31·5	...	28 29	...	0·1117	...	0·3600
July 6	... 19 25·9	...	28 48	...	0·1059	...	0·3594
14	... 19 19·9	...	−29 1	...	0·1055	...	0·3590

The planet will probably be situated at some distance in R.A. from these positions, which are only intended as an approximate indication of its places. The last reference to a search for it which we find in the circulars of the *Berliner Jahrbuch*, occurs in No. 118 (Correspondenz), 1881, March 3, where we read, "*Andromache* innerhalb −6m. 30s. bis −3m. 55s., und −2m. 20s. bis +4m. 15s. vergeblich gesucht." A special rough chart of stars in the vicinity to the tenth magnitude inclusive would be readily formed with the stars in the Bonn and Washington Zones as reference points.

THE GREAT COMET OF 1882.—M. W. Fabritius of Kieff has calculated the following elliptical elements of this comet from two normal positions for September 9 and October 6, and an observation at Königsberg on March 3 in the present year:—

Perihelion passage, 1882, September 17·2753 M.T. at Berlin.

Longitude of perihelion	276° 28′ 40″·1	M. Eq. 1882·0
,, ascending node	345 58 4·1	
Inclination	38 0 44·7	
Log. $(1-e)$	5·938209	
Log. semi-axis major	1·943548	
Log. perihelion distance	7·881757	

Motion—retrograde.

The corresponding period of revolution is a little less than 823 years, and as M. Fabritius attaches some weight to his result, he thinks the comet must have appeared about the middle of the eleventh century.

We shall doubtless have in due course a thorough discussion of all reliable observations; those made since September 30, when the disintegration of the nucleus commenced, will need special treatment.

THE OBLIQUITY OF THE ECLIPTIC.—In NATURE, vol. xxvii. p. 618, we quoted 23° 41′·1 as the value of the obliquity of the ecliptic at the assigned epoch of Ptolemy's catalogue. With reference to this statement Mr. W. J. Cockburn Muir, of Melrose, N.B., has made a discovery, on which he writes us as follows:—"In NATURE of April 26, at p. 618, I read that the 'obliquity of the Ecliptic' is 23° 41′·1, and I wondered much what had suddenly happened in the Kosmos. So I took means to ascertain from the Royal Observatory of Greenwich how the record stands, and I am comforted to find that, by the determinations in 1882, the earth's axis still remains at home—23° 27′ 16″·8." Our correspondent may be referred to any elementary treatise on astronomy.

GEOGRAPHICAL NOTES

MR. OSCAR DICKSON's Greenland Expedition, under the command of Baron Nordenskjöld, sailed from Gothenburg in the *Sofia*, 180 tons, 65 horse-power, drawing 10 feet, and of 11 knots speed, navigated by Capt. Nilsson and a crew of 13 men. With Baron Nordenskjöld are Dr. Nathorst, geologist; Dr. Berlin, doctor and botanist; Dr. Forsstrand, zoologist; Dr. Hamberg, hydrographer; Herr Kolthoff, zoologist; Herr Kjellström, typographer and photographer; two Laplanders, two Norwegian icemasters, and one harpooner. There is on board a complete scientific equipment and 14 months' provisions for subsistence on the inland ice. Eight or nine picked men accompany Baron Nordenskjöld. Count Stromfeldt, botanist; Dr. Arpi, archæologist and philologist; and Herr Flink, mineralogist, will disembark on the coast of Iceland for the purposes of

study and collection. The *Sofia* called at Thurso for coal on Sanday and left on Tuesday.

In connection with Prof. Fries' suggestion of colonising Greenland by mountain Lapps, to which we referred last week, we learn that Baron Nordenskjöld takes with him to Greenland two Lapps from Jockmock, to give their opinion of the country. One of them is thirty, and the other thirty-three years of age.

We learn from the last annual report of the East Siberian branch of the Russian Geographical Society that this Society, which has contributed so largely to the increase of our knowledge of Siberia, is beginning to recover from the losses it sustained during the great fire at Irkutsk. Private subscriptions have been raised for the reconstitution of the library and museum to the amount of 2170*l.*, and both are in a fair way of development. The library already has about 4000 volumes, but is in great want of foreign geographical publications, and makes an appeal to the geographical societies throughout the world to send their publications and, if possible, series of former publications, which ought to be addressed to the Secretary of the East Siberian branch at Irkutsk. The chief occupations of the Society were: the geological exploration east of Lake Baikal, by M. Chersky, who has already published a map of the western coast of the lake; archæological researches as to the prehistoric inhabitants of Siberia, by MM. Agapitoff, Khangaloff, Witkovsky, and Bogolubskiy; and the part it took in the organisation of the Arctic Meteorological Station at the mouth of the Lena, and of a series of four intermediate stations between Irkutsk and this station. This last scheme could not be realised in full, but two stations have already been opened at Verkholensk and at Preobrajenskoye. The last number of the *Journal* of the Society contains, besides the annual report and the proceedings, a list of new determinations of latitudes and longitudes in Transbaikalia; a notice on Shamanism with Yakuts; a paper on the populations of the basin of the Amur, according to Prof. Schrenck; a paper on the inscriptions on stones and rocks in the district of Minusinsk; and several notes, on the Lena Meteorological Station, on the Usuri region, &c.

Petermann's Mittheilungen for May contains a paper by Mr. Carl Bock describing a journey recently made by him from Bankok to the frontiers of the independent Shan States. He travelled along the Menam River in a boat given him by the Siamese Government, as far as Raheng, where he diverged into the Me Ping. He then proceeded partly by the river, partly by land through Lakon and Lampun, to a town which he calls Tschengmai, but which is more generally known as Kiangmai, or Zimmé. This place, which is the capital of the Shan States tributary to Siam, is an important point in Mr. Colquhoun's proposed railway from Rangoon and Moulmein, into south-western China. It formed the proposed terminus, too, of that gentleman's recent journey through Yunnan and the Shan States. Mr. Bock described it as a fortified town of about 700,000 people, lying in a fertile plain of uninterrupted rice fields, about 500 yards from the Me Ping, which is here 400 feet wide. Even now it is of great political and commercial importance, as it controls the trade of these regions both with Siam and with British Burmah. The teak forests of the States he describes as almost inexhaustible, especially higher up near the Meikong, where, however, it is not yet known whether the lumber can be easily floated down to the sea. For this purpose Mr. Bock recommends a careful survey of the various rivers and their tributaries. From Zimme he continued his way higher up to Kiangtsen, in the valley of the Meikong, and on the borders of the independent Shan States. It was his original intention to travel through these States into Yunnan, as it was Mr. Colquhoun's to travel through them from Yunnan, southwards. Failing this, he returned to the Me Ping, with the object of tracing this river to its source. He was prevented from carrying out either project by the native hostility, which, we regret to say, Mr. Bock himself did much to intensify, if not arouse, by his indiscreet behaviour. It would be inconceivable, if we did not have it on his own testimony, that any traveller among a people who, as he was specially warned, disliked even the Siamese, and absolutely hated any white man, should so far forget all discretion as to enter a populous town and "out of his own hand," as he describes it, take possession of the court of justice, and assault with a stick the official who endeavoured to prevent this unjustifiable trespass. He was punished by several days' imprisonment, but it is unfortunate for the cause of science that the hostility thus carelessly and wilfully aroused should have put a speedy termination to a journey full of promise. Mr. Bock, however, has shown beyond doubt that a railway from Bankok to the Shan frontiers is a possibility. It would pass through populous and rich districts in the valleys of the Menam and Me Ping. He says that no one who has not visited Zimmé can understand how extensive the trade of the place is, and his proposed railway would place the Laos States in direct communication with the sea, and attract the commerce not only of the Shan States, but also of Yunnan. These are exactly the arguments by which Mr. Colquhoun supports his scheme for a railway to Rangoon. Let us hope that in days to come, when this colossal project is an accomplished fact, there may be no dispute as to the originator of the idea of attracting the trade of south-western China to the sea by means of a railway through the Shan States.

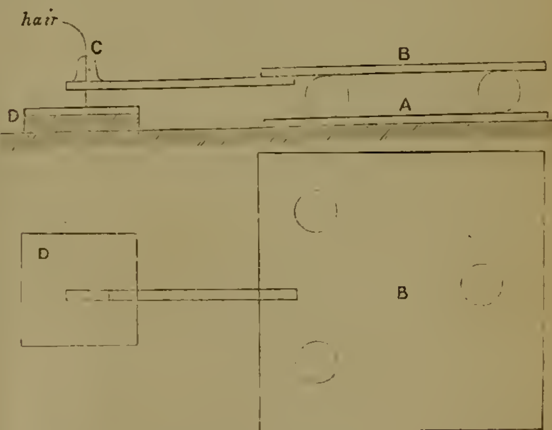
A NEW FORM OF SEISMOGRAPH[1]

Numerous forms of seismometers have from time to time been invented, and having these various instruments, it may be asked why there is any necessity for a new form, and I can best answer this by quoting from a report of a committee of the British Association of 1872, as follows:—"Some simple and cheap method of indicating earthquake movement is thus much to be desired—any apparatus for the purpose should occupy small space, be little liable to derangement, capable of being put up in any apartment not of special construction, and its indications such as any intelligent person could easily interpret and readily note."

Now none of the instruments yet invented fulfil these conditions, and hence I bring before you one which is of the very simplest nature.

The idea of the instrument I propose was suggested to me by the aseismatic arrangement designed by my father, Mr. David Stevenson, for averting damage to buildings and lighthouse apparatus in countries subject to earthquakes (*Trans. Roy. Scot. Soc. Arts.* vol. vii.).

The instrument is shown below, and consists of a ground and polished glass plate (A), about 5 inches square, placed level (once for all), on which rest three accurately turned ivory balls about $1\frac{1}{2}$ inch diameter, and on the top of these



balls is placed a plate (B) similar to the lower, but having attached to it a projecting arm with a long vertical hole pierced through it. Through this hole passes a steel needle (C) with a fine point, which rests by its own weight on a lampblack surface formed on the plate D. A hair about 2 inches long should be fixed to the eye of the needle to assist in adjusting it. The instrument thus becomes a pendulum of infinite length, so that whenever there is any movement of the ground, and therefore of the lower plates, the top plate with its arm and needle attached remain practically steady, and the point of the needle therefore marks on the lampblack surface the amount of motion and the direction in which the lower plate is moved. This instrument, it will be observed, fulfils all the requirements mentioned in the report of the committee of the British Associa-

[1] Abstract of paper read before the Royal Scottish Society of Arts, February 13, 1882, by Charles A. Stevenson, C.E., Edinburgh.

tion, and can be made more or less sensitive. It is impossible from a mere description to form any conception of the efficiency of the apparatus, nor has it been tried by any earthquake, but the instrument before you having been erected on the gable of a dwelling house during the past year, repeatedly registered the shaking of the gable to the amount of 1-16th of an inch.

UNIVERSITY AND EDUCATIONAL INTELLIGENCE

CAMBRIDGE.—The Museums and Lecture Rooms Syndicate have just issued their annual report, in which they mention the high value of the present of the late Prof. Balfour's scientific instruments and library to the University by his family, and again emphasise the necessity existing for a new chemical laboratory. Mr. Clark records the mounting of the fine male Indian elephant's skeleton in the Zoological Museum, received in an exceedingly complete state last year in consequence of its careful preparation by Mr. A. Haly of the Colombo Museum. The animal was shot by Mr. Le Mesurier of the Ceylon Civil Service; its height was nine feet. A specially interesting skeleton of the adult Gangetic Dolphin has been presented by Sir J. Fayrer. Mr. A. P. Maudslay, M.A., of Trinity Hall, has deposited in Mr. Clark's care a large portion of his ethnological collection made in Fiji and adjacent islands; these are almost certain to be presented to the University at no distant date. The Curator in Zoology (Mr. A. H. Cooke) has catalogued and arranged the British species in the MacAndrew collection. Its completeness may be judged by the facts that of 6 recorded species of Brachiopoda this contains 5; of 159 marine Conchifera this contains 146; of 248 marine Gasteropoda this contains 208; of 125 land and freshwater shells this contains 114 A recent appeal to add missing species has already resulted in the presentation of fourteen species by Mr. J. T. Marshall.

Dr. Michael Foster reports an average class of about 100 in Elementary Physiology, and of over 20 in advanced Physiology, in the three terms of the past year. Additional lecture room accommodation is much needed for these large classes.

The morphological work begun by the late Prof. Balfour has been continued on the same lines by Mr. Adam Sedgwick, Mr. W. H. Caldwell and Dr. Hans Gadow as lecturers, and Mr. Walter Heape and Mr. W. F. R. Weldon as demonstrators. In the Lent Term of this year 63 students attended the elementary class, and 26 the advanced classes. Five students have been engaged in original work. Mr. A. J. Balfour, M.P., has offered to give annually a sum sufficient to defray the cost of the complete series of scientific journals taken in by his late brother.

Dr. Vines has carried on practical instruction in Vegetable Anatomy and Physiology under considerable difficulties owing to the small space available; he has had to repeat all the work four times. The numbers attending his practical classes in the Michaelmas Term, 1882, were 19; in the Lent Term, 1883, 37; in the present Easter Term, 35.

Prof. Hughes reports that the whole of the geological library, consisting of 800 volumes and 1000 pamphlets, of the late Mr. E. B. Tawney, have been liberally presented to the Woodwardian Museum by his brother, Mr. C. H. Tawney, late Fellow of Trinity College. Opportunity has been taken in the past year to largely improve the foreign Tertiary collections in the museum.

Prof. Stuart reports the addition of a number of machines and a large development of his classes; a foundry begun as an experiment has proved one of the most successful parts of his undertaking.

The Philosophical Library in the new Museums has been largely increased by the valuable presents made by the family of the late Prof. Balfour, by Mr. J. W. Clark, by Prof. Darwin, Prof. Humphry, Prof. Newton, and others.

It has been recommended by the Special Board for History and Archæology that a separate Board be created for Archæology, distinct from that of History. This has been concurred in by the General Board of Studies.

The Botanic Garden Syndicate have reported many improvements in the collections of trees, of rock vegetation, and in the Plant Houses. The largest specimens in the Palm House have been safely lowered to about 2½ feet below the ground level. All the genera of carnivorous plants in cultivation and most of the species are now in the collection. *Vitis gongylodes* has been flowered for the first time in this country. The Curator, Mr. Lynch, was deputed to visit the Botanic Gardens at Dublin, Manchester, and Liverpool, and has also visited Chatsworth with the result that much valuable information has been obtained in all departments of management and cultivation, and many important exchanges have been made.

The Adams Prize, for a general investigation of the action upon each other of two closed vortices in a perfect, incompressible fluid, has been awarded to Mr. J. J. Thomson, M.A., Fellow of Trinity College.

Messrs. W. H. Besant and E. J. Routh are the first to be notified as "approved by the general Board of Studies for the Degree of Doctor in Science."

Candidates for the Professorships of Physiology and Anatomy are requested to send their names to the Vice-Chancellor on or before June 7.

THE Institute of Agriculture, South Kensington, will give an extended series of lectures next winter, beginning on October 1. The following courses are arranged for:—Mr. Bernard Dyer, Chemistry in Relation to the Soil; Mr. F. Cheshire, Practical Course on the Use of the Microscope (these two courses to be delivered in the Lecture Theatre of the Museum of Geology, Jermyn Street). The next series will be given in the Lecture Room of the Natural History Museum, South Kensington: Mr. Bettany, Vegetable Physiology; Mr. Worthington Smith, Diseases of Farm Crops; Prof. J. W. Axe, Animal Physiology in Relation to Farm Stock; Miss E. A. Ormerod, Farm Insects; Mr. W. Topley, Geology and Physical Geography in Relation to Agriculture. The remaining courses will be given in the Lecture Theatre of the South Kensington Museum: Prof. Tuson, the Chemistry of the Food of Farm Stock; Prof. Buckman, Farm Seeds; Prof. Tanner, Agriculture; Mr. R. Holland, Management of Grass Land; Mr. Gilbert Murray, Breeding and Management of Horses; Mr. W. Housman, Cattle; Mr. H. Woods, and Mr. J. A. Clarke, Sheep; Prof. J. W. Axe, Preventable Diseases of Farm Stock; Farm Implements and Machinery, Mr. W. R. Bousfield and Mr. W. W. Beaumont. A distinct course of lectures will be given on Poultry, Dairy, and Bee Management. The arrangements made enable students to give their undivided attention to one subject at a time, two lectures being given daily till the subject is completed. The fees being at the rate of half a guinea for each week's course of ten lectures, and any student being allowed to attend a single course, the greatest facility exists for persons choosing their work according to their needs or convenience. Thus it is believed, after the success of the tentative courses of the past winter, that many sons of tenant farmers will find this a most valuable and available mode of acquiring an agricultural education.

SCIENTIFIC SERIALS

THE *American Naturalist* for March, 1883, contains:—On the extinct dogs of North America, by E. D. Cope.—On the plains of Michigan, by V. M. Spalding.—Organic physics, by Charles Morris.—Indian music, by E. A. Barber.—On the occurrence of fossiliferous strata in the lower Ponent (Catskill) group of Middle Pennsylvania, by E. W. Claypole.—Pitcher plants, by Joseph F. James.

April, 1833, contains:—The Naturalist Brazilian Expedition, No. 1, from Rio de Janeiro to Porto Alegre, by H. S. Smith.—Unnatural attachments among animals, by J. D. Caton.—Butterfly hunting in the desert, by W. G. Wright—The extinct Rodentia of North America, by E. D. Cope.—Heterogenetic development in Diaptomus, by C. L. Herrick.—A study of the immature plumage of the North American shrikes to show their descent from a common progenitor, by Thos. H. Streets.

May, 1883, contains:—Wampum and its history, by E. Ingersoll.—The Naturalist Brazilian Expedition, No. 2, by H. S. Smith.—The Polar organisation of animals, by C. Morris.—On the classification of moths, by A. R. Grote.—Heterogenetic development of Diaptomus, by C. L. Herrick.—On the morphology of arteries, especially those of the limbs, by F. Baker.—The hairy woodpecker, by A. G. Van Aken.

Archives Italiennes de Biologie, tome ii. fasc. 2, November 30, 1882, contains among the original articles the following:—On the minute anatomy of the muscles which move the wings of insects, by G. V. Ciaccio.—On the structure of striated muscular fibre in some vertebrates.—On the development and the morphology of the kidney of osseous fish, by C. Emery.—On the substance preventing the coagulation of the blood and lymph whilst these contain peptone, by Jules Fano.—On the germs and lower

organisms found in ordinary and malariac earths, by A. Ceci.—Transfusion of blood and its effects on nutrition, by P. Albertoni.—On the pathological anatomy of the cornea in the glaucomatous eye, by F. Tartuferi.—On the presence of a cordon or slip on the Uncus of the Hippocampus in the brain of man and some other animals, by C. Giacomini.—On the chemical composition of the egg and its envelope in the common frog (*Rana temporaria*), by P. Giacosa.—Anatomical considerations of the doctrine of cerebral localisations, by C. Golgi.

Tome ii. fasc. 3, February 1, 1883, contains anatomical considerations of the doctrine of cerebral localisations, by C. Golgi (continued).—On compensative hypertrophy of the kidney, by C. Golgi.—Experimental studies on hypnotism, by A. Tamburini and G. Seppili.—The origin of the mesoderm and its relations to the vitellus, by G. Romiti.—On the anatomy of a fœtal Otaria (*O. jubata*), by L. Camerano.—On the physiology of smooth muscular tissue, by A. Capparelli.—On the physiological action of certain substances on the vesical muscles, by P. Pellacani.—On the anemia of miners from a parasitological point of view, by E. Perroncito.—On the change in form of uric acid by the action of glycerine, by J. Colasanti.—On Ptomaines, by J. Guareschi and A. Mosso.—On some endoparasitic Protista, by Dr. Grassi.

Tome iii. fasc. i., April 15, 1883, contains:—On the sanitary improvement of the Roman Campagna, by C. Tommasi-Crudeli.—On the anemia of miners (conclusion), by E. Perroncito.—On some endoparasitic Protista (conclusion), by Dr. Grassi.—On the presence of a secretive tissue in vertebrates, by C. Emery.—On vibratile endothelium in mammals, by J. Paladino.—On the attenuation of charbon virus, and on its transmission from mother to fœtus, by E. Perroncito.—On the acoustic epithelium, by A. Tafani.—On the termination of nerves in the striated muscles of torpedo, by J. V. Ciaccio.—The general physiology of smooth muscular tissue, by E. Sertoli.—On a new morphological element of the blood, and its importance in thrombosis and coagulation, by J. Bizzozero.—New studies of the chestnut disease, known as the ink disease, by J. Gibelli.

THE *Bulletin de l'Académie Royale des Sciences, des Lettres, et des Beaux-Arts* for 1883, part i., contains papers by F. Henrijean, on the part played by alcohol in nutrition; by MM. Valerius and Van der Mensbrugghe, on M. Delaurier's observations on the concentration of solar rays and the transformation of electricity into heat; by W. Spring, on the colour of marine, lacustrine, and fluvial waters; by C. Le Paige, on the homography of the third order in algebra; by Baron Northomb, on the political relations of the Netherlands during the seventeenth century.

SOCIETIES AND ACADEMIES

LONDON

Royal Society, February 1.—"On the Affinities of Thylacoleo." By Prof. Owen, C.B., F.R.S., &c.

Since the communication of the paper "On Thylacoleo," in the *Philosophical Transactions* for 1871, further explorations of the caves and breccia-fissures in Wellington Valley, New South Wales, have been made, by a grant for that purpose from the Legislature of the Colony, and carried out by E. B. Ramsay, F.L.S., Curator of the Museum of Natural History, Sydney. The present paper treats of the fossils contributing to the further restoration of the great carnivorous Marsupial (*Thylacoleo carnifex*, Ow.) They exemplify the entire dentition *in situ* of the upper and lower jaws of a mature individual; the bones of the forelimb, of which those of the antibrachium and the ungual phalanges are described, are compared with those of other Marsupials, and of placental, especially feline, *Carnivora*. An entire lower jaw with the articular condyles adds to the grounds for determination of the habits and affinities of the extinct Marsupial.

Figures of these fossils of the natural size accompany the paper.

Geological Society, May 9.—J. W. Hulke, F.R.S., president, in the chair.—Rev. William Spiers and H. A. Williams were elected Fellows of the Society.—The following communications were read:—The age of the newer gneissic rocks of the Northern Highlands, by Mr. C. Callaway, D.Sc., F.G.S., with notes on the lithology of the specimens collected, by Prof. T. G. Bonney, F.R.S. The object of the author was to prove that the eastern gneiss of the Northern Highlands, usually regarded as of "Lower Silurian" age, was to be placed in the Archæan. While admitting that this gneiss frequently overlies the quartzo-dolomitic group of Erriboll and Assynt, he held that this relation was due to dislocation accompanied by powerful thrust from the east, which had squeezed both formations into a series of folds, thrown over towards the west, so as to cause a general easterly dip. In Assynt the "Upper Quartzite" was first discussed. The author described several sections which he considered to prove that this band was the ordinary quartzite repeated east of a great fault, which brought up the Hebridean; in one place, Glen Coul, the quartzite being conformably succeeded by the brown flags and dolomite. The "igneous rocks" of Nicol ("Logan Rock" of Dr. Heddle) were regarded as the old gneiss brought up by a fault and thrown over on to the Assynt group to the maximum breadth of more than a mile. The "Upper Limestone" of authors was described as either outliers of the dolomite or a part of the Caledonian series. The "Caledonian" rocks were seen in Glen Coul to be immediately overlying the Hebridean, the Assynt group being caught in the angle between the two gneisses, and bent back in overthrown folds. The mountain groups of Assynt were described as usually consisting of cores of Hebridean gneiss swathed in or capped by sheets of quartzite. In the former case the quartzite on the western slopes was contorted into overthrown folds by the thrust from the east. In the Loch Erriboll district, the "granulite" of Nicol was considered to be a lower division of the Caledonian gneiss, though bearing some resemblances to the Hebridean. In other respects the views of Nicol were regarded as substantially correct. Along the entire length of Loch Erriboll, a distance of about twelve miles, the thrust from the east had bent back the Assynt group into overthrown folds, and pushed the Caledonian gneiss on the top of the inverted quartzite. This had produced the appearance of an "upper" quartzite passing "conformably" below the eastern gneiss. The superior antiquity of the Caledonian was confirmed by the occurrence of outliers of quartzite upon the Arnaboll (Lower Caledonian) series, and by the fact that the granite, which sent numberless veins into the gneiss, never penetrated the quartzite and associated rocks.—On a group of minerals from Lilleshall, Salop, by C. J. Woodward, B.Sc., F.G.S.—Fossil Chilostomatous Bryozoa from Muddy Creek, Victoria, by A. W. Waters, F.G.S.

Chemical Society, May 17.—Dr. W. H. Perkin, president, in the chair.—Capt. W. de W. Abney, F.R.S., delivered a lecture on photographic action studied spectroscopically. The lecturer said he wished that all chemists were photographers; photography occupied the borderland between chemistry and physics; he was firmly convinced that photographic action was interatomic. The action of a developer was then experimentally illustrated; this action is physical. Light causes the liberation of iodine in a film of silver iodide, and the developer precipitates metallic silver. The silver so reduced is infinitesimal, and must be in many cases derived from the film. The positive pole of the electric arc was found to be the best source of light. Gratings could not be used for quantitative work, as they varied so much in their ruling; a glass prism was therefore used to form the spectrum. A film of silver chloride absorbs only the violet end of the spectrum; silver iodide absorbs more, and the bromide most of all; accordingly when a photograph of the spectrum was taken on these three films it was seen that the portion of the chloride acted upon was very much less than when bromide of silver was used. It was shown that a sensitiser essentially takes up the halogen liberated by the action of light. One salt of silver may act as a sensitiser to another salt of silver. Photographic action is completely prevented by the presence of oxidisers, as bichromate, &c. Reverse photographs were discussed, and the action of sodium sulphite in preventing the evil effects of over exposure. The peculiar green condition of silver bromide which is sensitive to ultra-red rays was explained. In conclusion the lecturer said that his principal object was to warn chemists of some of the numerous pitfalls which they might encounter in scientific photography.

Meteorological Society, May 16.—Mr. J. K. Laughton, F.R.A.S., president, in the chair.—F. A. Bellamy, T. A. Mercer, Rev. H. J. Poole, and A. Wise, M.D., were elected Fellows of the Society. The following papers were read:—Composite portraiture adapted to the reduction of meteorological and other similar observations, by G. M. Whipple, B.Sc., F.R.A.S. It has often been remarked that one of the main, if

not the chief, of the difficulties the meteorologist has to contend with, is the enormous amount of preliminary labour which has to be expended in the not very pleasing task of forming the observations he may wish to discuss into tables, casting the columns of figures so obtained, and then computing the means. With the view of arriving at results by a shorter cut, the author has been led to consider the possibility of employing a method, suggested by a consideration of the highly ingenious system of composite portraiture, invented by Mr. Francis Galton, F.R.S., and utilised in his anthropological studies.—Note on atmospheric pressure during the fall of rain, by H. Sowerby Wallis, F.M.S. The author discusses the condition of atmospheric pressure while rain was falling, during 1882, and finds that, out of a total of 136 rainy days (which were available for his purpose), on 54 per cent. the rain was accompanied by diminishing pressure, on 27 per cent. by increasing pressure, and on 19 per cent. by steady pressure.—New method of reading a thermometer and hygrometer at a distance by means of electricity, by Arthur W. Waters, F.G.S.—An integrating anemometer, by W. F. Stanley, F.M.S.—Observations on the force of the wind at sea, by D.W. Barker, F.M.S.—Meteorological observations at Zanzibar, east coast of Africa, during 1880 and 1881, by Surgeon-Major C. T. Peters, M.B.—Diurnal rainfall at Bangkok, Siam, by Capt. G. H. Inskip, F.R.G.S.

BERLIN

Physiological Society, April 27.—Dr. Mendel read a paper on the anatomy of the corpus striatum and lenticular nucleus. The older view, which was supported by the valuable anatomical researches of Prof. Meynert, was that the relation of the corona of radiating fibres above the lateral ventricle ("Stabkranz") to the lenticular nucleus and corpus striatum consisted in this, that in it ran bundles of nerve-fibres, which arise from the brain cortex and end in the large ganglia, whereas Dr. Wernicke three years ago propounded the view that a connection did not exist between the brain cortex and the corpus striatum and lenticular nucleus, but that these latter were bodies of the same range as the cortex. Dr. Mendel has for some years past studied the anatomy of these parts of the brain very attentively, and has been brought back to the older view by a series of sections (of the brain) of dogs, monkeys, and men, which series he laid before the Society. He found not only the bundles of out-streaming fibres, which alone were acknowledged to exist by Dr. Wernicke, but also a larger number of in-streaming bundles of fibres which show the connection of these brain-nuclei to the cortex. In the discussion Dr. Wernicke stated that he was not convinced by the paper or preparations of the correctness of the view propounded by Dr. Mendel, whereas Prof. Munk believed that his not-yet-completed physiological experiments afford grounds for Dr. Mendel's view.

Physical Society, May 4.—Prof. Hauck laid before the Society a model of a mechanical apparatus which solves the problem of combining drawings and photograms, which are drawn in two planes into a combination figure in the third plane. Prof. Hauck then explained the principle of the apparatus, and pointed out by means of geometrical figures the conditions which must be fulfilled in order to project any given points of two planes in common points of a third plane. He then proceeded to the complicated problem of bringing points of three planes, which meet in a corner, to a common projection, and applied these figures to the special case of projecting the perspective drawing of a building from its ground-plan and elevation. The model was calculated and arranged for this case, but the apparatus, in which the motions are produced by means of polished lineals, each running upon two pins, can be put to manifold uses in physical space investigations.

PARIS

Academy of Sciences, May 14.—M. Blanchard, president, in the chair.—The following papers were read:—On the pyro-electricity of quartz, by C. Friedel and J. Curie, second part.—On the cultivation of the cacao plant, with an analysis of the constituent elements of the cacao and chocolate berries, which were shown to contain in various proportions albumen, legumine, phosphates, fat, starch, sugar, theobromine, besides the materials entering into the formation of bone.—On the action of birds in flight studied by means of photography, with figures showing the successive positions of a pigeon on the wing at intervals of one-ninth and one-eighth of a second, and a closed curve representing the trajectory of the tip of the wing obtained by means of a special contrivance, by M. Marey.—On a double sulphate of iridium and potassium, by M. Lecoq de Boisbaudran.—On the diminution of virulence in carbon bacterides and their spores under the influence of antiseptic substances, by MM. Chamberland and Roux.—On iodine associated with the sedative alkaloids of opium treated both as a preventative and curative in the case of typhoid fever, by A. Delbovier.—On the immunity against attacks of Phylloxera enjoyed by the vine cultivated in the sandy soil of Algeria, by MM. F. Convert and L. Degrully.—Observations on the new planet 233 Borelly made at the Paris Observatory, by G. Bigourdan.—On the determination of the meridian in low latitudes, such as that of Rio de Janeiro, by M. Cruls.—On the conservation of energy and periodicity of the solar spots, by A. Duponchel.—On the laws of coincidences between the reductions of periodical fractions of the "two modes," by E. de Jonquières (continued).—On the generalisation of Thermat's theorem of numbers due to M. Serret, by M. Picquet.—On the possibility of extending to any electrolytic field the electro-chemical method in the figuration of potential distribution, by A. Guébhard.—On the influence of atmospheric pressure on the eruptions of gas and water in the Montrond Geyser (Loire), by F. Laur.—On the differences in the temperature of the sea and air, by M. Semmola.—On the quantitative analysis of sulphur and carbon in sulphocarbonates, by A. Müntz.—On the regular surface-fissures in certain rocks, such as the hard eocene limestone used in the construction of the old ramparts of Genoa, by Ch. Contejean.—On new physiological studies of the torpedo, by M. Marey.—On the functions and organs of suction and deglutition in the leech, by G. Carlet.—On a case of purulent ophthalmia produced by the infusion of the seeds of the liquorice plant, by L. de Wecker.—On the fundamental principle of the electric log now in use in the French fleet, by M. G. Le Goarant de Tromelin, who claims priority of invention over the electric log invented by M. Fleuriais.

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THURSDAY, JUNE 7, 1883

WIEDEMANN'S "ELECTRICITY"

Die Lehre von der Electricität. Von Gustav Wiedemann. Vol. I., pp. xi. and 795 (1882); Vol. II., pp. vii. and 1002 (1883). (Braunschweig: Vieweg.)

FOR more than twenty years Prof. Wiedemann's "Lehre vom Galvanismus und Elektromagnetismus," first published in 1861, has been recognised without question as the leading authority and great storehouse of facts on the branch of science of which it treats. It is a practically exhaustive treatise, and each of the two editions (second edition, 1872 to 1874) marks with wonderful accuracy the high-water mark of knowledge of its subject up to the date of publication. It is safe to assume that any fact that is not to be found recorded in its pages had not been discovered, or at least had not been published, up to the date of completion of whichever edition is examined. The fulness and accuracy of the references to original authorities give to Prof. Wiedemann's book a unique value also as a classified index to the literature of galvanic electricity and electromagnetism.

The work which forms the subject of this notice, is in one sense a third edition of the "Lehre vom Galvanismus." It appears however under a new title, and is in fact to a great extent a new book. It is characteristic of the direction taken by the advance of electrical science during the last twenty years that, while Prof. Wiedemann found it practicable to confine himself in his first and second editions almost exclusively to the phenomena of current electricity and of magnetism, he has found it advisable in the present edition to enlarge the scope of his work so as to make it include the whole range of electrical science. It is true that the second edition contains an important chapter devoted to the discussion of a phenomenon that has usually been considered in connection with statical electricity, namely, the disruptive discharge in gases of different densities; but this is almost the only part of the book in which the considerations that have to be dealt with in treating of electrostatics occupy a prominent place. It is however becoming less and less possible to treat satisfactorily of one branch of electricity apart from the remainder. The terms frictional electricity and galvanic electricity have evidently an historical rather than a scientific origin. They do not refer to any logical classification of phenomena, but to two among the many processes by which electrical effects can be originated. It is not even by any means certain that electrification by friction is fundamentally a distinct phenomenon from electrification by contact as this occurs in a galvanic cell; on the contrary, various recent investigations tend to show that these actions are essentially similar, and that the friction which takes place in one case is of the nature of an accidental accompaniment. As a matter of fact, however, an electrical machine acting by friction serves (or at least did so until recently) as the readiest means of producing one large class of electrical phenomena; while a galvanic or voltaic battery serves (or at least did so until recently) as the readiest means of producing another large class of phenomena. Thus the division of electrical science for the purposes of study into frictional electricity and galvanic electricity originated in considerations of experimental convenience rather than in any strictly scientific distinction. So far as such a distinction can be drawn between these two branches, it may be said that the former includes the study of all those phenomena in which difference of potentials is the most characteristic factor; while the latter includes the study of phenomena characterised by the transfer of electricity. As examined by the instruments in use five-and-twenty years ago, the effects produced by the electrical machine seemed distinct enough from those due to the galvanic battery—indeed the difficulty rather was to establish their mutual connections; but with the galvanometers and electrometers that are now—thanks to Sir William Thomson—in the hands of every electrician, nothing is easier than to measure the current of an electrical machine or the difference of potentials of a galvanic cell. Moreover the recent rapid development of methods of converting mechanical into electrical energy, through the agency of magneto-electric induction, has made us familiar with the production of currents of great strength associated with great differences of potential. It is, however, not only the introduction of new instruments and apparatus, and the increased power over electrical phenomena that modern experimentalists have thereby acquired, that make it less possible now than formerly to treat of the laws of electric currents without reference to the principles of electrostatics. The conception that the immediate cause of the phenomena exhibited in either an electric or a magnetic field has its seat, not in electrified conductors, or in magnets or conducting wires, but in an impalpable medium existing throughout space, has completely shifted the scientific point of view as regards electrical effects. What is now demanded of electrical theory is an explanation of the conditions of the medium which are perceptible by us as the properties of an electric or magnetic field. The wider problem of the constitution of the electric medium, whether identical or not with the luminiferous ether, embraces in itself the phenomena of electrostatics, of electric currents, and of magnetism

There was thus every reason to wish that Prof. Wiedemann might be able to treat electrical science as a whole in the same complete way in which he had previously treated the portions included within the scope of his previous book. This is what he has now undertaken and in great part accomplished. The task is an enormous one, and probably, to any one except the man who has set himself to it, would have seemed overwhelming. Prof. Wiedemann's industry and care, however, never seem to fail before any mass of descriptive detail or complex mathematical discussion, and students of physics may therefore be congratulated upon the near prospect of having from his pen a complete treatise on electricity.

The first of the two volumes already published begins with a section on the General Properties of Electricity, including an historical sketch of early observations, the development of electricity by friction, &c., electrostatic attraction and repulsion, distribution on conductors, and a description of the various forms of electroscopes and electrometers. Then follows a section on the development of electricity by contact of heterogeneous bodies; next Ohm's law and its applications, the measurement of electrical resistance and of electromotive force, and a

description of various galvanic elements. These subjects occupy the first volume, consisting of close on 800 pages. The second volume begins with the electrical properties of dielectrics. The section devoted to this subject is erhaps the most interesting in the volume: it contains the mathematical theory of the behaviour of dielectrics, the experimental investigation of specific inductive capacity, the detailed study of electrical machines acting by friction and by induction, together with various allied matters. Next come thermoelectricity, pyroelectricity, and the thermal effects of the discharge of accumulated electricity and of continuous electric currents. After this follows the section devoted to electrochemical action: this occupies about five hundred pages, and concludes with a chapter on the theory of electrification by contact, which completes the volume. It is intended that the whole work should be finished in four volumes, and the manuscript of the two that still remain to be published is for the most part ready.

Prof. Wiedemann's great work has been so long known to physicists that it is needless for us to dwell upon its special qualities farther than to say that it fully retains in its new form all its old characteristics. It is true that it lacks the originality and unity of treatment of Clerk Maxwell's "Electricity and Magnetism," probably the most original systematic treatise on any great branch of physics that was ever written. Nor does it equal in the clearness and elegance of its mathematical discussions the treatise of Mascart and Joubert, a work which, while not laying claim to originality in respect of matter, exhibits in a remarkable degree consecutiveness and lucidity of exposition. Prof. Wiedemann's plan precludes his attaining to these particular excellences in an equal degree. Some sacrifice of unity and consecutiveness is inevitable in a work which aims not only at giving a complete account of what is known respecting a great branch of science, but also at showing what each author has contributed to the stock of knowledge and how he has presented it. From this point of view Prof. Wiedemann's book is without a rival in any language, and is indeed unapproached by any other work. G. C. F.

FLORA OF HAMPSHIRE

Flora of Hampshire, including the Isle of Wight, or a List of the Flowering Plants and Ferns found in the County of Southampton, with Localities of the Less Common Species. By Frederick Townsend, M.A., F.L.S., &c. Illustrated with Two Plates and a Map. (London: L. Reeve and Co., 1883.)

WE have here an important addition to the already large class of English local floras. To the general botanist, as to people who have made no study of botany, it would seem that the plants of so comparatively small a region as the British Islands must have been catalogued long since, and that there is little to be done in that direction which is worth doing. It certainly is remarkable that, besides facts connected with geographical distribution, which a more minute knowledge of the plants of a country must bring to light, there are actually new plants to be found—new, that is to say, not only to Britain, but to science. A Pondweed (*Potamogeton Griffithii*), new to science, has recently been described and figured by Mr. Arthur Bennett in the *Journal of Botany*, from specimens brought from a mountain lake in North Wales—the only place in the world where it is known to occur. Not that this is the only species peculiar to these islands. To take one example, there is a species of Centaury (*Erythræa latifolia*),[1] which has never been found anywhere in the world but on the Lancastrian sandhills; and there it is not known to have been seen more recently than 1865, if then. In Mr. Townsend's county, a Spearwort (*Ranunculus ophioglossifolius*), not hitherto found nearer these shores than Jersey, has been detected so lately as to appear only on the very last page of the book; *Spartina Townsendi* is another case in point; and another example of a plant having been long overlooked, and of which the distribution has quite recently been much extended, will be found in *Arum italicum*, which was detected in the Isle of Wight in 1854, and was afterwards found in West Cornwall and Sussex; this was recorded for Dorset last year, and its range has been extended during the present year to Kent (Folkestone). The volume now before us supplies a good illustration of the way in which novelties may turn up in the best known districts. Probably if there is one part of England which has been more thoroughly botanised than another it is the Isle of Wight; yet it was here, and in one of the best known parts—the Downs at Freshwater—that Mr. Townsend first distinguished in 1879 an *Erythræa* (*E. capitata*, var. *sphærocephala*), which is, as he says, "a peculiarly interesting addition to the British flora. It is," he continues, "a well-marked species, and is not known now to occur anywhere else in the world but in the Isle of Wight and in Sussex. The other form of it was found some fifty years ago somewhere in the neighbourhood of Berlin (the exact locality not being known), and though sought for diligently, it has never been found again."

It will doubtless seem strange to some to learn that a volume of more than 500 closely-printed pages can be occupied by an enumeration of the plants of one English county, especially when it is considered that the pages devoted to descriptions of species are very few. An interesting and instructive article might be written in which the history and development of the local flora should be traced. To undertake such is, however, not our present purpose; but we may note one or two of the more striking features of these later contributions to local botany, of which the "Flora of Hampshire" is the most recent. One thing to be noticed is their historical nature. Messrs. Trimen and Dyer, in their "Flora of Middlesex" (1869), were the first to develop this aspect of the work: their method of quoting the first authority for the occurrence of the species as a Middlesex plant has been followed by subsequent writers, and they also did good service by quoting the synonymy of the older (and pre-Linnean) authors—a work which has been very useful to their successors. When it is considered that a book of this kind is mainly undertaken by persons interested in the history of some particular locality, it seems natural that what has been called the antiquarian side of botany should be represented, although there are those who consider that

[1] The plant so named in Continental floras is certainly not the same as that of the Lancashire sandhills.

this line of action directs attention to persons rather than plants, and is thus out of place.

Another point to which much more attention is given now than was formerly the case is the division of a county into districts. Messrs. Webb and Coleman, in the "Flora Hertfordiensis" (1849), planned their divisions with reference to the river drainage; and this has been carried out in the best floras of later times. If it were generally adopted, and if our list of county floras were complete, we should arrive at a much greater knowledge of plant distribution than we have at present. The arbitrary boundaries of counties would give way to the natural divisions afforded by the various river-basins, and one county flora would fit into another, and form a harmonious whole. This subject has lately been worked out by Mr. Boulger in a careful paper "On the Origin and Distribution of the British Flora," published in the *Transactions of the Essex Field Club*. No one who has not tried it would suspect how greatly the floras of contiguous river-basins will be found to differ from each other.

It is time, however, to speak of Mr. Townsend's important contribution to our knowledge of local botany. As is well known, the work has occupied him during a large number of years: it has, we regret to say, been retarded by the ill-health of its author, or it would have been published two years since; but Mr. Townsend tells us that the delay has enabled him to improve the book in various details. The county is divided into twelve districts, two of which are in the Isle of Wight. A small but extremely clear and useful map showing the boundaries of these is given. The usual lists of books quoted and herbaria consulted are followed by a short sketch of the plan of the flora. The distribution of each species through the districts and subdistricts is then worked out at length. We confess to feeling some disappointment at the comparative fewness of the critical notes upon species. Mr. Townsend's extensive knowledge of British plants, especially in their relations to the Continental flora, had led us to expect that we should have had a good deal of additional light thrown upon some of our critical forms; but this, although not altogether wanting, occupies but a small portion of the volume. Mr. Townsend's notes are for the most part in the appendix—an arrangement which seems to us open to various objections, not the least being the fact that these notes and descriptions are often not mentioned in the index. Two or three varieties are described and named for the first time in these pages; and occasionally a specific name new to the British flora makes its appearance, as in the case of *Glyceria declinata* of Brébisson, with which Mr. Townsend identifies a plant which he had previously considered a dwarf variety of *G. plicata*.

One or two points seem to us open to criticism. "First record" in books of this kind is usually taken to mean first record in print; but this is not Mr. Townsend's view of the phrase. Thus under *Centaurea cyanus* we find, "First record: Herb. Reeves, 1837." It does not seem to us that the existence of a specimen in a private herbarium can be considered a record of its occurrence in the ordinary acceptation of the term. Sometimes we do not quite understand the author's meaning, as when he marks the curious and interesting *Spartina Townsendi* as "certainly introduced," although it has as yet been found nowhere else in the world. Equally puzzling is this sentence as to the specific rank of the same grass: "I believe this plant must take the rank of a sub-species; the characters which separate it from *S. stricta* being so important and distinctly marked. It is easily distinguished from *S. alterniflora*." This being so, surely it should be ranked as a full species? Mr. Townsend admits *Anthoxanthum Puelii* as indigenous, but its frequent substitution for *A. odoratum* by seed merchants throws much doubt upon its nativity: this plant, first found in Hampshire in 1874, had been collected in Cheshire two years previously, but Mr. Townsend cites the last-named county as one of those in which it "has since been found." We can, from observation of the two plants in several counties, confirm the statement of Mr. Pryor, which is doubted by Mr. Townsend, that *Viola Reichenbachiana* flowers about a fortnight earlier than the allied *V. Riviniana*. Some plants are included as natives of Hampshire on what seems to us insufficient evidence; *Silene noctiflora* is one of these, and *Orchis hircina* another. This latter, we do not hesitate to say, requires much confirmatory evidence before it can be accepted as a Hants plant; its occurrence rests solely on a manuscript note of the late Mr. Reeks, who stated that specimens had been found by a Mr. Lockart at St. Mary Bourne about 1866. The number of misprints is very considerable.

Such criticisms as these—and they might easily be extended—do not, however, prevent the "Flora of Hampshire" from taking a foremost rank among works of its class. A little more attention to uniformity would have improved the book, and, as we have shown, there is room for difference of opinion upon many of the points raised; but British botanists will be grateful to Mr. Townsend for giving them a handbook to the flora of one of the most interesting and beautiful of our English counties.

JAMES BRITTEN

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications.*

[*The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to insure the appearance even of communications containing interesting and novel facts.*]

On Real and Pseudo-Reversals of Metallic Lines

I AM much indebted to the courtesy of Prof. Liveing for a copy of a paper extracted from the *Proceedings of the Cambridge Philosophical Society*, vol. iv. part 5, p. 256, on the circumstances producing the reversal of spectral lines of metals, by Professors Liveing and Dewar. In this communication the following paragraph occurs:—"Prof. Hartley has lately (*Proc. Roy. Soc.* xxxiv. p. 84) called attention to pseudo-reversals of this class, which may be produced in the case of a strong line by over-exposure. It is well known that over-exposure (solarisation, as we used to call it formerly) produces such an alteration in the sensitive preparation of the photographic plate that the over-exposed parts cease to be developable, so that a very strong line may appear white in the negative where it ought to be black, but with a dark border, and so give the appearance of a reversed line. Prof. Hartley finds it difficult to distinguish real reversals of the class we are now discussing from these pseudo-reversals. His difficulty has not occurred to us, first, because we have always been in the habit of taking photographs in series with varying exposure, in order to get impressions both of the feeble lines in some and of strong lines in others; and

secondly, because we almost always close part of the slit of the spectroscope with a shutter, so that the image is cut off sharply by the shadow of the shutter. Strong lines extend into the shadow more or less, and if there is a real reversal the extension of the reversed part into the shadow is trumpet shaped, whereas if it is only a pseudo-reversal it is closed."

I beg to be allowed to call attention to one or two points in the above quotation which I imagine may lead to a misconception of the phenomena observed, and of my remarks thereon.

First, as regards over-exposure, it is assumed that solarisation is an equivalent for this expression. This is the case only when speaking of the cause, but the word has been used by photographers for many years to describe the effect of over-exposure.

In all collodion processes, wet or dry, this effect is an undue intensity of the high lights and an overpowering of the intermediate tints and delicate shadows adjoining them. This appears to be due to the fact that from the intensity of the light not only the direct rays, but those reflected from the back of the glass plate, or even those which are scattered, have sufficient power to act upon the sensitive film. In photographs of spectra this is seen in the nimbus or halo surrounding the strongest metallic lines, which disguises their form. It is well illustrated by my photographs of the magnesium, cadmium, and other spectra, published in the *Journal of the Chemical Society*, vol. xli., *Transactions*, 1882, p. 90.

Although I have worked with dry plates of almost every description, and with some modifications prepared by myself which have never been described, I do not recollect having observed that over-exposure causes any other effect than a too dense deposit of silver, excepting when the vehicle for the sensitive salt is a film of gelatine. As far as my experience goes, it is a property peculiar to gelatine plates, that with such extreme facility they are incapable of development after too strong an action of light, and I carefully avoided the term solarisation, since it has been used to describe an effect so different from that to which I desired to call attention.

Secondly, with regard to difficulty in distinguishing reversals, the sentence above does not exactly represent my experience, and I think it may be seen by those who read my communication, that any want of distinction between real and pseudo-reversals had reference only to photographs which had been already taken with a fixed period of exposure, and that I advocated a method of comparative exposures as necessary in the study of spectra. It appears that this is one of the means whereby Professors Liveing and Dewar are able to draw distinctions between real and pseudo-reversals. The second method, namely, the use of a shutter, is extremely useful in observations on arc spectra, which have been so completely studied by them. I have been studying spark-spectra exclusively, and have not been giving special attention to reversals, in fact, endeavouring as far as possible to avoid them. The use of a shutter does not commend itself to me, since it would cut off a highly characteristic feature in spark-spectra which it is desirable to observe, namely, the extension of the lines, but I may here mention that a speck of dust on the slit, or a fine wire stretched across it, will answer the same purpose as a shutter, without obscuring any considerable portion of the spark, and may be conveniently employed. And now permit me to add one word: the same alteration in the intensity of the spark which results in real reversals also frequently causes pseudo-reversals. Sometimes simply a turn of the screw attached to the spring of the contact-breaker on the induction-coil is sufficient to effect this change.

W. N. HARTLEY

Royal College of Science, Dublin, May 18

The Northern Zoogeographical Regions

THE facts of zoogeography are so involved, and often apparently contradictory, that a skilful dialectician with the requisite knowledge can make a plausible argument for antithetical postulates. Prof. Heilprin, being a skilful dialectician and well informed, has submitted a pretty argument in favour of the union of the North American or "Nearctic" and Eurasiatic or "Palæarctic" regions (*Proc. Acad. Nat. Sc. Phila.*, 1882, pp. 316–334, and NATURE, vol. xxvii. p. 606), but Mr. Wallace has, with perfect justness it seems to me, objected to his proposition (NATURE, vol. xxvii. pp. 482, 483). As Prof. Heilprin's arguments have not been entirely met, however, permit me to submit some further objections to his views.

Prof. Heilprin has contended "(1) that by family, generic, and specific characters, as far as the Mammalia are concerned, the Nearctic and Palæarctic faunas taken collectively are more clearly defined from any or all of the other regions than either the Nearctic or Palæarctic taken individually; and (2) that by the community of family, generic, and specific characters the Nearctic region is indisputably united to the Palæarctic, of which it forms a lateral extension."

Prof. Heilprin has formulated these conclusions after a summary of the families and genera common and peculiar to the regions in question.

As to families, Prof. Heilprin has presented the following figures:—

	All.	Peculiar.
Nearctic	26	1
Palæarctic	36	0
Oriental	36	3
Australian	22	8
Ethiopian	44	9
Neotropical	31	8

The proportions of peculiar genera to the entire Mammalian faunas of the several regions are stated to be as follows:—

	All.	Peculiar.	Percentage.
Nearctic	74	26	35
Palæarctic	100	35	35
Oriental	118	54	46
Australian	70	45	64
Ethiopian	142	90	63
Neotropical	131	103	78

The question may naturally recur why the line which separates "regions" from "subregions" should be drawn between 35 and 46 per cent. rather than between 46 and 63 or 64 per cent., or even between 64 and 78 per cent. Prof. Heilprin has not told us why, and I am unable to appreciate the reason therefor. Surely it is not sufficient to answer by simply asking the questions put in NATURE (p. 606).

But an analysis of more (but only approximately) correct figures and a more logical classification of mammals than that adopted by Prof. Heilprin reveal facts materially contravening the tabular statements of that gentleman.

First we must exclude the marine mammals, because their distribution and limitation are determined by other factors than those which regulate the terrestrial ones. A consideration then of the terrestrial forms leads to the following results:—

The Arctamerican or Nearctic region has 27 families, of which 11 are not shared with Eurasia and 4 are peculiar; it has 68 genera, of which 45 do not enter into Eurasia.

The Eurasiatic or Palæarctic region has 32[1] families, of which 17 are excluded from North America, and it possesses 89[1] genera, of which 60 have failed to become developed in America.

Such contrasts will more than compare generally with those existing between Eurasia and India, and even between the "Triarctic" or "Holarctic" and Indian "regions," and the same destructive process by which the northern regions are abrogated would entail the absorption of the Indian as well into a heterogeneous whole. The three can in fact be well united (as Cænogæa), and contrasted with a group (Eogæa) consisting of the African, South American, and Australian regions, as I long ago urged (*Ann. and Mag. Nat. Hist.* (4), xv. 251–255, 1875), but the claims of each to be considered as "regions" or realms is not thereby affected.

THEO. GILL

Smithsonian Institution, Washington, May 12

Deductive Biology

THOUGH no writer has yet afforded any remarks in criticism of Prof. Thiselton Dyer's "word of warning" to biologists, given in NATURE, vol. xxvii. p. 554, it does not, I think, follow that the objection raised by him is to be accepted as unanswerable. As no one of authority in such matters appears to be forthcoming, perhaps one who can lay no claim to being heard may still be permitted to venture to doubt the validity of the objection as given forth in such emphatic terms by Prof. Dyer, and to point out that most, if not all, of the scientific conclusions of importance, especially those accomplished during the present generation, have been arrived at mainly by means of the deductive method.

[1] These are the groups admitted by Prof. Heilprin, exclusive of the Pinnipeds.

The objection started is a serious one, for, if the deductive method is wrong at all, it is so absolutely, and must on no occasion be allowed a place in scientific reasoning, but—without any half-measure allowances—must be excluded altogether as a false and dangerous element of philosophy. If, on the other hand, we take exception, as I think we may do, to the exponent's opening expression,—"having formulated a few fundamental assumptions, to spin out from these explanations of what we see in the world about us . . . is merely a literary performance,"—as misleading in its main idea, we may still hold the method to be a perfectly scientific one.

The evolutionist, who has once ascertained by various careful experiments and extensive researches that there is a direct natural sequence of events in connection with certain phenomena, may be allowed to adopt set principles as recognised laws of action, fully as much as Euclid in the demonstration of problems from his formulated axioms. But perhaps Prof. Dyer's argument rests in reality upon his use of the word "assumptions," and thus his objection is merely urged against the false method assumed in his premise, rather than against deductive biology as a method of procedure, as he would have us believe. And so far, of course, every one will readily enough accept Prof. Dyer's remark as it stands.

But the conclusion that "the deductive method is a bad way of solving morphological problems" is opposed to all the evidence of Darwin himself, who constantly applied those well-tested principles which he had discovered even by this very method, and upon the bases of such fundamental *truths* it was that he reared his wonderful system. Are not the studies of comparative embryology and osteology, of comparative histology and biology, each founded entirely upon the method of deductive analogy?

As another sufficient witness, Mr. Wallace may be quoted as having adopted the same course with such remarkable results, and throughout his writings bears testimony to the value of deductive inference as a method of procedure, and I will deduce a couple of sentences taken from his work on "Island Life," bearing directly upon our point.

"On the theory of evolution," he says, "nothing can be more certain than that groups now broken up and detached were once continuous, and the fragmentary groups and isolated forms are but the relics of once widespread types which have been preserved in a few localities where physical conditions were especially favourable, or where organic competition was less severe. The true explanation of all such remote geographical affinities is that they date back to a time when the ancestral group of which they are the common descendants had a wider or a different distribution," &c., p. 296. And, in summary of the chapters on Madagascar, Mr. Wallace remarks: "The method we have followed in these investigations is to accept the results of geological and palæontological science, and the ascertained fact as to the powers of dispersal of the various animal groups; to take full account of the laws of evolution as affecting distribution, and of the various ocean depths as implying recent or remote union of islands with their adjacent continents; and the result is that wherever we possess a sufficient knowledge of various classes of evidence we find it possible to give a connected and intelligible explanation of all the most striking peculiarities of the organic world" ("Island Life," p. 419). We may then assuredly decide that the deductive system of logic,—the use without abuse of certain known factors,—instead of being in any way "bad," is (granting always that the general laws of Nature applied are sufficiently trustworthy) found to be even superior to the older and tardier processes of induction, to which the mere collectors of the facts dealt with have limited themselves, and has proved itself to be the *only* means of elucidating many of those abstruse problems, the solution of which has been conducive of such immense gain to the scientific philosophy of our day.

WILLIAM WHITE

Science and Art

As a rule it would be the extreme of absurdity for me to venture an adverse remark on the criticism of an art critic on paintings, yet there is one single exception regarding which I may perhaps be permitted to say a word or two.

In the very interesting critique on some pictures in the Royal Academy, written evidently by a master-hand, there is one picture—No. 764, "a snowstorm" (see NATURE, vol. xxviii. p. 76), not only somewhat severely, but I think unjustly or incorrectly, commented upon, because "there is not a single snowflake to be seen in the first twenty yards."

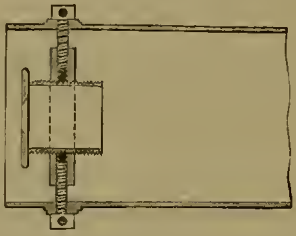
I have witnessed and been in the midst of many snowstorms in America, and some in Scotland. In a large proportion of these not a snowflake was to be seen, the snow being in very minute particles, so fine as to penetrate all openings in the clothing, however small. Snowstorms of this kind are the most dismal, bitter, and chilling of any.

On looking at Mr. Farquharson's picture, I was struck with its resemblance to a most unpleasant evening and night spent in the hills between the Coppermine River and Great Bear Lake, about 50 miles north of the Arctic circle. It being early winter, the weather was not very cold, but there was a combination of fog, fine snow (no snowflakes), and snowdrifts, which produced one of the most dismal and dreary scenes imaginable.

4, Addison Gardens, May 29 JOHN RAE

Transit Instrument

IN your issue of the 17th ult. (vol. xxviii. p. 51) you notice a cheap form of transit instrument introduced by me, and you point out the defect that no means are provided for placing the cross wires truly vertical. In all the most recent instruments which have been made this difficulty has been met by an arrangement which answers so effectively that I think it may interest others beyond those who are likely to use the instrument in question.



I employ the ordinary diaphragm with the usual four stretching screws, and the collimation is corrected in the usual manner by these screws. Into this diaphragm I insert a tube with a very fine screw, on the outside of which is fixed a plate carrying the cross hairs; by screwing this tube in or out the focus may be perfectly adjusted for objects at an infinite distance, while a slight additional movement to the right or left enables one at the same time to adjust the cross hairs truly vertical.

LATIMER CLARK

Sea-Shore Alluvion, Dungeness

REVERTING to an article in your journal of July 28, 1881, and a letter of mine in that for April 20, 1882, the following at the present time may be of interest:—

As regards the local wasting away in the bays east and west of Dungeness and the redistribution of the materials at Lighthouse Point;—on the west side the whole margin from Rye Bay to "Denge Marsh Gut," a distance of eight miles, has receded of late years; this is shown by the fact that the Denge Marsh authorities have recently erected a clay counter sea-wall at the back of the modern shingle "fulls" in front of the "Midrips" and "Wicks" (small land-locked pools of water) to check the overflow of the sea in south-west gales. This action is felt to the eastward in front of the "Holmstone," the Lydd coastguard station, and up to "Denge Marsh Gut," eastward of which we have modern "fulls,"—the resultant in part of this waste, overlapping and adding to the south-east outline of the "Ness" or extreme projection of this natural mole of shingle, and thence travelling northward until reaching "Great Stone End," which forms the southern boundary of Romney "Hoy" or Bay, northward of which Dymchurch Wall, an artificial stone-faced sea-wall three to four miles in length, is sufficient evidence of the modern local waste and necessity for sea-defences to the rich grazing district of Romney Marsh. Still further northward the sea-wall recently constructed by the municipal authorities of Hythe is equally suggestive of this recession. Going no further back than Cole's survey of 1617, we have a *status quo ante* very nearly, as regards outline from Rye Bay to within two miles of the lighthouse, and this accompanied by a local south-east increase and movement around the lighthouse,

involving towards the end of the last century the removal of the old and erection of the present tower, which, due to this increase, is now very much in the same relative position as its predecessor of 1792. This local accumulation runs northward to "Romney Hoy," but is accompanied beyond this again northward by a constant struggle to preserve the sea frontage right up to Hythe.

Take the Royal Military Canal, twenty miles long, as the base of a triangle running out ten miles seaward therefrom, with two inclined sides of ten miles and fourteen miles, or twenty-four miles of sea margin in all; of this we have on the west side eight miles of stationary or receding shore, thence two miles to the "Ness," and northward of it four miles, or six miles in all of local increase, and northward again ten miles of stationary or receding shore.

Now under these conditions who is to fix, and on what principle, the landward boundary of what may be termed the "live beach," and is it not this very material (not grass-grown or covered as it ultimately becomes by vegetation) that lies most temptingly for removal?

A very tentative advertisement has recently been displayed at the Charing Cross terminal station of the South-Eastern Railway offering sea-beach or shingle for sale. J. B. REDMAN

6, Queen Anne's Gate, Westminster, S.W., May 28

Sheet Lightning

SOME people never see lightning; I have met one lady who cannot, and have heard of other instances. The question suggests itself, consequently, whether the duration of a flash of lightning is sufficient to produce a visible image on the retina or whether the image is only produced after successive reflections in the eye itself, which might be too few to produce such an image in the case of people with very dark eyes; if the latter is the case, this would go far towards accounting for the difficulty there is in deciding as to the character of sheet lightning as far as any optical test is concerned, but would indicate the possibility of further light being thrown on the subject by photography.

Ripon, May 24 N. W. TAYLOR

[WE *never* heard of any one (except blind people, of course) who could not see lightning, nor have we any idea how the colour of the iris (or reflection either) can have to do with it. As to the *duration* and visibility of a flash, see NATURE, vol. xxii. pp. 340-41. As to "summer lightning," the following statement from Prof. Tait's lecture on Thunderstorms (NATURE, vol. xxii. p. 438) may be of interest:—"I have said nothing of what is commonly called *summer lightning*, which is probably, at least in a great many cases, merely the faint effect of a distant thunderstorm, but which has also been observed when the sky appeared tolerably clear, and when it was certain that no thunderstorm of the ordinary kind had occurred within a hundred miles. In such cases it is probable that we see the lightning of a storm which is taking place in the upper strata of the atmosphere, at such a height that the thunder is inaudible, partly on account of the distance, partly on account of the fact that it takes its origin in air of small density." We know that Prof. Tait speaks from having himself seen what he here describes, which shows unquestionably that (*on some rare occasions*) its source is really above our heads, and not (as is the general rule) a thunderstorm of the ordinary kind several miles off.—ED.]

Curious Nest-building—"Scarecrows"

THERE is an old house at Whetstone at which a robin lately built its nest in a singular position. The gate in the garden wall, opposite the door, is opened from inside by a servant, when the bell rings. To do this she goes to a little hole in the wall close by the gate, and pushes along a small bell-pull handle in the line of the wall (the motion very slight). In the space behind this handle, and evidently scanty for the purpose, a robin built its nest, and it is now filled with little birds, which stretch out their yellow gaping beaks when one pushes the handle. The case is the more curious in that the master of the house, fearing inconvenience on both sides, had the nest twice removed when in course of building; but the robin persisted, and was ultimately allowed. A little way along from this gate is an old disused pump, the front of which opens on hinges. Round the vertical rod of this pump a tomtit has laboriously built up a pile of twigs and various scraps, quite filling the body of the pump for about a foot in depth; and on the top is the nest proper, with six or seven eggs. The handle is never moved by any one. The bird apparently enters at the hinge.

While on this topic I would ask, Has any systematic study been given to the question of scares for birds? I recently sowed some grass and clover seed on a lawn, and, to scare the sparrows, stuck up some bits of wood, with square pieces of paper, attached with string to flutter in the wind; but from the occasional position of sparrows on the lawn, I suspect the radius of action of these scares was decidedly limited. Are birds most scared by still effigies of persons, or by sight of movements apart from such imitation, or again by sounds, as in a scare I saw lately, where pieces of glass were hung so as to clink together?

Finchley, May 29 M.

Ground Ivy

I SHOULD like to know if a peculiarity I often see in the position of the stigmas in the pistillate form of this flower is generally observed. Instead of the stigmas opening up and down from the style as usual in Labiates, they often diverge to right and left across the flower, and the style also often curves forward, so as greatly to facilitate cross-fertilisation as it seems to me. If I am right this slight change may be of interest as a step towards diœciousness. I found this peculiarity in 291 out of 531 flowers with abortive stamens which I looked at; the stigmas opened in the usual way in 85, while in 141 one stigma was vertical and one horizontal; 14 cases were doubtful. In some unopened buds, the stigmas already diverged horizontally. For comparison I looked at 418 perfect flowers, and here, while the stigmas of 360 opened as usual, only 15 spread horizontally; 34 had one stigma vertical and one horizontal, and 9 cases were doubtful. S. S. DOWSON

Geldeston, May 22

Meteor

I HAVE just seen a very splendid meteor (at 10.40 p.m.). I watched it during about thirty seconds, in which time it traversed the heavens from about the point south-east nearly to that of north-west, where it burst. Its path was nearly parallel to the horizon, probably approaching it at an angle of about 5 degrees. When first seen it appeared nearly yellow as to colour, with a very fine tail, but just before it broke up the colour changed to white, and the fragments reminded me very much of some "magnesium stars" fired from a rocket. No doubt you will have a quantity of communications concerning this meteor. I wondered whether any one else had noticed this appearance.

Filston Hall, Shoreham, Kent, June 3 A. HALL

WASPS (L. C.).—Thanks; but there is nothing new in your observations.

MIMICRY (DR. KESTEVEN).—The occurrence is perfectly well known. It is probably *Urapteryx Sambucana*. You have mistaken the anterior for the posterior extremity.

RECENT ORNITHOLOGICAL WORKS [1]

TO those who imagine that British ornithology is worked out, and that there is nothing left to do in this well-worn field, we commend the study of the present book, as presenting us with a delightfully fresh view of an old and familiar subject. The author is already well known to the public from his admirable books of Siberian travel, but it is only his private friends who have been aware of the devotion to this favourite branch of science which has characterised Mr. Seebohm for many long years, when most people imagined him to be absorbed in business in the north of England. Brief excursions to points of interest on our own coasts, snatched in intervals of scanty leisure, succeeded in after years by more important expeditions to Greece and Asia Minor, the River Petchora, the Yenisei, &c., have given him an acquaintance with field-ornithology which is surpassed by few of his contemporaries, while the fact that the dry details of literary research have no terrors for him is proved by the

[1] "A History of British Birds, with Coloured Illustrations of their Eggs." By Henry Seebohm. Published by the Author, 1883.

masterly way in which he executed the fifth volume of the British Museum "Catalogue of Birds."

The present volume, however, will appeal to a class of readers very different from those who study the high and dry literature above-named, and even those accustomed to the well-written pages of Prof. Newton's edition of "Yarrell's British Birds," will find delight and instruction in the volume now issued by Mr. Seebohm.

The first part contains an account of the Birds of prey, and the Thrushes, and considerable novelty is introduced in the style of nomenclature of these two groups. First of all we notice that Mr. Seebohm gives up the idea of *Orders* in the class *Aves*. Although commencing with the Birds of prey, the time-honoured opening "Order Accipitres" is absent, and we are introduced to the family *Falconidæ* instead, and we consider that it is in the classification adopted and in certain points of the nomenclature that the weak spot lies in this otherwise admirable work.

Mr. Seebohm is the kind of man who would speak disrespectfully of the Equator! With unremitting energy he charges full tilt against what he considers the abuses of scientific nomenclature in the present day, and not content with heartily belabouring those who differ from him, he returns to the assault on every possible opportunity, "fights all his battles o'er again, and thrice he slays the slain." He is quite furious with the rules propounded by the Committee of the British Association, and rebukes the authors, promoters, and followers of these rules with unabated vigour, but with perfect sincerity, as is exemplified by the following sentence in his "Notice to Subscribers," where he writes:—"If I have criticised the work of any of my fellow ornithologists too severely, I ask their pardon, and hope that they will pay me back in my own coin by correcting my blunders with an unsparing hand. The object of all true scientific work is the elimination of error and the attainment of truth."

We can promise Mr. Seebohm that, as one of the authors most severely attacked in his volume, we shall accept the above challenge, and shall not hesitate to pay him back in his own coin when occasion arises, trusting to the strength of the late Marquis of Tweeddale's dictum, that it is "by the flails of disputation that the truth is threshed out." And yet this is not an easy book to criticise. There is so much that is elegant in the treatment of the subject, and the work is so evidently done *con amore*, that in reading it through one is apt to lose sight of the irritating attacks on one's own writings in the admiration which the general style of the book compels; nevertheless there are several points on which it is impossible to agree with the author.

To drop the idea of *Accipitres* as an Order, and treat the Birds of prey as a simple family, suggests that the author has only a limited acquaintance with this group in its entirety, and this is a failing which appears throughout Mr. Seebohm's work, viz. that he is apt to judge of the classification of birds from a knowledge of Palæarctic forms alone, without any consideration of the mass of birds which are extra-Palæarctic in their habitat. This remark would be perhaps unnecessary did not the author aim at such a high standard. Thus his families are provided with "Keys to the genera," which, as Mr. Seebohm is nothing if not seeking after natural affinities, may be supposed to give the author's matured opinion on the relations of the genera. We can only wonder, therefore, at the importance attached to the characters which ally *Falco* with *Vultur* (in the same primary section of the *Falconidæ*), and place the Ospreys as intermediate between the Falcons and the Swallow-tailed Kite. The Falcons and the Honey-kites are united by such forms as the Neotropical *Harpagus*, the Indo-African genus *Baza*, and other forms, but what *Pandion* has to do with any of them we fail to see entirely, and so far we have not seen any reason to modify our opinion expressed in 1874, that the Ospreys are co-ordinate with the Falcons and the Owls, and form an intermediate group between these two. We should have thought, too, that at least as good characters could have been found to separate *Neophron* from *Vultur*, as some of those employed by Mr. Seebohm for distinguishing other genera of his family Falconidæ.

In the much-vexed question of the Jer-Falcons Mr. Seebohm brings in his favourite theory of interbreeding, and accounts for the variation in plumage between the different races on this score with much ingenuity and some show of success, but we must totally dissent from his view of the Iceland Falcon being an intermediate form (*F. gyrfalco-candicans*). To our mind it is quite as good a race as the true Jer-Falcon of Scandinavia, and has a perfectly distinct habitat. In Greenland the case may be different, and it is by no means improbable that the resident Jer-Falcon of Southern Greenland, *Hierofalco hoelboelli*, Nob., sometimes crosses with the Arctic white Jer-Falcon (*H. candicans*), and that the result is seen in those specimens which are so numerous in collections, and whose exact specific position it is difficult to define; nevertheless fully adult birds, both of *H. candicans* and *H. hoelboelli*, are very easily recognised, but Mr. Seebohm's theory of hybridisation carries a strong probability.

In the article on the Peregrine Falcon the author sounds the first note of the trumpet which is to carry the charge into the enemy's lines and work havoc and destruction among the followers of the British Association rules of nomenclature. Mr. Seebohm asserts (and he is probably right) that the *Falco gentilis* of Linnæus, founded on Albin's Falcon Gentle, is absolutely the oldest-known name for the Peregrine, if the above rules are to be carried out to the bitter end. In the year 1767, a posthumous work by Gerini, who cuts a great figure throughout Mr. Seebohm's book, contained the name *Falco peregrinus* for the species, and as this is also the best known one, it is adopted by Mr. Seebohm as being that "auctorum plurimorum." By the simple process of using that name which has been employed by the majority of standard ornithological writers, the author settles all vexed questions as to priority, and does away with the difficulties of nomenclature arising from the discovery of a prior name in some long-forgotten "musty tome" by some diligent bibliographer. In the present case Gerini's book cannot be invested with the authority which Mr. Seebohm claims for it, because, as Prof. Newton has lately shown, the work was the result of the labours of three *collaborateurs* who published it in 1767, Gerini himself having died in 1751. The work is generally quoted by authors as the "Storia degli Uccelli."

We must candidly confess that Mr. Seebohm's plan of selecting the best known names for a species of bird has much to recommend it, and in the present volume the result is in general satisfactory, as it restores to many of the common European species the names by which they are most familiar to the general public. At the same time this rule of adopting the nomenclature *auctorum plurimorum* requires great care in its application, and it will probably be found to work better in the case of European birds than in the less-studied species of other countries. The whole subject is deserving of earnest thought, but for our own part we cannot entirely free ourselves from the idea that a certain amount of injustice will be done to the labours of many of the early writers in ornithology whose names have been overlooked by their successors, but who scarcely deserve to be passed over entirely, as their work might be up to the standard of knowledge of the times in which they lived. We cannot help seeing throughout Mr. Seebohm's volume that justice to the labours of the forerunners in ornithological science is *not* tempered with mercy to those who have endeavoured in all sincerity to fix the earliest recognisable names to the species of European birds. We must regret

that we have not space to give extracts from the many charming accounts of the habits of our English birds of prey, which have certainly not been surpassed by any modern writer. We have already alluded to the anomalous position given to the Osprey in Mr. Seebohm's classification, and we notice that in the characters which he assigns to the genus (p. 54) he does not refer to the skeleton, which is so essentially Owl-like in structure. The author calls attention to a very serious slip made by ourselves in the "Catalogue of Birds" with regard to the Rough-legged Buzzard (*Archibuteo lagopus*). We were certainly in error in placing this bird with the genus *Buteo*, and indeed the woodcut of the reticulated tarsus convicts us on the face of it; but we strongly doubt the correctness of Mr. Seebohm's relegation of the species to the genus *Aquila*, and we hardly think that Dr. Gadow's evidence as to the resemblance of certain points in the anatomy of the genera *Aquila* and *Archibuteo* was intended to suggest that they were closely enough allied to be considered inseparable. On p. 134 we are told that "ornithologists seem to have a fatality for making petty blunders." This probably accounts for Mr. Seebohm's admitting (p. 130) a woodcut of the nest of the Hen Harrier with the bird appearing in the background about the size of a Song Thrush. Perhaps Mr. Whymper, the artist who has drawn this otherwise pretty sketch, will, like Mr. Hanhart, who has done the plates of the eggs, "get better as he improves." (*Vide* the "Notice to Subscribers.")

Passing on to the family *Strigidæ* or Owls, we find with regret that Mr. Seebohm has once more ruthlessly destroyed the simplicity of nomenclature in the European species, and this on the authority of the "Storia degli Uccelli," whose fourfold authorship would surely be more than sufficient to place the book out of court. The genus *Aluco* is once more invoked for the Barn Owl, *Strix* is restored to the Tawny Owl as well as to the Long-eared Owl, Short-eared Owl, and Tengmalm's Owl, and the Snowy Owl and the Hawk Owl are placed in one genus, *Surnia*. This classification of the Owls is by far the most disappointing portion of Mr. Seebohm's book, and ornithologists will be inclined to view with suspicion the ideas of an author who, in endeavouring to upset the rules of the British Association, requires them to pin their faith to a system which would lead to such a result as is here offered to us. Gerini's "Ornithologia Methodice Digesta" may have gone down a hundred and forty years ago, but in the present day it appears to be "*Chaos, rudis indigestaque moles*," which the stomachs of the present generation of ornithologists will not be found strong enough to assimilate. A little woodcut is appropriately inserted as a tailpiece on p. 182, which represents the author coming to grief on a downhill path!

In the account of the *Passeridæ*, or Singing Birds, another suggestive tailpiece at once meets our eye at p. 199: it represents a peaceful scene on a river, and is probably placed there as emblematical of the joy of the author at finding himself once more in smooth waters. The rest of the volume is occupied with an account of the Thrushes and Warblers, Chats, Redstarts, and Flycatchers, with which birds Mr. Seebohm possesses an acquaintance beyond that of any of his contemporaries; and no one who reads his book will find fault with this portion of the work, which appears to us to be in every way excellent. We unhesitatingly express our opinion that since the time of Macgillivray no such original book as Mr. Seebohm's has been published on British ornithology, and, in spite of a few less satisfactory illustrations, we think that the figures of the eggs are by far the best that have yet been given. We have ourselves too often run counter to the rules of the British Association Committee to allow of a suspicion of our complete sympathy with these rules, and Mr. Seebohm has done much to prove their unworkable character in many instances, but at the same time his strong expressions with regard to some of their most conscientious supporters seem to us likely to lessen the respect with which many of his incontrovertible strictures would otherwise have been received.

Another most useful ornithological work has also just made its appearance in Mr. Eugene Oates's "Handbook to the Birds of British Burmah." [1] Although less ambitious in its scope than Mr. Seebohm's work above noticed, it is nevertheless a very complete *résumé* of the ornithology of the country of which it treats, and it forms one of those useful volumes which appear from time to time from the pens of hardworking ornithologists, which bring into one focus the results of many scattered essays in various journals. It must not be supposed, however, that Mr. Oates's work has been confined to the incorporation of the labours of his predecessors, for although he has gathered together into one compass the results of the travels of Mr. Davison and Capt. Bingham in Tenasserim, and of Capt. Wardlaw Ramsay in Karennee, the book is also enriched with an account of his own personal experiences during a fourteen years' residence in Pegu. One great characteristic of this book is its conciseness. In the present volume of 430 pages, four hundred species are disposed of, and yet the principal references are given, as well as descriptions of all the species. In fact, the book quite comes up to our idea of what a model "handbook" should be, and there is no doubt that it will be simply invaluable to the collector in British Burmah, within whose reach it is placed by the exceedingly modest price at which it is published. All workers in the field of Indian and Indo-Malayan ornithology will not be able to do without this most useful volume.

R. BOWDLER SHARPE

THE AURORA BOREALIS [2]

III.

THE "*Utströmnings*" *Apparatus*.—On the top of a mountain, or in a spot situated so high that it commands the surrounding country within a radius of some 5 kilometres, the apparatus, which I have termed an "utströmnings" apparatus, should be erected. This instrument consists of a copper wire, at least 2 mm. in diameter, laid out on insulators fixed on poles 2 metres in height, along which points or nibs of copper or brass are attached at every half metre in such a way that they always point upwards. The wire is, I believe, arranged with most advantage as shown in the subjoined Fig. 1. If the wire begins at o, and the distance oo′ is = 18 metres, the total surface area of the apparatus will be = 324 square metres. The letter *i* indicates insulator.

The length of the wire is, therefore, 194 metres, and the number of insulators, if one insulator is attached in the centre of each outer coil, = 27.

The insulators should be of a peculiar construction, so that they would, under all conditions, even when covered by hoar frost, be perfectly efficient. The kind shown in Fig. 2, based on the principle of M. Mascart's insulator, appears to me to be the most serviceable.

This diagram shows the vertical section of the insulator attached to the pole. *ab* is a glass tube 7 mm. thick, 5 cm. in diameter, and 20 cm. in height. This tube is soldered to the bottom of the jar *cdef*, the outer diameter of which is 11 cm., and height 13·5 cm., and is, at the side, 10 cm. from the bottom, provided with an opening o (2 cm. in diameter), which can be closed with a cork. Above the tube, *ab*, the bell, *mnn′* is affixed, which is provided with arms for the coiling of the wire. In the cork, *o*, a U-shaped glass retort, with short arms pointed downwards, is inserted, and if the retort *fede* is filled

[1] "A Handbook to the Birds of British Burmah, including those found in the adjoining State of Karennee," by Eugene W. Oates, Executive Engineer, Public Works Department of India (British Burmah). London: R. H. Porter, 6, Tenderden Street, W., and Dulan and Co., 1883.

[2] Continued from p. 109.

with sulphuric acid, the outer surface of the glass tube, *ab*, will be kept dry, and almost completely insulated. The distance between the jar and the bell should be as great as possible, in order that the hoar-frost may not form a bridge across the intervening space.

From this apparatus a telegraph wire is led on poles

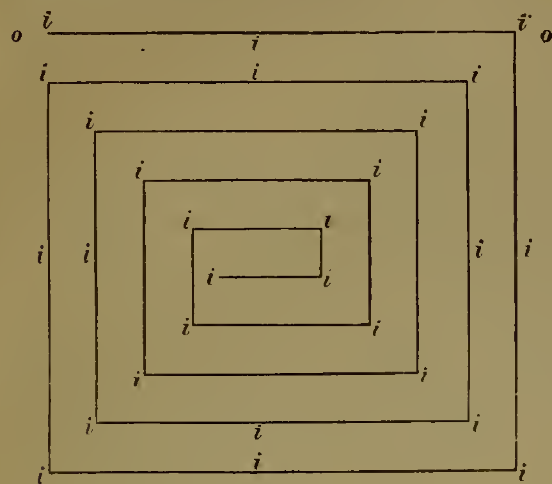


FIG. 1.

provided with insulators to a convenient chamber of observation. The conducting wire may, when the above-described kind of insulators is used, be an ordinary iron wire 2 mm. in diameter. The poles should be at most 40 metres apart.

The galvanometer should be constructed with a great

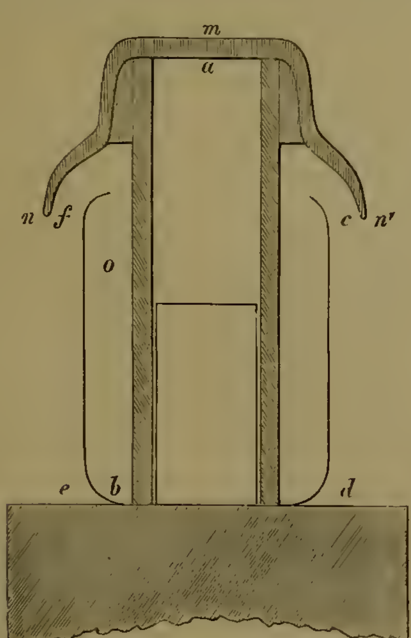


FIG. 2.

number (about 10,000) coils, and be provided with a pair of astatic needles, near which the mirror is affixed. In order to avoid too great oscillations, the needles should be hung side by side on fine threads of cocoon silk, the distance between the ends of the threads may be regulated according to circumstances. The readings should be made with a telescope and scale. It is besides clear that the conductive resistance of the galvanometer should be exactly measured, and that the readings of the instrument should be verified, as, for instance, with an inductor whose action on the galvanometer has been ascertained in absolute measure. For this purpose an ordinary Daniell's element may serve, and may in fact be the best, as a similar element should also be used for the actual measurements. If an iron wire is used it must of course be replaced with one of copper near the chamber of observation. The earth conductor of the current is a zinc disk about 4 square decimetres in area. The theory of the apparatus is this:—The entire quantity of electricity which is suffused in a certain part of the atmosphere which is situated above a certain horizontal plane, as that formed by the points in the "ütströmnings" apparatus, produces in each one of these points an *electromotive force*. And if the potential of these quantities of electricity on all the points be denominated as V_a, and the potential of the aggregate electricity on the zinc disk as V_z, the electromotive force E will be—

$$E = V_a - V_z,$$

and the strength of the current i

$$i = k\frac{V_a - V_z}{R},$$

where R denotes the whole conductive resistance, and k a constant dependent on the construction of the galvanometer, &c.

Generally, V_z is assumed $= 0$; but this is, in the present case, not correct; we therefore put—

$$i = k\frac{E}{R}.$$

If a constant element is introduced into the current, we have, if the electromotive force is denominated e, and the internal resistance of the element is not taken into account—

$$i_1 = k\frac{e}{R},$$

when i_1 means the intensity of the current which is created by the galvanic element.

If the positive pole is turned first against the "utströmnings" apparatus, we obtain—

$$i - i_1 = \delta,$$

and if the negative pole is turned against it—

$$i + i_1 = \delta',$$

i.e. if δ and δ' means the deflexion of the galvanometer in each case.

We obtain therefore in i a measure of $\frac{E}{R}$, and in i_1 a measure of $\frac{e}{R}$. If the deflexions are always reduced to the same value for $\frac{e}{R}$, which is easily done as e is constant, we obtain measures capable of being compared with E or $V_a - V_z$.

In the deflexions observed when a constant galvanic element is introduced into the circuit, one obtains, when the element is turned in the first instance with the positive pole against the "utströmnings" apparatus, and in the second against the earth-plate, a relative measure of the potential due to induction in the air on the particular occasion. From this it will appear that the observations should always be effected in the following manner:—

1. With the constant element in the current—
   (*a*) With the positive pole against the apparatus.
   (*b*) With the positive pole against the disk in the earth.
2. Without the element in the current—
   (*a*) First deflexion.
   (*b*) Constant deflexion.

3. With the constant element in the current—
   (a) With the positive pole against the apparatus.
   (b) With the positive pole against the disk in the earth.

The deflexions obtained will give the particulars required for an easy calculation of the strength of the current from the atmosphere to the earth.

I am fully aware that several details of this method may be open to discussion, but I do not deem others than the following of any great importance, viz. that as the intensity of the current is greatly dependent on the condition of the points, a gradual oxidation of the same will have the effect of causing an alteration in the current. This alteration also takes place in the strength of the current from the constant element, so that even the deflexions caused by the same will always be a measure of the aggregate potential due to induction, both through the points and in the air.

As it is not always possible to calculate the extent of the deflexions, an instrument permitting part of the current to be shunted should be employed. When the apparatus is erected care should be taken that the height between the disk in the earth and the apparatus is at least 180 metres; but experiments with disks at various elevations are of course of great interest.

From the account I have thus given of my experiments at Sodankylä, I think that all the subsidiary points which should be taken into account, as well as those questions which still await solution in connection with the aurora borealis, will be readily comprehended. It would, however, be of great advantage when making similar experiments to have two sets of apparatus; while thus measurements are being made with one, the variations in the current could be traced with the other, and thus the particulars requisite for a reduction to a fixed mean standard would be obtained.

SELIM LEMSTRÖM
Professor of the Helsingfors University

HISTORICAL NOTES IN PHYSICS

I.—*The Discovery of the Electric Light*

IN looking through an old volume of the *Journal de Paris*, I came across the following entry, for the date 22 Ventôse, An X. (March 12, 1802), which clearly relates to an exhibition of the electric arc light:—

"Le citoyen Robertson, auteur de la fantasmagorie, fait dans ce moment, des expériences intéressantes, et qui doivent sans doute avancer nos connoissances sur le *galvanisme*. Il vient de monter des piles métalliques, au nombre de 2500 plaques de zinc, et autant en cuivre rosette. Nous parlerons incessament de ses résultats, aussi que d'une expérience nouvelle qu'il a faite hier avec deux charbons ardens. Le premier étant placé à la base d'une colonne de 120 élémens de zinc et argent, et le second communiquant avec le sommet de la pile, ils ont donné, au moment de leur réunion, une étincelle brillante, d'une extrême blancheur, qui a été aperçue par toute la société. Le citoyen Robertson répétera cette expérience le 25."

The individual who thus came before the public was named Étienne Gaspard Robertson, a name suggestive of Scotch descent. He was better known for his "Phantasmagoria," exhibited a few years later in London. Of this invention a notice appears earlier in the volume from which the above passage is taken; and in an earlier volume of the *Journal de Paris* in the month "*Fructidor*, An viii.," there occurs a mention of some of his experiments on the *couronne de tasses* of Volta.

It is worthy of casual notice that in the number where Robertson's "Phantasmagoria" is advertised, the very next advertisement on the page is one of an exhibition to be given by Citoyen Martin at the Hôtel de Fermes, wherein as part of a "spectacle extraordinaire et amusant de physique," &c., was to be shown "l'expérience du télégraphie plus rapide que la lumière, d'un effet extraordinaire et amusant."

The usual date given for the invention of the electric light by Sir Humphry Davy is 1809; but I was aware that earlier notices existed both in Cuthbertson's "Electricity" (1807) and other works. I was also under the impression that some earlier reference to the matter existed in Davy's own works. The finding of this notice in the *Journal de Paris* induced me to consult the early volumes of the *Philosophical Magazine* and of *Nicholson's Journal*.

In the *Philosophical Magazine*, vol. ix. p. 219, under the date February 1, 1801, the following passage occurs in a paper by H. Moyes of Edinburgh, in which experiments with a voltaic pile or column are described:—

"When the above column was at the height of its strength its sparks were seen in the light of the day, even when taken with a piece of charcoal held in the hand."

In the *Journal of the Royal Institution*, vol. i. (1802), Davy describes (p. 106) some experiments on the spark yielded by the pile, and states: "When, instead of the metals, pieces of well-burned charcoal were employed, the spark was still larger and of a vivid whiteness." On p. 214 he describes and depicts an "apparatus for taking the galvano-electrical spark in fluids and aëriform substances." This apparatus consisted of a glass tube open at the top and having a tubulure at the side through which a wire tipped with charcoal was introduced, another wire, also tipped with charcoal, being cemented in a vertical position through the bottom.

But earlier than any of these is a letter printed at p. 150 of *Nicholson's Journal* for October, 1800. This letter is entitled "Additional Experiments in Galvanic Electricity, in a Letter to Mr. Nicholson." It is dated "Dowry Square, Hotwells, September 22, 1800," and is signed by Humphry Davy, who at that time was assistant to Dr. Beddoes at the old Philosophical Institution in Bristol. The letter begins thus:—

"SIR,—The earlier experimenters on animal electricity noticed the power of well-burned charcoal to conduct the common galvanic influence. I have found that this substance possesses the same properties as metallic bodies in producing the shock and spark,[1] when made a medium of communication between the ends of the galvanic pile of Signor Volta."

In none of these extracts, however, is anything said of the properties of the *arc* as a *continuous* luminous spark. These were made known in Davy's later researches. Yet the electric light attracted attention as we see before the special property of continuity was observed.

II.—*The Invention of the Telephone*

In the *Journal of the Physical Society of Frankfort-on-the-Main* for 1860–61 (p. 57) may be found a memoir on telephony by the galvanic current, in which its writer says: "I have now succeeded in constructing an apparatus by means of which I am in a position to reproduce the tones of divers instruments, and even to a certain degree the human voice." The inventor further says: "Since the length of the conducting wire may be extended for this purpose just as far as in direct telegraphy, I give to my instrument the name 'telephone.'" Towards the end of the memoir it is stated that until now it had not been possible to reproduce the tones of human speech with a distinctness sufficient to satisfy everybody: "The consonants are for the most part tolerably distinctly reproduced, but the vowels not yet to an equal degree." The author of the memoir in which these remarkable statements occur was Philipp Reis. The paper from which the preceding quotations have been taken contains many other points of interest, and in particular a com-

[1] Here Davy adds a footnote: "The spark is most vivid when the charcoal is hot."

parison of the action of the transmitting part of the instrument with that of the human ear upon which it was founded. The author says: "How could a single instrument reproduce at once the total action of all the organs operated in human speech? This was ever the cardinal question. At last I came by accident to put this question another way. How does *our ear* perceive the total (or

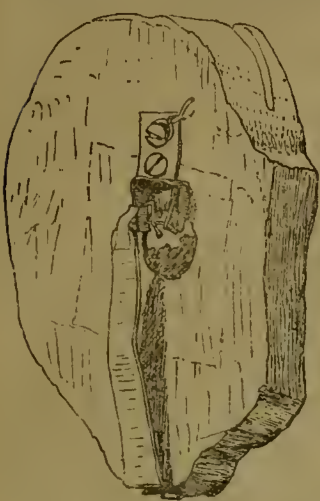


FIG. 1.

FIG. 2.

resultant) vibrations of all the simultaneously operant organs of speech?" He then goes on to describe the action of the auditory ossicles when made the recipients of sound-waves, and points out how they execute movements and exert forces upon one another in proportion to the condensations occurring in the sound-conducting medium and to the amplitudes of vibration of the tym-

FIG. 3.

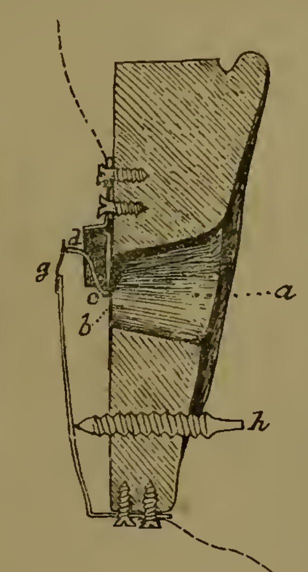


FIG. 4.

panum. Having stated this law of proportion between the cause and its effect, he goes on to speak of the graphic method of representing varying forces, such as those of sound-waves, by curves; and emphatically lays down that the ear is absolutely incapable of perceiving anything more than can be expressed by such a curve. After giving samples of undulatory curves corresponding to musical tones and to discordant sounds he makes the following significant remark: "So soon therefore as it is possible, at any place and in any manner, to set up vibrations whose curves are like those of any given tone or combination of tones, we shall then receive the same impression which the tone or combination of tones would have produced upon us. Taking my stand upon the preceding principles, I have succeeded in constructing an apparatus," &c. He concludes his paper by saying that the newly invented phonautograph of Duhamel may perhaps afford evidence as to the correctness of the views which he has asserted respecting the correspondence between sounds and their curves.

The actual apparatus figured in this memoir and exhibited to the Frankfort Society in October, 1861, is now in my possession; and I have also temporarily intrusted to me a still earlier experimental telephone made by Philipp Reis in the form of a model of the human ear.[1] This interesting instrument is depicted in its actual condition and size in Figs. 1, 2, and 3, and in section in Fig. 4. It is carved in oak-wood. Of the tympanic membrane only small fragments now exist. Against the centre of the tympanum rested the lower end of a little curved lever of platinum wire, which represented the "hammer"-bone of the human ear. This curved lever was attached to the membrane by a minute drop of sealing-wax, so that it moved in correspondence with

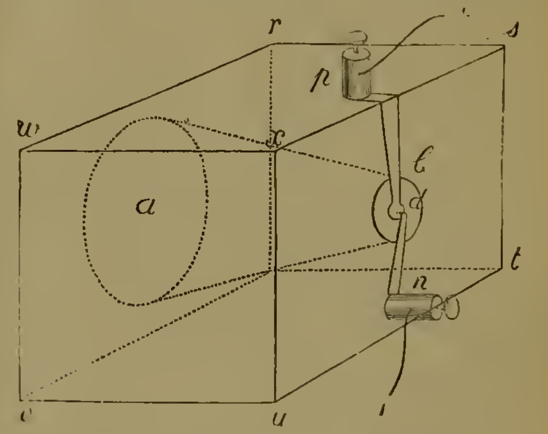


FIG. 5.

every movement of the tympanum. It was pivoted near its centre by being soldered to a short cross-wire serving as an axis. The upper end of the curved lever rested in loose contact against the upper end of a vertical spring, about 1 inch long, bearing at its summit a slender and resilient strip of platinum foil. An adjusting screw served to regulate the degree of contact between the vertical spring and the curved lever. Conducting wires, by means of which the current of electricity entered and left the apparatus were affixed to screws in connection respectively with the support of the pivoted lever and with the vertical spring.

If now any words or sounds of any kind were uttered in front of the ear, the membrane was thereby set into vibrations, as in the human ear. The little curved lever took up these motions precisely as the "hammer"-bone of the human ear does; and, like the "hammer"-bone, transferred them to that with which it was in contact. The result was that the contact between the upper end of the lever and the spring was caused to vary. With every rarefaction of the air the membrane moved forward, and the upper end of the little lever moved backward and pressed more firmly than before against the spring,

[1] The property of M. Léon Garnier, Director of Garnier's Institute at Fredrichsdorf, near Homburg, where Philipp Reiss was formerly Teacher of Natural Sciences.

making better contact, and allowing a stronger current to flow. At every condensation of the air the membrane moved backward, and the upper end of the lever moved forward, so as to press less strongly than before against the spring, thereby making a less complete contact than before, and by thus partially interrupting the passage of the current, caused the current to flow less freely. The sound-waves which entered the air would in this fashion throw the electric current, which flowed through the point of variable contact, into undulations in strength. Reis himself termed the contact-part of his telephone an interruptor. That it was not intended to operate as an abrupt make-and-break arrangement, as some persons have erroneously fancied, is evident; firstly, because the inventor introduced delicate springs to give a following-contact, and so prevent abrupt breaks from occurring; secondly, because abrupt breaks would have violated the fundamental principle to which he refers in the sentence immediately preceding his description of the instrument shown to the Frankfort Society, namely that of creating tones whose curves were like the undulatory curves imparted at the transmitting end of the instrument; thirdly, because (in another article) he described his instrument as opening and closing the circuit in proportion to the sound-wave, which obviously an abrupt "break-and-make" apparatus without a spring-contact could not possibly do. The mechanism which Reis thus invented —and which is substantially alike in all his instruments —might be appropriately described as the combination of a tympanum with an electric current-regulator, the essential principle of the electric current-regulator being the employment of a loose or imperfect contact between the two parts of the conducting system, those parts being so arranged that the vibrations of the tympanum would alter the degree of contact, and thereby vary the resistance offered at the point of contact to the passage of the current, and so regulate the strength of the current that it should magnetise and demagnetise the core of a distant electromagnet in a manner corresponding to the undulations of the tympanum of the transmitter.

The particular form of the instrument shown at Frankfort in 1861, and described in the *Journal*, is somewhat different from the "ear." The figure (5) and description are taken from the *Journal*.

"In a cube of wood, *r s t u v w x*, there is a conical hole, *a*, closed at one side by the membrane, *b* (made of the lesser intestine of the pig), upon the middle of which a little strip of platinum is cemented as a conductor of the current. This is united with the binding-screw, *p*. From the binding-screw, *n*, there passes likewise a thin strip of metal over the middle of the membrane, and terminates here in a little platinum wire, which stands at right angles to the length and breadth of the strip. From the binding-screw, *p*, a conducting-wire leads through the battery to a distant station."

In the original instrument there is also an adjusting-screw to regulate the contact, though this is not shown in the drawing.

The receiver used to reproduce the sounds transmitted by these telephones is also described in the memoir of Reis. It consisted of a steel needle surrounded by a coil of wire. This was at first set up for the purpose of increasing the sounds by resonance, upon the top of a violin; later it was mounted upon a pinewood box, to which still later a lid of thin pine was added against which the listener could press his ear. The sounds emitted by such a wire during magnetisation and demagnetisation were well known before, but to Reis is due the discovery that other tones than the natural vibration-tone of the wire could be electrically imposed upon it by the varying magnetising force of the current in the surrounding coil. Reis explained the reproduction of the transmitted sounds by supposing a magnetic attraction between the atoms of the steel wire to work synchronously with the fluctuations of the current. He later devised a different receiver in which an electromagnet was provided with an elastically mounted armature of iron which it threw into vibrations corresponding to those of the original sound-waves. With this apparatus and a transmitter with a small curved lever like that in the "ear," he was able (see Kuhn's "Handbuch der Angewandten Elektricitätslehre," 1866, p. 1021) not only to reproduce melodies with astonishing exactness, and single words as in speaking and reading (less distinctly), but even to transmit the inflexions of the voice expressive of surprise, command, interrogation, &c.

Considering how far these early researches were carried, it is remarkable that their historic value has been so greatly overlooked. SILVANUS P. THOMPSON

SQUALLS

IN a short calendar for the present year, issued by Dr. Gustavus Hinrichs,[1] are two interesting charts of the fronts of squalls passing over Iowa from north-west to south-east. He remarks that the lines between which ·5 inch and 1·0 inch of rain fell in one of these squalls on July 31, 1877, gradually diverge as the storm-front rolls down to the south-east, while bending more and more, so as strangely to recall the lines of equal timber in Eastern and Southern Iowa.

The lines showing the configuration of these squalls are very similar to those showing the shapes of the most extensive European squalls, and the almost complete parallelism between the chart of the squall above alluded to, and those[2] of the *Eurydice* squall, which traversed England on March 24, 1878, is worthy of the attention of meteorologists. Squalls of this description strictly deserve the name of "arched squalls" (apparently bestowed by English seamen on all squalls which are seen in perspective to rise as arches of cloud above the horizon), for when plotted out upon a chart they are found to be, at the period of their greatest development, *scimitar or crescent-shaped*, the central portion of the area of squall being in front of the right and left wings, and a chord of the arc so formed being commonly normal, or nearly so, to the isobars existing at the time. It seems probable that the most projecting portion of the line of clouds forming the front of the squall traverses the line of steepest atmospheric gradients, but of this no proof has yet been furnished. The strongest wind is, in any case, commonly experienced in the immediate front of the squall along the line traversed by the focus of the squall, and the greatest precipitation in the rear is also usually experienced along this line. There is one characteristic which the crescent-shaped squall shares with the very local squalls common in the temperate latitudes in the rear of a depression, viz. the violent down-rush of cold air experienced under the front of the squall-cloud, followed by a comparative calm in its wake. In the crescent-shaped squall there is a veering of the lower wind and a backing of the upper wind, sometimes to the extent of about eight points, which is not traceable in the more local squall. In the crescent-shaped squall a sudden increase of atmospheric pressure is experienced at the earth's surface at the time of the strongest rush of wind. This characteristic seems to be shared by the majority of squalls which occur in the Persian Gulf and in Northern India from the north-west, and also by many squalls in the Indian and China seas (which may possibly prove to be of the crescent-shaped type); and it is precisely analogous to the rise of barometer frequently noticed at inland localities in the temperate zones during a summer thunderstorm. It is, however, stated that in

[1] "Notes on the Cloud Forms and Climate of Iowa." Dr. G. Hinrichs, Director Iowa Weather Service.

[2] *Meteorological Magazine*, vol. xiii. p. 33; *Nautical Magazine*, vol. xlvii. 5.

the "tornadoes" of the Gold Coast (which are merely severe squalls) a fall of the barometer occurs. In some squalls, especially in the Indian and China seas, a change of wind occurs to nearly an opposite point of the compass, and in these instances there is sometimes a diminution of pressure [1] during the passage of the squall. These squalls I should regard as small typhoons.

The clouds which mark the front of an actual typhoon, as described by Dampier [2] and subsequent navigators, seem to be very similar to those which accompany the true squall, wherever observed. These consist of a dense curtain of ice-cloud in the higher regions of the atmosphere, usually permeated, except in the extreme rear, by mountainous cumuli from beneath, and having, when viewed at a distance, a very white and shining appearance. In the final stage of the squall, when it is diminishing in severity, these cumuli commonly disappear. A watchful outlook for these clouds, not least of all when coming off a high windward shore, may save many sailing vessels, as it might in all probability have saved the *Eurydice*, from destruction. W. CLEMENT LEY

[1] Schück. *Annalen der Hydrographie*, March, 1877; *Quart. Journ. Met. Soc.* vol. iv p. 78.

[2] "Voyages," ii. 36.

NOTES

As we anticipated some weeks ago (p. 41), Prof. Lord Rayleigh has been nominated by the Council of the British Association as President for the Meeting at Montreal in 1884. The death of the late Prof. H. J. S. Smith having caused a vacancy among the vice-presidents elected at Southampton for the meeting at Southport in the present year, the Council have nominated Dr. J. M. Dawson, C.M.G., F.R.S., Principal of McGill College, Montreal, to be a vice-president.

PROF. HUXLEY'S Rede Lecture will have for its subject "The Origin of the Existing Forms of Animal Life: Construction or Evolution?" It will be delivered in the Senate House (Cambridge) on Tuesday next at noon.

THE death is announced of M. Charles Bresse, on May 22, at the age of sixty-one years. He was, since 1855, Professor of Mechanics in the School of the Ponts et Chaussées, and also for the last few years at the École Polytechnique.

A SERIES of conferences will be held in connection with the Fisheries Exhibition, in which the foreign commissioners, jurors, and others connected with or visiting the Exhibition, will be invited to take part. The first meeting of the Congress will be held on Monday, June 18, at 12 noon, when Prof. Huxley will deliver an introductory address. H.R.H. the Prince of Wales, K.G., has graciously consented to read a paper by H.R.H. the Duke of Edinburgh, K.G., entitled "Notes on the Sea Fisheries and Fishing Population of the United Kingdom," on Tuesday, June 19, at 12 o'clock. At all other conferences, the chair will be taken by the appointed chairman at 11 o'clock a.m. precisely. Papers will be read, and discussions on them will follow. The conferences will be held on Mondays, Tuesdays, Thursdays, and Fridays.

THE Company of Grocers have announced as the matter of competition for the first quadrennial discovery prize of 1000*l.* the following problem:—"To discover a method by which the vaccine contagium may be cultivated apart from the animal body, in some medium or media not otherwise zymotic: the method to be such that the contagium may by means of it be multiplied to an indefinite extent in successive generations, and that the product after any number of such generations shall (so far as can within the time be tested) prove itself of identical potency with standard vaccine lymph." The prize is open to universal competition, British and foreign. Competitors for the prize must submit their respective treatises on or before December 31, 1886, and the award will be made not later than May, 1887. In relation to this prize, as in relation to other parts of the Company's scheme in aid of sanitary science, the Court acts with the advice of a scientific committee which at present consists of the following members:—Messrs. John Simon, F.R.S., John Tyndall, F.R.S., John Burdon Sanderson, M.D., F.R.S., and George Buchanan, M.D., F.R.S.

A CORRESPONDENT in West Australia writes to us that the Exploring Expedition to Kimberly District, North-West Australia, to which Mr. E. T. Hardman, of the Geological Survey of Ireland, is attached as geologist, reached Roebuck Bay, Kimberly (lat. 18°.10′ S., long. 122° E.) on April 9, after a favourable voyage from Fremantle of ten days; all well, with the exception of a native who, in a fit of delirium, jumped overboard and was lost. Mr. Hardman proceeds with Mr. John Forrest, Surveyor-General, a well-known and experienced explorer, on a preliminary examination of the district for some months, and will then accompany the main party about to make a trigonometrical survey of the country along the Fitzroy River traced in 1879 by Mr. Alexander Forrest. The party, which consists of thirty-two, all told, with fifty horses, left Fremantle in the *Macedon*, on March 25, but were shipwrecked on Rottnest Island, and subsequently went on in the steamer *Rob Roy*. The field-work will be continued until the middle of next November, and will probably be resumed next year. Previous explorers pronounce this district to be one of the best in West Australia.

WE have received a communication from Herr Sophus Tromholt, dated Bossekop, May 18, in which he informs us that his work at Kautokeino having been finished he has paid a visit both to the Finnish station at Sodankylä and the Norwegian at Bossekop. Herr Tromholt now intends to proceed to Bergen, and promises, when settled, to send an account to NATURE of his final researches on the aurora borealis. He states, however, that at neither of the above-mentioned stations has any photograph of the aurora been obtained. Next winter, Herr Tromholt informs us, he will spend in Iceland, in order to proceed with his studies of the aurora borealis there, chiefly on the principle laid down by Prof. Lemström, and with the apparatus invented by the latter.

SIR JOHN LUBBOCK has given notice that he intends on Friday three weeks to draw attention to the fact that the Minister whose duty it is to bring forward the Educational Estimates has no power to appoint officers, and to move that it is desirable that there should be a separate department of education.

IT is stated that M. Jules Verne, the world-known novelist, will offer himself as a candidate to fill the chair vacated in the Académie Française by the recent death of M. Jules Sandeau.

AN Exhibition of Hygienic Dress and Sanitary Appliances, intended to illustrate as far as possible the aims and objects of the National Health Society, was opened by the Lord Mayor on Saturday afternoon, in Humphreys Hall, Knightsbridge. The Exhibition, which will continue for a fortnight, includes clothing, food products, everything connected with the sanitation of the house and hygienic decoration, appliances for the sick-room, home nursing, and home education, industrial dwelling and cottage hygiene, heating, lighting, and cooking apparatus, fuel, &c. Perhaps, however, the greatest attention will be devoted to the stands of the Rational Dress Society, and another close to it, where are shown examples of ladies' dresses made on purely hygienic principles.

TOURISTS with entomological proclivities who may be about to visit the Alps, Pyrenees, Norway, or other parts of Europe, will find Dr. H. C. Lang's "Butterflies of Europe" (L. Reeve

and Co.) very useful. So far as it has gone, indeed, it is the best book on the subject in the English language. Part xiii., just published, brings the work down to the larger Fritillaries, and is one of the most satisfactory so far as the plates are concerned.

THE Russian Chemical Society having established a competition for the best lamps for burning the intermediate oils of the Caucasian naphtha, which have a density from 0·860 to 0·875, has found that the four competing lamps satisfy the required conditions, the best of them being that of M. Kumberg. According to experiments made by Prof. Mendeléeff, the new lamps burn not only the intermediate oils but also a purified mixture of all distillations, the heavy greasy oils which have a density of 0·910 at 15° included. Like the American naphtha, the Baku naphtha would thus yield more than two-thirds (nearly three-quarters) of its weight of oils available for lamps, the oils from this last being far less dangerous than those of the former. It yields, besides, nearly 30 per cent. of greasy oils of great value.

M. YANKOVSKY mentions the disappearance of the spotted deer from the neighbourhood of Vladivostok. Before 1877 they were so numerous that flocks numbering forty and fifty were often seen, and their meat was cheaper than beef. Since the snowy winters of 1877 and 1878, however, during which they were hunted on a great scale, they have become very rare. It seems that other causes too have contributed towards diminishing their number. In 1878, after a great fire which consumed the whole of the depression around the lake of the Slavyansky peninsula, M. Yankovsky saw the valley dotted with the bodies of deer and antelopes. It will be a pity if a succession of mild winters does not give an opportunity to the spotted deer of multiplying again, as their number is already very limited, and the region they inhabit is very restricted, as it comprises only the sea-coast from Corea to the Bay of Olga.

NOTWITHSTANDING the active pursuit of the tigers in the South Usuri region, their number does not much diminish. In a communication to the Irkutsk branch of the Russian Geographical Society it is stated that in 1880 and 1881 no less than nine tigers were killed on the small space of thirty-five miles long, on the western coast of the Bay of Amur; and at the head of this bay five tigers were perceived at one time. The zoologist of the Society, M. Yankovsky, writes also that the South Usuri tigers do not seem to abstain from eating corpses and digging out graves as is generally believed.

ON May 19, at about 10 p.m., a remarkable aurora borealis was observed at Ludvika, in Sweden. It began as a faint band of light parallel with the horizon, which gradually grew broader and broader. The extraordinary feature of the phenomenon was, however, that this band had the appearance of an ice-covered lake on which the moon was shining. Promontories and shores covered with trees were seen, and also the faint outlines of farms. This phenomenon lasted about ten minutes, when the aurora changed into a suffused pink luminosity, like that of clouds near the setting sun.

A STRONG earthquake was felt throughout the state of Antioquoia at 6 p.m. on the 8th ult. Little damage was done in Medellin, although much alarm was caused and the walls of the cathedral were injured. In the town of Antioquoia the façade of the cathedral was thrown out of plumb, many of its columns were thrown down, and all the houses suffered more or less. In Santa Rosa the church steeple was injured and a number of houses rendered uninhabitable. In Aquadas the town hall was destroyed, and at Abejirral the church and a number of houses were injured. It was feared more disasters had occurred in districts which had not been heard from. The shock lasted more than two minutes, and appeared to move from the north to the south. This same shock was felt all over the isthmus, all along the Atlantic coast of Columbia, doing damage only at the mouth of the Atrato, so far as reported up to the present, and in the Magdalena Valley. It appears to have been the sharpest and most widely experienced since the great one of September 7 last year.

TELEGRAMS from Batavia state that Mount Karang in the Straits of Sunda is in full eruption. The shocks are heard several hundred miles away. It is now two hundred years since the last eruption of this volcano. The mountain is situated on the island of Krakatoa, near Anjer in the Straits of Sunda, and as it is in the path of sailing vessels from Europe to the East, which generally call at Anjer point for provisions and orders, we may shortly expect details of the eruption.

A CORRESPONDENT writes:—During the last ten years much has been written on the origin of the jade objects found in America and Europe, no raw materials of the stone having yet been discovered out of which the articles could have been manufactured. Prof. H. Fischer of Freiburg in Baden therefore brought forward the hypothesis, supported by several of his scientific brethren, that the jade objects of America had been transported thither from Asia in prehistoric times, when Mongolian tribes settled in the New World, and that the intercourse of trade had later acted in the same manner. For Europe, where thousands of those objects have been found, the Aryans had done this service, when wandering from the very heart of Asia to the west, the source of the jade objects of both continents being Asia, where deposits of the mineral are known to occur in Siberia, Turkestan, and Burmah. Recently Dr. Meyer of Dresden has energetically opposed these views in a large folio work containing many plates, and has come forward with the opinion that the jade sources of Europe and America yet remain to be discovered. As to America we are glad to hear that this much simpler and more reasonable explanation of the problem has now been verified, the Smithsonian Institution of Washington lately having received from Louisiana an immense number of objects of jade, among them implements, knives, and other articles, many having an admirably high finish, and with them a considerable quantity of the stone of which the objects were made. We do not doubt that similar discoveries may soon be expected in Europe, especially in Switzerland, and that we shall succeed in ascertaining the exact districts where the mineral is to be found.

WE are glad to see that there is at last some prospect of the immediate publication of Mr. W. Colenso's Maori-English Lexicon, which was submitted to the New Zealand Government nearly eight years ago. A specimen sheet of twenty folio pages has recently been printed and presented to both Houses of the General Assembly by command of the executive authorities. From this specimen it is evident that the work is of an encyclopædic character, embodying a vast amount of information collected from original sources on the languages, ethnology, traditions, religions, habits, and customs of the Polynesian races. The plan is at once simple and comprehensive. The various meanings of each word are first given in large type, and each meaning is then illustrated by one or more passages in small type from the native poems, myths, legends, proverbs, and colloquial usage. Thus nearly four pages are devoted to the different significations and grammatical applications of the single word *a*, which plays such an important part in all the Polynesian dialects. To the particle *atu* as many as thirty distinct meanings are assigned, and these meanings are illustrated by no less than seventy-two quotations from the various sources above indicated. In some cases the quotations are Englished, and it would certainly be satisfactory if this could be done uniformly. In the English-Maori part the same plan is adhered to, only here quotations

illustrating the different senses of the English words are omitted as unnecessary. Should the work be carried out on these lines it will enable the student to wait somewhat more patiently for the appearance of Mr. Whitmee's long-promised Comparative Dictionary of the Polynesian Languages.

THE Minister for Postal Telegraphy will ask from the French Parliament the credits required for connecting by a cable Saigon to Haïfong, the principal seaport of Tonquin, and Haifong to Hanoi by another line laid down in the bed of the Red River.

AT the Polytechnic, which is now occupied by a Young Men's Christian Institute, there was recently an exhibition of drawings, and works of art and manufactures, executed wholly or in part by the members of the institute and the students at the numerous classes held there. Most of the exhibits show proofs of the usefulness and success of the institution. The exhibition included also many valuable works of art and a very costly and interesting collection of Japanese, Chinese, and Indian curiosities lent for the occasion by Mr. Quintin Hogg and other friends of the institute.

THE Oxford University Junior Scientific Club held a very successful *conversazione* in the University Museum on Friday evening last.

PROF. DEWAR, F.R.S., will give an experimental discourse on the Chemistry of the Electric Discharge at the last Friday evening meeting on June 8 at the Royal Institution.

THE additions to the Zoological Society's Gardens during the past week include two Pig-tailed Monkeys (*Macacus nemestrinus* ♂ ♀) from Sarawak, presented by His Highness the Rajah of Sarawak; an Egyptian Cat (*Felis chaus*) from India, presented by Mr. W. R. Glyn Griffiths; three Common Kingfishers (*Alcedo ispida*), British, presented by Mr. Frederic Houghton; a Barbary Ape (*Macacus inuus*) from North Africa, four Elliot's Pheasants (*Phasianus ellioti* ♂ ♂ ♀ ♀) from China, five Ceylon Terrapins (*Clemmys trijuga*) from Ceylon, four Bungoma River Turtle (*Emyda granosa*) from India, four Lacertine Snakes (*Cœlopeltis lacertina*), a Horseshoe Snake (*Zamenis hippocrepis*), a Pleurodele Newt (*Pleurodeles walti*), South European, a Red-legged Partridge (*Caccabis rufa*), European, deposited; a Buffon's Touracou (*Corythaix buffoni*) from West Africa, two Bronze-winged Parrots (*Pionus chalcopterus*) from South America, two Varied Hemipodes (*Turnix varia*) from Australia, two American Siskins (*Chrysomitris tristis*) from North America, two Black Larks (*Melanocorypha yeltonensis*) from Siberia, a Cerastes Viper (*Vipera cerastes*) from Egypt, purchased.

LOCAL SCIENTIFIC SOCIETIES

FOR some years past there has been a growing expression of desire of local scientific societies to be officially represented at the meetings of the British Association. The question is one of considerable difficulty and delicacy, and though it has been the subject of frequent discussion and some legislation, no measure has yet been carried that is satisfactory to all parties. Last year the subject was referred to the Council, who appointed a special committee, and this committee made on Tuesday its preliminary report. They asked in it for permission from the Council to circulate the report among the local societies in order to obtain from them that response which is needed before the committee can feel themselves in a position to report finally, and *a fortiori* before the Council can take their report into consideration. This permission has been granted, together with that of free publication. A copy of the report will consequently be shortly sent to the various societies by the secretary, Mr. H. George Fordham, Odsey Grange, Royston, Cambridgeshire, with the request that their replies will be forwarded to him. But as the subject presses, and as the season is advancing and the annual sessions of societies are drawing to a close, the best method of bringing the report before the members of those societies is through the columns of NATURE. I therefore forward it at once. FRANCIS GALTON

June 6

Preliminary Report of the "Local Scientific Societies' Committee, consisting of Mr. FRANCIS GALTON (*Chairman*), *the* Rev. Dr. CROSSKEY, Mr. C. E. DE RANCE, Mr. H. G. FORDHAM (*Secretary*), Mr. JOHN HOPKINSON, Mr. R. MELDOLA, Mr. A. RAMSAY, Prof. SOLLAS, Mr. G. J. SYMONS, *and* Mr. W. WHITAKER, *appointed by the Council in compliance with the following resolution referred to the Council by the General Committee:*

"That the Council be empowered to appoint a Committee, as recommended in their Report adopted by the General Committee on August 23, in order to draw up suggestions upon methods of more systematic observation and plans of operation for Local Societies, together with a more uniform mode of publication of the results of their work. It is recommended that this Committee should draw up a list of Local Societies which publish their proceedings."

The Committee have communicated with all the Societies known to them which appear to fall under the designation of "Local Societies which publish their proceedings," giving to this definition a somewhat liberal interpretation, and they submit a tabular list of the publications with other particulars of those which have furnished replies. These societies are about 170 in number, and seem from their rules and publications to be centres whence local scientific information may conveniently be obtained.

The Local Societies differ widely in character. Those which are established in large towns, and are not particularly well situated for carrying on systematic local investigations, are often of high scientific rank, and their affairs are administered in a business-like manner by a regular staff. On the other hand, there are numerous smaller societies and field clubs, scattered over the country, which are excellently placed for conducting local investigations, but whose organisation is so incomplete that it has often been difficult to discover their official addresses.

In some parts of the country the smaller societies either group themselves into what is practically a federation, or else affiliate themselves to some large society in their district, and the Committee think that if the Local Societies generally could be induced to group themselves round what might be described as local sub-centres, it would not be difficult to devise methods of uniting the representatives of those sub-centres in the performance of interesting and important duties during the meetings of the British Association, with the final effect of establishing systematic local investigation throughout the country, and uniformity in the modes of publishing the results. The recommendations the Committee are about to make will tend wholly in this direction, because, although they have considered many plans of fulfilling their instructions in a direct manner that perhaps look well on paper, no plan recommends itself to them as superior to this indirect method in its capacity of producing valuable and durable effects.

The Committee do not suggest any new topics for systematic investigation, but confine themselves to giving a few examples of what these topics are, taken from a circular printed last year by a committee appointed at a conference of delegates of scientific societies: (1) *Underground Waters* (to record the height of water in wells, and its variations in level in different parts of the country). (2) *Erratic Blocks* (to record their position, height above sea, lithological character, &c.). (3) *Underground Temperature* (to investigate the rate of its increase downwards in various localities). (4) *Rainfall* (its measurement). (5) *Periodical Natural Phenomena* (to record time of flowering of certain plants, arrival of certain migratory birds, appearance of certain insects). (6) *Injurious Insects* (to record their appearance in unusual numbers, the injuries they cause, and the degrees of success in preventing them). The first three of these investigations were set on foot by Committees of the British Association, and the last three by societies or private individuals.

It can hardly be doubted that numerous systematic investigations of a local character will from time to time be carried on, and that their successful prosecution would result in important gains to science. Neither does it appear doubtful

that the successful prosecution of such investigations by the smaller Local Societies would be greatly encouraged and facilitated by the general interest shown in their work by the more influential societies in their neighbourhood, by a watchful oversight, a readiness to discuss and publish results, and by the personal influence of their leading members. The Committee offer the recommendations they are about to make in the trust that, if the Council are pleased to publish them, they will serve to remind the more important Local Societies of the high and useful function they are able to perform by entering into friendly and helpful relations with the small and scattered societies of their respective districts, and by offering themselves as their scientific representatives wherever representation may be necessary.

The Committee recommend that they be empowered to print and circulate among the Local Societies the following draft of suggested rules, to give an opportunity to those societies of taking that initiative without which no action on the part of the Association is likely to produce much effect. After the Committee have been informed of the views of these societies, they will be in a better position than they are at the present moment for appreciating at its true value the desire for cooperation which they believe to exist. They will also perhaps receive useful suggestions from the societies that have not occurred to themselves, and they will probably be in a position to submit their final recommendations before the approaching annual meeting.

"SUGGESTED NEW RULES, THE EXISTING RULES BEING ALTERED ACCORDINGLY.

"*Corresponding Local Societies.*

"Application may be made by any society publishing scientific memoirs to be placed on the list of Corresponding Local Societies of the British Association. These applications must be addressed to the Secretary, and be made on or before the second day of the annual meeting, and they must be accompanied with a copy of the publications of the Society during the preceding year.

"The Secretary shall transmit these applications to a Committee appointed by the Council for the purpose of considering them, as well as for that of keeping themselves generally informed of the annual work of the Corresponding Local Societies. This Committee shall make an annual report to the Committee of Recommendations, and shall suggest such additions or changes in the list as they may think desirable; but the final determination of the list will rest with the Committee of Recommendations, subject only to the conditions—(1) That the number of Societies on the list shall not exceed that which is prescribed by the Council; (2) that the intended removal of any Society from the list shall not take effect until immediately before the commencement of the next annual meeting.

"The privileges of a Corresponding Local Society shall consist in—(*a*) The insertion in the Annual Report of the British Association of an index, in such abbreviated form as the Council may sanction, of the titles of the scientific memoirs published by the Society during the previous year; (*b*) the right to nominate any one of its members, who is also a member of the British Association, as its delegate to the annual meeting of the Association, who shall have for the time the rights of a member of the General Committee.

"Before the delegate can enter into his rights, he must transmit to the Secretary of the British Association a copy of the publications during the previous year of the Society he represents. He must also fill up a schedule, that will be furnished to him by the Secretary on application. This schedule will ask for—(*a*) The names of the President and chief executive officer of his Society; (*b*) a list of the institutions, if any, in its neighbourhood with which it has official relations and whose interests it represents; (*c*) a brief report on the character, number, and results of any systematic local observations carried on during the past year, either by itself or by any of the institutions on the foregoing list: (1) at the instance of Committees of the British Association, (2) at the instance of other Societies or private persons; (*d*) such other information as may be thought desirable.

"The delegates of the various Corresponding Local Societies shall constitute a Committee, which shall be summoned by the Secretary of the Association to hold one or more meetings during each annual meeting of the Association, under a Chairman and with a Secretary appointed by the Council. The Secretaries of each Section shall be instructed to transmit to the Secretary of the Committee of Delegates copies of any recommendations forwarded by the Presidents of Sections to the Committee of Recommendations bearing upon matters in which the cooperation of Local Societies is desired; and the Secretary to the Committee of Delegates shall invite the authors of those recommendations to attend the meeting of the Committee and give verbal explanations of their objects and of the precise way in which they would desire to have them carried into effect, and to discuss difficulties that may be raised by any member of the Committee, so that the Delegates may be qualified on their return to bring those recommendations clearly and favourably before the notice of their respective Societies."

The Committee believe that the distinction accorded to a Local Society through its selection and formal recognition by the British Association as one of its Corresponding Societies, the advantage of a widely-circulated notice of its work in so important a volume as the Report of the British Association, and the honourable and useful duties assigned to its delegate, would give considerable value to the title.

They also anticipate that a Local Society, which had asked for and received recognition as the representative centre for the time being of the institutions in its district, would be thereby stimulated to exercise that very creditable and important function with increased zeal and efficiency. The result would be to strengthen the mutual relations of the larger and the smaller Local Societies, to insure the encouragement of any disposition to engage in systematic investigations, and to establish a practice of printing the scattered results obtained by the smaller Local Societies of any district in a consolidated form in the publications of their leading Society.

Finally, the Committee believe that the annual meetings of the proposed Committee of Delegates, under the chairmanship of a distinguished member of the Association, would have large influence in harmonising the action of their several Societies, and that it would offer a facility that does not now exist for the natural and healthy growth of a federation between remote Societies which have no more direct bond of union than through the British Association.

THE ROYAL OBSERVATORY

THE following are the leading points referred to in the Report of the Astronomer Royal to the Board of Visitors of the Royal Observatory, Greenwich, read at the annual visitation on June 2.

On the subject of Astronomical Observations Mr. Christie says:—

"The regular subjects of observation are the sun, moon, planets, and fundamental stars, with other stars from a selected list. The working catalogue of 2500 stars down to the fifth magnitude having been cleared off, a new working list of 2600 stars, comprising all stars down to the sixth magnitude inclusive which had not been observed since 1860, has been prepared, and was brought into use at the beginning of March. About 1200 stars were observed in 1882, but amongst these there are nearly 500 single observations, necessitating careful comparison with catalogue place for the detection of any mistakes of observation or reduction. The labour thus entailed is considerable, and efforts will be made to obtain in this and each future year at least two observations of every star observed.

"The following statement shows the number of observations with the transit-circle made in the year ending 1883, May 20:—

Transits, the separate limbs being counted as separate observations	4488
Determinations of collimation error	354
Determinations of level error	323
Circle observations	4485
Determinations of nadir point (included in the number of circle observations)...	298
Reflection observations of stars (similarly included)	484

"Comet *a* 1882 has been observed seven times on the meridian since the date of the last Report, and Comet *b* 1882 has been observed three times.

"As regards the computations—

Clock times of transit over the true meridian after all corrections for instrumental errors are prepared to	1883, May 13
Clock errors and rates are determined to ...	May 5
Mean R.A.'s on 1883, January 1, are formed to...	April 25

"The investigation of personal equations has been completed for the year 1882, the results being very accordant with those found in the preceding year.

"The circle observations are completely reduced so as to form mean N.P.D. for 1883, January 1 to April 21, apparent Z.D.'s being formed to April 28.

"From the beginning of this year a correction of $-0''\cdot39$ has been applied to the results of the nadir observation to make them agree in the mean with the results of reflection observations of stars. This correction has been deduced from a comparison of the nadir results throughout 1822 with corresponding reflection results for stars north and south of the zenith. The discordance appears to be increasing, and its source has not yet been traced. It does not appear to originate on this occasion with the microscope-micrometer or telescope-micrometer, and it is not connected with the extension of the range of observation of stars by reflection. The discordance, which was insignificant in 1878, amounting only to $-0''\cdot03$, has gradually increased since, being $-0''\cdot10$ in 1879, $-0''\cdot29$ in 1880, $-0''\cdot30$ in 1881, $-0''\cdot39$ in 1882, and for the first four months of this year $-0''\cdot58$.

"Determinations of flexure have been made on 1882, December 30, and 1883, May 10 and 18, the resulting values being $-0''\cdot07$ and $-0''\cdot78$ and $-0''\cdot33$. The observations on May 18 were not altogether satisfactory, as the sun was shining during the second set of measures. The values resulting from the first and second sets respectively are $-0''\cdot72$ and $+0''\cdot05$. There is apparently nothing in the observations on May 10 to account for the exceptionally large value found on that day. No correction for flexure, as apart from the correction for R – D, has been applied to the observations.

"The correction for R – D, the error of assumed colatitude, and the position of the ecliptic have been investigated for 1882. For the planetary results, errors of R.A. and N.P.D. have been formed, but the heliocentric errors have not yet been computed.

"The reflection observations of stars available for investigation of the R – D discordance extend from Z.D. $71\frac{1}{2}^\circ$ north to Z.D. $70\frac{1}{2}^\circ$ south, and the discussion of these shows discordances steadily increasing from the zenith towards the horizon, and amounting to $-1''\cdot58$ for the group at Z.D. $68\frac{1}{4}^\circ$ north and to $+1''\cdot66$ at Z.D. 70° south, a correction of $+0''\cdot16$ sin Z.D. having been first applied to the reflection observations for inclination of the vertical at the mercury trough. It is quite evident that the discordances do not follow any such law, as $a + b \sin z \cdot \cos^2 z$, which was used from 1862 to 1880. Assuming the law $a + b \sin z$, which was adopted in the years preceding 1862 and in 1881, the R – D correction for 1882 would be $+0''\cdot07 + 0''\cdot42 \sin z$, and for the sake of continuity in the system of reductions this correction has been provisionally adopted for use in 1882. But the discordances between this formula and the observed quantities increase regularly from the zenith towards the horizon, amounting to half of the observed quantities at Z.D. 50° to 60°. The formula $+ 0''\cdot08 + 0''\cdot29 \tan z$ represents the observations better, though even this does not give sufficiently large results at large zenith distances. In this discussion corresponding reflection and direct observations made on the same day have alone been used.

"The value found for the colatitude from the observations of 1882 is 38° 31′ 21″·93, very slightly larger than the assumed value; the correction to the tabular obliquity of the ecliptic is $+ 0''\cdot44$; and the discordance between the results from the summer and winter solstices is $+ 0''\cdot37$.

"The mean error of the moon's tabular R.A. from observations with the transit-circle in 1882 is + 0·82s.

"The following observations have been made with the altazimuth from 1882, May 20, to 1883, May 20:—

Azimuths of the moon and stars	317
Azimuths of the azimuth-mark	228
Azimuths of the collimating-mark	216
Zenith-distances of the moon	176
Zenith-distances of the collimating-mark	214

"Azimuths and zenith-distances of Comet *b* 1882 were observed on a single day.

"The altazimuth observations are completely reduced to May 6, so as to exhibit errors of moon's tabular R.A., N.P.D., longitude, and ecliptic N.P.D. The restriction of the observations, and the limitation of the computations to 0·01s. and 0″·1 have made these reductions comparatively light.

"The moon's diameter has been measured—

With the transit-circle, twice in R.A., 17 times in N.P.D.
With the altazimuth, 4 times in azimuth, 10 times in Z.D."

On the subject of Equatoreals Mr. Christie states:—"A very valuable addition has been made to the instruments of the Royal Observatory by the gift of the Lassell two-feet reflecting equatoreal, which has been generously presented by the Misses Lassell. The exceptional qualities of this fine telescope (with which Hyperion was discovered in 1848) are well known, and there could be no hesitation in accepting on the part of the Admiralty the offer of such a valuable gift. The instrument was removed from Maidenhead early in March, and has been erected in the south ground, where it commands a nearly unobstructed view of the sky to within about 5° of the horizon. A circular building 30 feet in diameter, has been erected for the Lassell telescope, and the construction of a suitable dome is authorised. There are two large mirrors available for use, and I contemplate taking advantage of the firm mounting and perfect clock movement of the south-east equatoreal to mount the spare mirror on this instrument, attaching it to the tube of the refractor, so as to have on the same mounting a refractor and reflector with their axes parallel. The former would be available for eye observation, whilst the latter could be used on the same object for physical work, spectroscopic or photographic. The Lassell telescope itself would be well suited for observation of faint satellites and comets which are beyond our present instrumental means.

"The observations of the solar eclipse of 1882, May 17, with the south-east equatoreal are completely reduced, and the final equations have been solved.

"The spectroscopic observations during the past twelve months have been somewhat restricted through the pressure of the photographic reductions at a time of maximum of sunspot frequency. The solar prominences have been observed with the half-prism spectroscope on eight days, and four sun-spots have been examined on eight days with reference to the broadening of lines in their spectra. The spectrum of the great spot of 1882, November 12–25, showed some remarkable reversals of the lines of hydrogen and sodium, and an extraordinary displacement of the F line.

"As regards the determination of motions of stars in the line of sight, 142 measures have been made of the displacement of the F line in the spectra of 23 stars, and 26 measures of the b_1 line in 9 stars. The observations of Sirius during the past winter tend on the whole to confirm the impression that the rate of recession of this star has diminished progressively since 1877, and that the motion is now on the point of being converted into one of approach.

"The spectrum of Comet *a* 1882 was examined on three nights, that of the great Comet *b* 1882 on three nights, and that of Comet *a* 1883 on one night The spectrum of the first-named object showed the yellow sodium lines with great brilliancy just before perihelion passage. The spectrum of the aurora of 1882, November 17, was also examined.

"The spectroscopic observations of all kinds have been completely reduced to 1883, May 20.

"In the year ending 1883, May 20, photographs of the sun have been taken on 200 days, and of these 339 have been selected for preservation. There were 7 days on which the sun's disk was observed to be free from spots. The number and size of spots and faculæ continued to increase in a marked way till last November, when a group of spots of very unusual size appeared. Since that date, however, the sun has become more quiescent.

"Since the beginning of December, gelatine dry plates have been used instead of the old wet-plate process. They are more convenient in use, and appear to give as good average results. The photographs on a scale of 8 inches to the sun's diameter recently obtained in India, under the auspices of the Solar Physics Committee, are so successful that the Committee have recommended the general adoption of this scale, and I propose, as soon as we have a spare photoheliograph returned from the Eclipse Expedition, to have it altered in the same manner as the Indian photoheliograph, so as to obtain eight-inch photographs of the sun instead of four-inch.

"It was suggested in the last Report that the measurement of such of the Indian and other photographs as were required to fill up gaps in the Greenwich series might with advantage be undertaken here. This proposal has now been carried out, and 111 photographs for the period from 1881, December 22, to 1882, October 19, have been received from the Solar Physics

Committee, so that a record of the condition of the sun on 279 out of the 302 days in that interval is now presented. From 1882 October 20, eight-inch photographs were taken in India, and for the measurement of these a special micrometer has been ordered of Messrs. Troughton and Simms by the Solar Physics Committee.

"All the photographs received from the Solar Physics Committee have been measured in duplicate, and the measures have been completely reduced so as to exhibit heliographic longitudes and latitudes of spots, and areas of spots and faculæ, from 1881, December 22, to 1882, October 19, the end of the series of four-inch photographs.

"Magnetical Observations :—

"The course of observation continues the same as in former years, changes in the magnetic declination, horizontal force, and vertical force being continuously recorded by photography with the three magnetometers, whilst absolute values of magnetic declination, dip, and horizontal force are found by eye observation. Earth-currents in two directions nearly at right angles to each other are also photographically registered.

"A great improvement has been made in the photographic registration by the substitution in June last of Morgan and Kidd's argentic-gelatino-bromide paper with ferrous oxalate development for the old photographic process.

"The large temperature correction of the vertical force magnet has been reduced to less than one-fourth of its former amount by some alterations which were carried out by Mr. Simms last autumn. The effect of these alterations has been to reduce the correction for change of 1° Fahrenheit from 0·00088 of the vertical force to about 0·00020. The coefficient, has, however, still the opposite sign to that which would result from mere loss of magnetic power with increase of temperature. It is intended to make an attempt to still further diminish the temperature correction by shifting the magnet in its carrier so as to reduce the horizontal stalk and balance weight.

"It was remarked in the last Report that the earth-current registers frequently showed abnormal disturbance during rain. By the kindness of Mr. Lemard (the successor to the late Mr. C. V. Walker, as telegraph engineer of the South-Eastern Railway), the wires were repaired in February, and the rain disturbance seems now to have disappeared.

"The following are the principal results for magnetic elements for 1882 :—

Approximate mean westerly declination	18° 22′.
Mean horizontal force ...	3·913 (in English units). 1·804 (in metric units).
Mean dip	67° 33′ 33″ (by 9-inch needles). 67 34 34 (by 6-inch needles). 67 34 14 (by 3-inch needles).

"There has been considerable magnetic activity during the year, the month of November, which was characterised by the appearance of a very large sunspot, being particularly disturbed with remarkable magnetic storms on November 17, 19, and 20, and many interesting cases of lesser disturbance. The magnetical changes in November are so interesting in relation to the accompanying outburst of sunspots that it seems desirable to have the registers for a great part of the month as well as for other days of magnetic disturbance in the year lithographed in the 'Greenwich Magnetical Results for 1882' on a reduced scale. The character of a disturbance would, I think, be much better shown by a reproduction of the curves traced on thf photographic sheets than by tables of numerical values oe ordinates. I am making inquiries as to the practicability of using some anastatic process, which would not be very expensive.

"The magnetic disturbances on October 2 and November 17 were accompanied by brilliant auroras.

"Particulars of magnetic disturbances are regularly communicated to the *Colliery Guardian* newspaper for the information of mining surveyors.

"Meteorological Observations :—

"On the occasion of the gale of 1882, October 24, a velocity of 64 miles an hour was registered with Robinson's anemometer for two successive hours, being greater than any velocity previously recorded here, but the greatest pressure registered with the chain was only 29 lbs on the square foot, whilst on 1882, April 29, a pressure of 49½ lbs. was recorded with the copper wire at a time when the velocity was only 50 miles an hour.

"The observations of temperature of the Thames have recently been resumed under the charge of the Corporation of London, who have instructed Mr. G. J. Symons to arrange details. The observations are now made at the end of one of the jetties of the Foreign Cattle Market at Deptford, where a record is to be kept (by means of two Six's thermometers) of the daily maximum and minimum temperatures of the Thames at a depth of 2 feet below the surface, and also near the bottom of the river. Mr. Symons has arranged that these observations shall be regularly communicated to the Royal Observatory to be included in the meteorological table published weekly in the Registrar-General's Reports.

"The mean temperature of the year 1882 was 49°·6, being 0°·1 lower than the average. The highest air temperature was 81°·0 on August 6, and the lowest 22°·2 on December 11. The mean monthly temperature was above the average from January to May, then below until September. In October, November, and December it differed little from the average.

"The mean daily motion of the air in 1882 was 306 miles, being 27 miles greater than the average For the month of November the mean daily motion was 449 miles, being 159 miles above the average. The greatest daily motion was 758 miles on November 4, and the least 30 miles on December 11. As already mentioned, the greatest hourly velocity was 64 miles an hour, and the greatest pressure (with the chain) 29 lbs. on October 24.

"During the year 1882 Osler's anemometer showed an excess of 11 revolutions of the vane in the positive direction N., E., S., W., N., if all the turnings are counted (as has been the practice in former years); or of 23 revolutions in the positive direction if the turnings which are evidently accidental are excluded.

"The number of hours of bright sunshine recorded by Campbell's sunshine instrument during 1882 was 1245, which is more than 40 hours above the average of the 5 preceding years.

"The rainfall in 1882 was 25·2 inches, being very slightly above the average.

"The Westminster clock has maintained its high character, its errors having been under 1s. on 66 per cent. of the days of observation, between 1s. and 2s. on 25 per cent., between 2s. and 3s. on 6 per cent., and between 3s. and 4s. on 3 per cent. The error has never exceeded 4s.

Mr. Christie concludes as follows :—

"The changes suggested in the last Report have been carried out, and will, I trust, tend to increase the efficiency of the Observatory. The restriction of the altazimuth observations of the moon to the semi-lunation from last quarter to first quarter has enabled us to devote more attention to equatoreal observations, though the results hitherto obtained have been somewhat limited through the inadequacy of our instrumental means. The presentation of the Lassell telescope has now removed this difficulty, and when this fine instrument is in working order we may hope to be able to take up with success observations of comets, faint satellites, and other objects of interest. In regard to the spectroscopic observations we have now two observers available, and it may be expected that in the coming year we shall reap the full benefit of the arrangement by which Mr. Nash takes a share in this work.

"In solar photography we have undertaken the measurement and reduction of Indian photographs, supplementing those taken at Greenwich from the commencement of 1882. The Solar Physics Committee propose to undertake the arrears of this work for preceding years.

"In some slight degree the past year has been one of transition and of preparation for future work. Some administrative changes have been made, and the observers have been gaining experience in some new directions; but the regular course of observation and reduction has not been disturbed, and it has been my special endeavour to maintain the standard meridian observations in full vigour—a task in which I have received the hearty cooperation of all the staff.

"In regard to the coming year, I may mention one special work of meteorological reductions which it seems desirable to take in hand. The hourly ordinates of barometer and thermometer registers have been read out and tables of mean values formed for the 20 years of the meteorological reductions, and also year by year since 1877; but there is a gap of three years for the barometer (1874–1876), and of 8 years for the thermometer (1869–1876), for which the photographs have not been discussed. The continuity of the Greenwich series is thus broken, and the results are not available to their full extent. The dis-

cussion which I contemplate for the years in question would probably occupy one computer for a year and a half, involving an outlay of about 70*l*."

ON THE DARK PLANE WHICH IS FORMED OVER A HEATED WIRE IN DUSTY AIR[1]

IN the course of his examination of atmospheric dust as rendered evident by a convergent beam from the electric arc, Prof. Tyndall noticed the formation of streams of dust-free air rising from the summits of moderately heated solid bodies (*Proc. Roy. Inst.*, vol. vi. p. 3, 1870). "To study this effect a platinum wire was stretched across the beam, the two ends of the wire being connected with the two poles of a galvanic battery. To regulate the strength of the current a rheostat was placed in the circuit. Beginning with a feeble current, the temperature of the wire was gradually augmented; but before it reached the heat of ignition, a flat stream of air rose from it, which, when looked at edgeways, appeared darker and sharper than one of the blackest lines of Fraunhofer in the solar spectrum. Right and left of this dark vertical band the floating matter rose upwards, bounding definitely the non-luminous stream of air." . . .

"When the fire is white hot it sends up a band of intense darkness. This, I say, is due to the *destruction* of the floating matter. But even when its temperature does not exceed that of boiling water, the wire produces a dark ascending current. This, I say, is due to the *distribution* of the floating matter. Imagine the wire clasped by the mote-filled air. My idea is that it heats the air and lightens it, without in the same degree lightening the floating matter. The tendency, therefore, is to start a current of clean air through the mote-filled air. Figure the motion of the air all round the wire. Looking at its transverse section, we should see the air at the bottom of the wire bending round it right and left in two branch currents, ascending its sides, and turning to fill the partial vacuum created above the wire. Now as each new supply of air, filled with its motes, comes in contact with the hot wire, the clean air, as just stated, is first started through the inert motes. They are dragged after it, but there is a fringe of cleansed air in advance of the motes. The two purified fringes of the two branch currents unite above the wire, and, keeping the motes that once belonged to them right and left, they form by their union the dark band observed in the experiment. This process is incessant. Always, the moment the mote filled air touches the wire, the distribution is effected, a permanent dark band being thus produced. Could the air and the particles under the wire pass *through* its mass, we should have a vertical current of particles, but no dark band. For here, though the motes would be left behind at starting, they would hotly follow the ascending current, and thus abolish the darkness."

Prof. Frankland (*Proc. Roy. Soc.*, vol. xxv. p. 542), on the other hand, considers that what is proved by the above described observations is that "a very large proportion of the suspended particles in the London atmosphere consists of water and other volatile liquid or solid matter."

Last summer (1881) I repeated and extended Tyndall's beautiful experiment, not feeling satisfied with the explanation of the dark plane given by the discoverer. Too much stress, it appeared to me, is placed upon the relative lightening of the air by heat. The original density is probably not more than about 1/1000th part of that of the particles, and it is difficult to see how a slight further lightening could produce so much effect. In other respects, too, the explanation was not clear to me. At the same time I was not prepared to accept Prof. Frankland's view that the foreign matter is volatilised.

The atmosphere of smoke was confined within a box (of about the size of a cigar-box), three of the vertical sides of which were composed of plates of glass. A beam of sunlight reflected into the darkened room from a heliostat was rendered convergent by a large lens of somewhat long focus, and made to pass in its concentrated condition through the box. The third glass side allowed the observer to see what was going on inside. It could be removed when desired so as to facilitate the introduction of smoke. The advantages of the box are twofold. With its aid much thicker smoke may be used than would be convenient in an open room, and it is more easy to avoid draughts which interfere greatly with the regularity of the phenomena to be observed. Smouldering brown paper was generally used to produce the smoke, but other substances, such as sulphur and phosphorus, have been tried. The experiment was not commenced until the smoke was completely formed and had come nearly to rest. In some respects the most striking results were obtained from a copper blade about ½-inch broad, formed by hammering flat one end of a stout copper rod. The plane of the blade was horizontal, and its length was in the line of sight. The unhammered end of the rod projected from the box, and could be warmed with a spirit-lamp. The dark plane was well developed. At a moderate distance above the blade it is narrow, sometimes so narrow as almost to render necessary a magnifying glass; but below, where it attaches itself to the blade, it widens out to the full width, as shown in the figure.

Whether the heated body be a thin blade or a cylindrical rod, the fluid passes round the obstacle according to the electrical law of flow, the stream-lines in the rear of the obstacle being of the same form as in front of it. This peculiarity of behaviour is due to the origin of the motion being at the obstacle itself, especially at its hinder surface. If a stream be formed by other means, and impinge upon the same obstacle without a difference of temperature, the motion is of a different character altogether, and eddies are formed in the shadow.

The difference of temperature necessary to initiate these motions with this dark plane accompaniment is insignificant. On July 20, 1881, a glass rod, about ¼-inch in diameter, was employed. It was heated in a spirit-lamp, and then inserted in the smoke-box. The dark plane gradually became thinner as the rod cooled, but could be followed with a magnifier for a long time. While it was still quite distinct the experiment was stopped, and on opening the box the glass rod was found to be scarcely warmer than the fingers. It was almost impossible to believe that the smoky matter had been evaporated.

In order to test the matter more closely, smoke was slowly forced through a glass tube heated near the end pretty strongly by a spirit-lamp, and then allowed to emerge into the concentrated sunshine. No distinct attenuation of the smoke could be detected even under this treatment.

It is not necessary to dwell further upon these considerations, as the question may be regarded as settled by a decisive experiment tried a few days later. The glass rod before used was cooled in a mixture of salt and ice, and after wiping was placed in the box. In a short time a dark plane extending *downwards* from the rod, clearly developed itself and persisted for a long while. This result not merely shows that the dark plane is not due to evaporation, but also excludes any explanation depending upon an augmentation in the difference of densities of fluid and foreign matter.

The experiment was varied by using a U-tube through which cooled water could be made to flow. When the water was not very cold the appearances were much the same as with the solid rod; but when by means of salt and ice the tube was cooled still further, a curious complication presented itself. Along the borders of the dark plane the smoke appeared considerably brighter than elsewhere. Sometimes when the flow was not very regular it looked at first as if the dark plane had been replaced by a bright one, but on closer examination the dark plane could be detected inside. There seems no doubt but that the effect is caused by condensation of moisture upon the smoke due to the chilling which the damp air undergoes in passing close to the cold obstacle. Where the fog forms more light is scattered, hence the increased brightness. That the fog should not form within the smoke-free plane itself is what we might expect from the interesting observations of Aitken.

With respect to the cause of the formation of the dark plane, the most natural view would seem to be that the relatively dense particles are thrown outwards by centrifugal force as the mixture flows in curved lines round the obstacle. Even when the fluid is at rest a gradual subsidence must take place under the action of gravity; but this effect could at first only manifest itself at the top where the upper boundary of the gas prevents the

[1] Paper read at the Royal Society, December 21, 1882, by Lord Rayleigh, F.R.S., Professor of Experimental Physics in the University of Cambridge.

entrance of more dust from above. It is known that air in a closed space will gradually free itself from dust, but the observation of a thin dust-free stratum at the top of the vessel is difficult. If we conceive a vessel full of dusty air to be set into rapid rotation, the dust might be expected to pass outwards in all directions from the axis, along which a dust-free line would form itself. I have tried this experiment, but looking along the axis through the glass top of the vessel, I could see no sign of a dark line, so long as the rotation was uniform. When, however, the vessel was stopped, a column of comparatively smoke-free air developed itself along the axis. This I attributed to the formation of an inward flow along the top of the vessel, combined with a downward flow along the axis after the manner described and explained by Prof. James Thomson, so that the purified air had been in intimate proximity with the solid cover. It would almost seem as if this kind of contact was sufficient to purify the air without the aid of centrifugal force.

The experiments made hitherto in order to elucidate this question have given no decisive result. If the thin convex blade already spoken of be held in the smoke-box in a vertical instead of in a horizontal plane, the lines of motion are much less curved, and we might expect to eliminate the influence of centrifugal force. I have not succeeded in this way in getting rid of the dark plane; but since under the magnifier the curvature of the motion was still quite apparent, no absolute conclusion can be drawn.

ON THE MORPHOLOGY OF THE PITCHER OF "CEPHALOTUS FOLLICULARIS"[1]

THE brief, but most interesting, memoir on this subject read by Prof. Alexander Dickson before the Botanical Society of Edinburgh on March 10, 1881, was the first to throw any clear light upon the obscurity which had previously enveloped it. The conclusions at which he arrived seemed to be fully sustained by the facts which he then published; but since there are still botanists who do not fully accept those conclusions, any independent evidence bearing upon the problem of the morphology of these curious pitchers may be worth recording.

The publication of Prof. Dickson's memoir caused me to watch the growth of my plants of Cephalotus with increased interest. From time to time abnormal leaves have made their appearance, which seemed to afford more or less support to the views which the Professor entertained. This spring one of my plants has developed a leaf the growth of which I have watched. When this leaf first became visible it bore no indication of being other than an ordinary leaf of the plant, but its upper surface soon exhibited a somewhat shrivelled appearance, like that of a leaf distorted by the action of Aphides. It soon became evident that this disturbance was but the commencement of the process of pouching described by Prof. Dickson. That which at first appeared to be a mere distortion of the surface of the leaf soon deepened into a considerable depression, which became more considerable day by day until the leaf reached the condition represented in my figures 1, 2, and 3. Fig. 1 represents the upper, 2 the lateral, and 3 the inferior surface of this leaf.

From the beginning of its growth *a* was the unmistakable, somewhat cuspidate apex of the leaf, as it was also the distal end of the prominent ciliated ridge, *b*, the obvious precursor of the middle dorsal wing, which forms so conspicuous a feature of the normal pitcher. It will be seen in Fig. 2 that this ridge only extends downwards to the point *c*, whilst *d* was evidently the fundus of the enlarging pouch, relations which approximate closely to what characterise these portions of the perfect pitcher. On the under surface of the leaf (Fig. 3) we find this middle wing extending downwards from *a*, flanked on either side by a smaller, slightly curved ridge, also ciliated, the two unquestionably representing the lateral wings of the normal organism. It is perfectly clear that the peripheral outlines of the figures 1 and 3 represent the true primary margins of the leaf from the point *a* to the base of the petiole *e*. The upper half of this margin is abundantly ciliated, the hairs becoming more scanty as we approach the lower half of the leaf.

Figs. 1 and 2 show the form of what obviously represents the lid, *f*, of the true pitcher. In its essential features it accords with those figured by Prof. Dickson, who correctly recognised its true homology. As in his Fig. 5, this lid is two-lobed, its central indentation, *g*, separating two triangular lobes. This arrangement corresponds substantially with what exists in the normal pitcher, only in the latter the lobes are large and rounded instead of being small and triangular. The free margin of this rudimentary lid is abundantly ciliated, as in the perfect pitchers.

Thus far my specimen only confirms and illustrates the conclusions arrived at by Prof. Dickson, viz. that the pitcher is merely a depression in the upper surface of the leaf, of which the petiole *e* is identical morphologically with the terete petiole of the true pitcher, whilst the lid, *f*, is an outgrowth of the

FIG. 1.

FIG. 2.

upper surface of the leaf from the proximal margin of that depression.

Prof. Dickson was not able to decide with absolute certainty which part of the matured normal pitcher represented the primary apex of the leaf. In his abnormal specimens, as in mine, that apex coincided with the apex *a* of the middle dorsal wing. As is well known, in the perfect pitcher the entrance into the pitcher is bounded by a thick, involuted, toothed rim, to which the apical point of the dorsal wing is external. The Professor

FIG. 3.

was uncertain whether the apex *a* of the wing coincided with the true apex of the leaf, or whether that apex is hidden in the involuted margin of the pitcher. He inclines, however, towards the former view, and I have the conviction that he is right. The two *ciliated* margins, *a' a'*, of Fig. 1, are obviously the two lateral margins of the anterior portion of the normal leaf, demonstrating clearly that the point *a* is its apex. In the true pitcher these margins have lost their ciliæ, a few prominent teeth being substituted for them, and become thickened at their inner side

[1] By W. C. Williamson, LL.D., F.R.S., Professor of Botany in the Victoria University, Manchester.

by the development of the rounded and ribbed involuted border. It appears clear to me that this thick involuted structure is an outgrowth from the upper surface of the leaf, and which crossed the base of the cuspidate apex, *a*, without materially modifying it; and as it developed in a similar manner round the base of the distal surface of the lid *fg*, it contracted that base so as to reduce the attachment of the lid to the pitcher to very small dimensions. If the explanation is as correct as I believe it to be, the apex *a* of the middle dorsal wing is also the true apex of the leaf, whilst the involuted margin of the pitcher and the whole of its lid are equally outgrowths from its upper surface.

UNIVERSITY AND EDUCATIONAL INTELLIGENCE

OXFORD.—In a Convocation to be held in the Sheldonian Theatre on Wednesday, June 13, at twelve o'clock, it will be proposed to confer the degree of D.C.L. *honoris causâ* upon Lord Rayleigh, M.A., F.R.S., Professor of Experimental Physics and Honorary Fellow of Trinity College, Cambridge, and Sir Frederick A. Abel, K.C.B., F.R.S.

In a Convocation held on June 5 the following decree was submitted to the House:—"That the Curators of the University chest be authorised to expend a sum not exceeding 10,000*l*. in the erection of a laboratory, working-room, and lecture-room for the Waynflete Professor of Physiology, and in providing fixtures, warming apparatus, and gas for the same," which was carried by 88 to 85.

CAMBRIDGE.—The Geological Museum Syndicate recommend the combination of a new chemical laboratory with the Sedgwick Geological Museum, and believe that no better site will be available than the Downing Street frontage. They have asked permission to obtain plans and estimates in accordance with this proposal.

The Special Board for Medicine recommend the combination of the subjects of botany and comparative anatomy, now taken in the 1st and 2nd M.B. respectively, into one—elementary biology, which is to include much less than the two separate subjects. They propose that chemistry and physics in the 1st M.B. may be taken at a distinct period from the elementary biology, if candidates prefer it, and also that human anatomy and physiology in the 2nd M.B. may be taken at a distinct time from pharmacy and pharmaceutical chemistry. They also propose to discontinue classifying candidates, which has only been adopted of late years, and to publish merely alphabetical lists.

The university Local Lectures in populous centres have during the past winter, as usual, included numerous courses on science subjects, including chemistry and electricity, by Messrs. C. M. Thompson and S. L. Hart; Europe in Prehistoric Times, by Mr. J. E. Marr; Geology and Physical Geography, by Mr. W. W. Watts; Physics of the Earth, by Prof. Teall, &c.

LONDON.—On Tuesday afternoon last a large number of the friends of the medical education of women met at the Ladies' Medical College in Brunswick Square at the ceremony of presenting the prizes to the successful students of that institution. Countess Granville presided, and in the course of the proceedings Mr. Stansfeld, M.P., referred to the financial condition of the college as satisfactory. Two gentlemen from India spoke on the recent movement in Bombay to secure qualified medical women for that country, and referred to the immense value of a knowledge of medicine as an adjunct to missionary effort. Mrs. Garrett-Anderson, M.D., as Dean of the College, supplied some statistics of its progress, and Mrs. Fawcett, in proposing a vote of thanks to Countess Granville, dwelt on the assistance which the movement for placing a medical training within reach of women had received from Earl Granville in his capacity as Chancellor of the University of London.

WITH a view to encourage the study of veterinary science, the Lieutenant-Governor of Bengal has resolved to offer two prizes—one of 50*l*. and the other of 20*l*.—for competition by holders of Agricultural Scholarships from Bengal, studying in the Royal Agricultural College, Cirencester.

SCIENTIFIC SERIALS

THE *American Journal of Science*, No. 149, May, 1883.—Observations of the transit of Venus, December 6, 1882, at Princeton, New Jersey, and South Hadley, Massachusetts, by Prof. C. A. Young. Two sets of measurements of the planet's diameter, and some spectroscopic observations were made by the author and Mr. McNeill. During the transit 191 photographs were taken by Prof. Brackett and assistants. Of these 40 were first class, 30 worthless, the rest of all grades of excellence. The planet's atmosphere was seen by all observers at Princeton. But no satellite, spots, or marks were detected upon the planet's disk.—Notes on the occurrence of certain minerals in Amelia County, Virginia, by Wm. F. Fontaine. These have been brought to light during the excavations carried on for some years past for the purpose of obtaining mica. They are chiefly feldspar, beryl, fluorite, columbite, garnet, orthite, microlite, monazite, and helvite.—On the surface limit or thickness of the continental glacier in New Jersey and the adjacent States, by J. C. Smock.—Contributions to the geological chemistry of Yellowstone National Park, by F. Leffmann and W. Beam.—Notes on American earthquakes, with records from June, 1879, to end of December, 1882, by Prof. C. G. Rockwood.—A four years' record of earthquakes in Japan, studied in their relation to the weather and seasons, by Dr. Thos. H. Streets, U.S. Navy. The shocks are tabulated, with remarks on the state of the barometer and temperature. Three charts show the relation between the height of the barometer and the earthquakes.—Observations on the fossils of the metamorphic rocks of Bernardston, Mass., by R. P. Whitfield.—On De Candolle's "Origin of Cultivated Plants," with annotations upon certain American species, by Asa Gray and J. Hammond Trumbull (part ii.).

Annalen der Physik und Chemie, 1883, No. 5.—Experimental researches on the elliptical polarisation of light by reflection from surface-coloured bodies, with ten illustrations, by Julius Merkel.—A new radiometer, described and figured by C. Bauer.—The radiation of rock-salt under various temperatures, by the same author.—On the generation of heat in the absorption of gases by solids and fluids, with illustration, by P. Chappuis.—Some remarks on the action of air condensed on glass surfaces, by W. Voigt.—On the theory of the longitudinal impact of cylindrical rods, by the same author.—Observations on the action of quicksilver drops falling in thermometrical tubes, by Paul Volkmann.—On the galvanic resisting-power of psilomelan, by Hugo Meyer.—Remarks on W. Siemens' theory of luminosity, by W. Hittorf.—On a hitherto unrecorded phenomenon acccompanying electric discharges, with three illustrations, by Heinrich Hertz.—On the action of platinum, palladium, gold, coal, and aluminium in nitro-muriatic acid, by Carl Fromme.—On the dynamometrical method of determining the ohm, by J. Fröhlich.—On the measurement of local variations in terrestrial magnetic horizontal intensity, by F. Kohlrausch.—Researches in the electromagnetic phenomena of rotation, with three illustrations, by Friedrich Koch.—Experiments in connection with the theory of the Nobili-Guébhard rings, by W. Voigt.—Measurement of the diminution of sound in the telephone, by K. Vierordt.—On electric undulatory movements, with illustration, by A. Overbeck.—On the selective absorption of solar energy, with two plates, by S. P. Langley.—Remarks on C. Bohn's treatise on "Absolute Dimensions," by Paul Volkmann.—An account of Foucault's experiment with the pendulum, by A. Schuller.

No. 6.—On the measurement of the refractive relations of coloured fluids, with four illustrations, by C. Christiansen.—On the determination of the power of emission and absorption of heat in bodies, by the same author.—Observations on Norman Lockyer's theory of dissociation, by Hermann W. Vogel.—Researches on the variation of temperature in the pole-plates of a voltameter during the transmission of electric currents, with two illustrations, by E. Edlund.—Carl Fromme's electrical investigations (continued): Experiments on the condensation and absorption of hydrogen by platinum and palladium; *résumé* and further explanation of the results contained in the two previous sections.—Remarks on A. Kundt's treatise on "The Optical Action of Quartz in the Electric Field," by W. C. Röntgen.—On some experiments with static electricity, with numerous illustrations, by V. Dvorák.—Some remarks on the unipolar conduction of solid bodies, by F. Braun.—On the elliptical polarisation of the heat rays reflected by metals, by H. Knoblauch.—On the fluorescence of the vapour of iodine, by E. Lommel.—On the thermodynamic equilibrium of vapour mixtures, by Max Planck.—On some modifications of the pyknometer, by G. W. A. Kahlbaum.—On the selective absorption of solar energy (continued), with fresh observations on the invisible prismatic spectrum, by S. P. Langley.

THE *Beiblätter* to part 4 contains papers on the necessity of introducing certain modifications into the study of mechanics, and eliminating diverse problems from them, by Yvon Villarceau.—On the influence of temperature on manifestations of molecular energy, by A. Millar.—On the inner pressure and energy of superheated vapours, by G. Schmidt.

Journal de Physique Théorique et Appliquée, April, 1883.—Methods for determining the ohm, by Marcel Brillouin.—On the solidification of phosphorus and other substances in superfusion, by M. D. Gernez.—On the theory of colourless curves in double refractive crystals, by J. Macé de Lépinay.—A new hygrometer condensing internally, described and figured by A. Crova.—A new electrocapillary translator, described and figured by E. Debrun.—On the reading of a system of two electrodynamic machines, by A. Potier.

Revue Internationale des Sciences, February, 1883, contains articles:—On the contagiousness of tubercle.—On the Khouds, by Élie Reclus.—On the dangerous properties of finely-divided coal-dust, by Prof. Abel.—On the adulteration of food in Paris, by M. Egasse.—Medical anthropometry from the point of view of aptitude for military service, by M. Jansen.—Proceedings of the Academy of Sciences, Paris.

Journal of the Asiatic Society of Bengal, Vol. li. Part II, No. 4, 1882, contains:—On a new species of Hipparchia from the North-West Himalayas, by Major G. F. L. Marshall.—Notes and drawings of the animals of various Indian land mollusca, by Lieut.-Col. H. H. Godwin-Austin (Pl. 5).—Some further results of sun-thermometer observations, with reference to atmospheric absorption and the supposed variation of solar heat, by H. F. Blanford.

THE *Archives des Sciences Physique et Naturelles* for March contains papers by M. Schneebeli, on the determination of the absolute capacity of some condensers in electromagnetic measurement; by Raoul Gautier, on the great comet of September, 1882; by Dr. Julius Maurer, on the theory of the atmospheric absorption of solar radiation.—M. Casimir de Candolle has an interesting biographical notice of the eminent naturalist Emile Plantamour.

SOCIETIES AND ACADEMIES

LONDON

Royal Society, April 19.—"On the Limiting Thickness of Liquid Films." By A. W. Reinold, M.A., Professor of Physics in the Royal Naval College, Greenwich, and A. W. Rücker, M.A., Professor of Physics in the Yorkshire College, Leeds.

The previous investigations of the authors have shown that the specific electrical resistance of a soap film thicker than 374×10^{-6} mm. is independent of the thickness, and that the composition of films formed of M. Plateau's "liquide glycérique" may be largely altered by the absorption or evaporation of aqueous vapour which attends even slight changes in the temperature or hygrometric state of the air (*Phil. Trans.* Part II. 1881, p. 447).

In the present paper they describe a modified form of the apparatus which they previously employed. The glass case in which the films are produced is surrounded by water, and additional precautions are adopted for maintaining the aqueous vapour within it at the tension proper to the liquid of which the films are formed. These changes have entailed considerable alterations in details, but the main features of the apparatus remain unaltered. The new form, however, possesses the important advantage that the temperature and hygrometric state of the air in contact with the films can be kept perfectly constant during the progress of the experiments. With this apparatus a number of measures have been made of the electrical resistance of films which have thinned sufficiently to show the black of the first order of Newton's rings. To deduce the thickness from the resistance, it is necessary to assume that the specific resistance of the films is the same as that of the liquid in mass. The authors' previous experiments do not enable them to assert the truth of this assumption for such thin films, and it was therefore important to ascertain by an independent method whether it might be taken as approximately true.

For this purpose between fifty and sixty plane films were formed in a glass tube 400 mm. long and 18 mm. in internal diameter. The tube was closed by pieces of plate glass and placed in the path of one of the interfering rays in a Jamin's "interferential refractometer." When the films had become black, a known number were broken by bringing an electromagnet near to the tube and thus moving some sewing needles which had been inclosed along with the films. The mean thickness of the films was deduced from the displacement of the interference "fringes" caused by their rupture.

Two liquids were observed, viz. M. Plateau's "liquide glycérique," and a soap solution containing no glycerine. The following are the means of the various groups of observations:—

Liquid.	Method.	Mean thickness in terms of 10^{-6} mm.
"Liquide glycérique" ...	Electrical... ...	11·9
	Optical	10·7
Soap solution	Electrical... ...	11·7
	Optical	12·1

The agreement between these numbers is sufficiently close to make the fact that they are approximately correct unquestionable, and to prove that the mean thickness of a black film is nearly the same for both liquids.

The electrical observations afford a means of comparing the thicknesses of different black films and observing whether or not the thickness of the black portion of any particular film alters as its area increases. The results obtained in the paper and in a previous preliminary investigation on the same subject (*Proc. Roy. Soc.*, 1877, No. 182, p. 334) are summed up by the authors as follows:—

(1.) Persistent soap films which thin sufficiently to exhibit the black of the first order of Newton's rings invariably display an apparent discontinuity in their thickness at the boundary of the black and coloured portions.

(2.) The whole of the black region, at the time of or very soon after its formation, is of uniform thickness.

(3.) This thickness remains unaltered in any film, whether the coloured parts of the film are thinning or thickening, increasing or diminishing in extent.

(4.) It is different for different films, but no connection has been traced between its magnitude and the time which elapses between the first formation of the film and the first appearance of the black, or between either of these and the time of observation.

(5.) The mean values of this thickness are the same to within a fraction of a millionth of a millimetre, whether the films are plane or cylindrical, in contact with metal or with glass, formed of soap solution alone, or with the addition of more than two-fifths of its volume of glycerine.

(6.) Two totally independent methods of measuring the thickness of the black portions of the films give completely concordant results.

(7.) The mean value of the thickness calculated by giving equal weight to the results of the electrical and optical experiments is $11·6 \times 10^{-6}$ mm. The extreme values formed were $7·2 \times 10^{-6}$ and $14·5 \times 10^{-6}$ mm.

The smaller of these quantities is therefore a limiting thickness to which a soap film in air saturated with the vapour of the liquid from which it is formed rarely attains, and below which none of the films observed by us have thinned.

Linnean Society, May 24.—Anniversary Meeting.—Sir John Lubbock, Bart., president, in the chair.—Mr. R. McLachlan read for the Audit Committee the statement of receipts and payments for the year; 750*l.* had been invested, and a balance at banker's (April 30) remained of 514*l.* 8*s.* 7*d.*—The Secretary (Mr. B. D. Jackson) read his annual Report. Since the last anniversary 11 Fellows and 1 foreign member had died and 11 withdrawn, while 54 new Fellows had been elected. Between purchase, exchange, and donations, 407 volumes and 442 separate parts had been added to the library.—Mr. G. J. Romanes, on behalf of the subscribers, formally handed over the portrait of Charles Darwin, painted by Mr. J. Collier, its exhibition at the Royal Academy last year having then prevented its presentation.—A bust of the late Prof. Louis Agassiz by the American sculptor, Mr. Hiram Power, was handed over by Prof. Allman to the Society as a present from the sculptor's son, Mr. H. Power of Florence.—An engraving from Gainsborough's painting of the old English naturalist, Mr. Thomas Pennant, was presented by Mr. Howard Saunders in the name of Mrs. Alston, as a bequest from her son, the Society's late secretary, Mr. E. R. Alston.—The President then delivered his anniversary address, commenting generally on the events of the past year, with special reference to their bearing upon the Society; in congratulating

the Society on its annual balance-sheet, he reminded the Fellows that, besides investments, the property of the Society might be valued at 25,000*l.*, or a total of 30,000*l.*; he alluded to colonial Fellows and the good work they are doing, incidentally referring to the British Association meeting in Canada in 1884. Reference was also made to the progress of rearrangement of the Biological Collections in the Natural History Museum at South Kensington; this was followed by reports on the various botanical and zoological publications issued at home and abroad during the last twelvemonth. Remarks were made on the stock of the Society's Journals and Transactions, also on the purchase of a portrait of Jacob Bobart (1598–1679), and the President himself presented a valuable portrait of Linnæus painted from life, by the Swedish Magnus Hollman.—A resolution was unanimously accorded by the Society, at the instance of the Chair, to Mr. G. Bentham and Sir J. D. Hooker on the completion of their great work, the "Genera Plantarum."—The scrutineers having examined the ballot, then reported that Mr. Thomas Christy, Mr. H. E. Dresser, Mr. G. Murray, Mr. H. Saunders, and Mr. H. T. Stainton had been elected into the Council in the room of Mr. H. W. Bates, Mr. G. Busk, Mr. C. B. Clarke, Sir John Kirk, and Mr. R. McLachlan, who retired; and for officers, Sir J. Lubbock as president, Mr. Frank Crisp as treasurer, and Mr. B. Daydon Jackson and Mr. G. J. Romanes as secretaries.

Physical Society, May 26.—Prof. Clifton in the chair.—Mr. G. Griffith read a paper on the graphical representation of musical intervals, in which he gave an account of previous attempts to represent musical intervals in a graphical manner, and exhibited an enlargement of a diagram published by Dr. Pole in Sir F. Ouseley's "Treatise on Harmony." In this diagram the musical intervals contained in one octave are represented by the differences between the logarithms of the vibration-numbers forming them. Mr. Griffith proposes to apply this principle to the whole musical scale. Retaining the lines used in ordinary music he inserts a faint line between these at unequal distances to represent the tones and semitones. Several diagrams were exhibited, in which the principle was applied to the representation of intervals to the sequence of the keys in the major diatomic scale, and to actual music. Mr. W. G. Blakely and Dr. Coffin considered that it would be a great help to students to have the method proposed. Mr. Blakely considered that it combined the advantages of the tonic solfa and ordinary notations. Dr. Coffin thought that it might become generally used.—A paper by Dr. J. Fleming on a phenomenon of molecular radiation in incandescent lamps. When the carbon filament in an Edison lamp volatilises, the vapour is condensed on the glass in a cloud. When the copper electrode is volatilised, the copper is likewise deposited, but there is a bare space or line left on the glass in the plane of the filament, forming as it were a shadow of the filament. Dr. Fleming explains this on the supposition that the copper particles are thrown off in straight lines, as in a Crooke's vacuum. This shadow is not noticed in the carbon deposits. Dr. Fleming also remarks that the colour of a thin copper couch is the same as a thin layer of gold in transmitted light.—Mr. W. Baily read a paper on an illustration on the crossing of rays. He took the case of three rays of homogeneous light of the same intensity, and parallel to one plane, and polarised so that the vibrations were also parallel to the plane, and he exhibited and explained diagrams showing the motion which would occur under the circumstances.—Prof. F. Guthrie exhibited one of Chladin's plates bearing a striking resemblance to one of these figures. Mr. Baily thought the analogy might be a real one.—Prof. Clifton described an improvement which he had made in the glass insulating stem he had exhibited to the Society on a former occasion. This stem had a glass cup encircling it, and of a piece with the stem. Sulphuric acid was put into the cup. The new pattern had a hole formed into the bottom of the cup, and the upper part of the stem fitted into this hole like a stopper. It could thus be removed at will and the acid renewed. Prof. Ayrton stated that he had used a similar arrangement for nearly two years, a narrow necked glass bottle taking the place of the cup.—[In the report of the Physical Society for April 28 (p. 47), Mr. H. R. Droop's name was written *Troop*.]

Entomological Society, May 2.—J. W. Dunning, M.A., F.L.S., &c., president, in the chair.—The President said: "You scarcely need to be reminded that we this day complete the fiftieth year of our existence. It was on May 3, 1833, that nine gentlemen—Messrs. Children, J. E. Gray, G. R. Gray, Hope, Horsfield, Rudd, Stephens, Vigors, and Yarrell—met and resolved to found the Entomological Society of London. No time was lost; for on the 22nd of the same month the first general meeting was held at the Thatched House Tavern, the Rev. Wm. Kirby was chosen Honorary President, 103 Members were enrolled, and a Council of thirteen were chosen to complete the organisation of the Society and prepare rules for its government. Rooms were taken at No. 17, Old Bond Street, and on November 4, 1833, under the presidency of Mr. Children, the then Secretary of the Royal Society, a code of by laws was adopted and our first scientific meeting was held. Of the original Members six, and six only, still survive—Prof. C. C. Babington, the Rev. Leonard Jenyns (now Blomefield), Sir Sidney S. Saunders, Mr. W. B. Spence, Mr. G. R. Waterhouse, and Prof. Westwood. Of these Mr. Waterhouse has the additional distinction of having been one of the original Council, and the first Curator of the Society. Our meetings continued to be held at 17, Old Bond Street, from 1833 until 1852, when we removed to No. 12, Bedford Row; during nine sessions commencing in 1866, by the kindness of the Linnean Society, we assembled in Burlington House, but our library remained in Bedford Row. In 1875 the library and place of meeting were again united in this house; and though the building operations now in progress have prevented us from indulging in any celebration of our jubilee, we shall soon be in the enjoyment of improved accommodation, and I hope it may be long before the Society has again to change its quarters. At the present moment we have 33 Subscribers and 205 Ordinary Members, making a total of 238 contributing Members. Three years ago I ventured to express from this chair a hope that we might be able to publish a jubilee list of not less than 300 Members. It is not yet too late. And I appeal to each and all of you, gentlemen, to be active in striving to attain this object. 'The Entomological Society of London is instituted for the improvement and diffusion of entomological science.' From first to last this has been our only object. To bring fellow-workers into friendly communication and facilitate the interchange of ideas, to extract the hidden knowledge of secluded students, to provide a library for consultation, to encourage observation and experiment, and to publish the results for the benefit of all whom they may concern—such is our aim, the very reason of our being. And I venture to assert that the Society has succeeded in its object. If any be inclined to doubt, I refer him to the thirty volumes of our *Transactions*, to the *Record of Proceedings* at our more than 600 meetings, as proof of our activity and of the unfailing ardour with which the Society has now for half a century devoted itself to the diffusion of entomological science. I can only regret that by the irony of fate it has fallen to *my* lot to fill the presidential chair on this occasion, when, of all others, it ought to have been occupied by one of the fathers of British entomology. But you have willed it otherwise, and I will bury my regret; nay, it is already swallowed up in the delight I feel at the commission with which I have been intrusted by the unanimous voice of the Council, and I am sure that the proposition I have now to make will meet with your approval, and be carried by acclamation. I have to suggest that Prof. Westwood be made titular Life-President of the Society. There is no man to whom we as a body owe so much. An Original Member, he has never failed us; during the crucial period of our childhood he was the motive power, the life and soul of the Society; for fourteen consecutive years he was Secretary, and for part of that time he was Curator also. The Council has seldom been complete without him; he has been vice-president times without number, and during six years (1851–52, 72–73, 76–77) he was our president. Whilst he resided in or near London he rarely missed one of our meetings; even Oxford cannot keep him away from us; and there is not a single year from first to last that he has not been a contributor to our *Transactions*. From 1827 to the present time his pen and his pencil have never been idle; his papers are scattered broadcast over the scientific publications of this and other countries. Scientific bodies, both at home and abroad, have delighted to do him honour. I do not propose to abdicate the function with which your kindness has invested me. But if it be your pleasure to adopt the suggestion that has been made, I shall be proud to recognise Prof. Westwood as my titular chief, and to yield the chair to him at any of our scientific meetings when we are favoured with his presence. I know no better way of showing that our constancy is equal to his, and that our gratitude is enduring and lifelong. It is a barren title and an empty honour, but it is all that we as a Society can bestow. He has grown gray in our service, and in recognition of his

services, to us in particular and to our science in general, I ask you to confer upon him a title which will be a standing record of the esteem in which we hold him, and which throughout the evening of his days shall assure him of our affectionate respect." The proposal was carried by acclamation, and Prof. Westwood was declared honorary life-pre ident of the Society.

Anthropological Institute, May 22.—Mr. Hyde Clarke, vice-president, in the chair.—Mr. G. P. Rathbone exhibited and described a collection of ethnological objects from Bolivia. —Major H. W. Feilden read a paper on stone implements from South Africa. The specimens exhibited form part of a collection made by the author in Natal, the Transvaal, and Zululand during the years 1881 and 1882. Out of the large number of worked stones and implements that have passed through the author's hands he had seen scarcely any with water-worn edges. It would appear, therefore, that these implements, chiefly made of comparatively soft materials, must have been used and lost in the immediate vicinity where they are now found, and the large numbers found in certain spots seem to indicate settlements on stations at such spots; moreover, the most prolific spots are generally just those which would be most advantageous for procuring game. On the summit range of the Drakensberg and in its rocky kloofs, where game must always have been scarce, stone implements are scarce, if not altogether absent, whilst on the lower levels of the Newcastle district, which even in the memory of middle-aged colonists swarmed with countless herds of antelope, we find abundant traces of the Stone period. The conclusion at which the author arrived was that the users of the stone implements found in the more recent of the superficial alluviums were not separated from the present day by any great lapse of time. On several occasions crystals of quartz were found in company with stone implements in the alluviums, and the author believed that the Stone age people had carried these crystals either as charms or ornaments. Possibly the Stone age existed for a lengthened term in South Africa, and may resolve itself into Palæolithic and Neolithic periods, but at present we have hardly sufficient data at command to enable us to arrive at definite conclusions.—The Rev. C. T. Price read a paper by the Rev. James Sibree on relics of the sign and gesture language among the Malagasy.

Institution of Civil Engineers, May 22.—Mr. Brunlees, president, in the chair. The first paper read was on the Edinburgh Waterworks, by Mr. Alexander Leslie, M.Inst.C.E.—The second paper read was on the waterworks of Port Elizabeth, South Africa, by Mr. J. G. Gamble, M.A., M.Inst.C.E.—The third paper was on the water-supply of Peterborough, by Mr. John Addy, M.Inst.C.E.

PARIS

Academy of Sciences, May 28.—M. E. Blanchard, president, in the chair.—The following papers were read:—General considerations on scientific methods with special reference to the *a posteriori* method of Newton and the *a priori* of Leibnitz, by M. E. Chevreul. The author concludes that the experimental inductive method, as practised by Newton and his successors, is unquestionably the cause of the progress of the physico-chemical sciences, while the absolute *a priori* method, as conceived by Leibnitz, barred the way to all further progress. While Newton sought the proximate cause in order gradually to ascend to a possible first cause, Leibnitz started from the first cause, which for him was everything. The study of the material world accessible to the senses led, according to the German philosopher, to nothing real, while the spiritual world, without parts or dimensions, as represented by monads, numerical unities endowed from their creation with motion, was the object of pure knowledge, that is, of God Himself.—An account of the meteorological station of Aigoual in the Cevennes, where an observatory for the systematic study of atmospheric phenomena is about to be erected, by F. Perrier.—Remarks on the violet sulphate of iridium in the heated state, due apparently to oxidation, by M. Lecoq de Boisbaudran. —On the physical and chemical constitution of the vine-growing lands treated by the method of submersion in the lower Rhone valley and Languedoc, by M. P. de Gasparin.—Experimental researches on the action of various alcohols applied slowly and continuously to the pig, by MM. Dujardin-Beaumetz and Audigé. The alcohols invariably produced sleep, prostration, lassitude, while absinthe gave rise to phenomena of excitation somewhat analogous to epilepsy. During the experiments, begun in June, 1879, and concluded in July, 1882, some of the animals died from the effects of the alcoholic poison, and others were sacrificed in order to study its action on the vital functions. This was in all cases found to be injurious.—Observations on the great comet of September, 1882, made at the Paris Observatory, by M. G. Bigourdan.—On the relations existing between the covariants and invariants of the binary form of the sixth order, by C. Stephanos.—On the relations existing between solar eclipses and terrestrial magnetism, by P. Denza.—Note on the hydrates of baryta, by H. Lescœur.—Constituents of the Montrond (Loire) mineral water, by M. Terreil.—On some combinations peculiar to the kreatine and kreatinine groups of substances, by E. Duvillier.—On the fermentation of bread-stuffs, by M. Chicandard. —On some features in the structure of the placenta of the rabbit, by M. Laulanié.—On the origin of the follicular cells and of the ovula in Ascidians and other animals, by M. H. Fol. The author considers that these cells are genetically the strict homologues of the spermatoblasts in zoosperms, while the ovula itself corresponds to the polyblast or male ovula of Duval.—On the formation of the cystoliths and their reabsorption in plants, by M. Chareyre.—On the shingle, sand, and mud formations along the beach of geological seas, by M. Stan. Meunier.—Fresh observations on the dimorphism of the foraminifera, with four illustrations, by MM. Munier-Chalmas and Schlumberger.—On a saccharine substance extracted from the lungs and phlegm of consumptive patients, by M. A. G. Pouchet.—On condiments, especially salt and vinegar, studied from the point of view of their influence on the digestion, by C. Husson. The author's experiments confirm the conclusions of Wurtz, Dumas, Béclard, Claude Bernard, and others, that taken moderately these condiments are useful, especially in stimulating the formation of the gastric juice. In excess they render the food more indigestible, and are irritating to the coats of the stomach. The proportion of salt should not exceed 5 or 10 grams to 0·5 kilograms of meat; of acids 1 to 4 per 1000.

CONTENTS

THURSDAY, JUNE 14, 1883

THE ECLIPSE OBSERVATIONS

THE following telegrams have been received touching the observations of the total eclipse of the sun on the 6th ult. :—

To the Secretary, Science and Art Department

SAN FRANCISCO, June 12, 8.16 A.M.

Double grating on equatorial indifferent; dense prism on 6-inch equatorial good; integrating Hilger good; red end slit good, red end prismatic camera indifferent, first order blue Rowland bad, second order blue Rowland bad, 4-inch photoheliograph indifferent, small photoheliograph good. Lines obtained mostly hydrogen, prominences almost absent.

Through Reuter's Agency

SAN FRANCISCO, June 12

The solar eclipse on the 6th ult. was very successfully observed by the English, American, and Continental astronomers stationed on Caroline Island, the sky being beautifully clear at the time. The corona extended over a distance of two diameters from the sun. The light during the middle of totality was equal to that of the full moon. Successful observations were made by Dr. Janssen, as well as by Prof. Tacchini, the intra-Mercurial planet Vulcan was not seen by M. Palisa. The D line of the spectrum was seen dark in the corona by Dr. C. S. Hastings. Good photographs of the corona were obtained by the English observers as well as by Dr. Janssen. The English observers were also successful in obtaining photographs of the flash. Good photographs were taken of the coronal spectrum in the blue end. The health of Messrs. Lawrence and Woods, the English observers, is excellent.

Putting these two telegrams together, there is every reason to be contented with the work which has been done by the polyglot band of observers on Caroline Island, for, as is to be gathered from the telegrams, this and not Flint Island was the one selected for the site of the observatories. Certainly the photographic attack has been stronger than it has ever been before; more novelties have been attempted, and more have been successfully achieved, whilst the scale on which the work has been done leaves nothing to be desired. Taking, for instance, the photographs of the corona, although we do not know the precise size to which Dr. Janssen limited himself, we may be certain that among his attempts would be included one to take pictures, giving a dark moon of at least six inches in diameter. That by means of the clockwork-driven photographic plate, the flash, by which term is meant that instantaneous appearance of bright lines at the moment of commencement and end of totality, has been secured, shows us that we have now a method of recording eclipse phenomena which is not likely to be neglected on a future occasion. We may hope in a few days' time to get some further information touching the eye-observations made by Prof. Tacchini and the American observers. Reuter's telegram is strangely silent about them at present, and there is little doubt that they have something far more important to tell us than that the dark line D was seen in the spectrum of the corona, for that was observed as long ago as 1871 by Dr. Jannsen. It would be much to be regretted if observations of the lines visible before and after totality were not attempted, especially as we learn that the photographs are limited to a greater or less extent to the lines of hydrogen.

The following general remarks have already appeared in the *Times* with regard to the results of the observations, and we cannot do better than reproduce them :—

"News from the eclipse party has at length arrived. As we stated in our article, published on the 4th of May, the American ship of war, instead of returning to Callao as was at first anticipated, proceeded to the Sandwich Islands, and there is little doubt that the English party made the voyage thence in one of the Pacific mail steamers.

"A telegram coming through Reuter's Agency informs us generally of the success of the observations. The weather seems to have been everything that could be desired, and although the observations were necessarily made from the lowest possible level, the extension of the corona was quite as great as was expected at this period of *maximum* solar activity. Further, we learn that the light during totality was quite equal in intensity to that of the full moon. This is another indication of the exceptional brightness of the corona, because in this eclipse, which was one of exceptional duration—and that is why such strenuous efforts were made to observe it—the lower and more brilliantly illuminated portions of the sun's atmosphere being more than usually veiled by the dark body of the moon during the middle of totality, the illumination of the air by these portions of the sun was less than is ordinarily the case. Unfortunately, the telegram may be read both ways touching the intra-Mercurial planet observations. We take it, however, to mean that no intra-Mercurial planet was seen by M. Palisa, who would probably give his chief attention to that point. It is satisfactory to learn that good photographs of the corona were obtained both by Dr. Janssen and the English observers. We may expect that the French photographs of the corona will surpass in beauty and detail anything which has yet been secured during eclipse observation. It is good news, too, to learn that for the first time in the history of eclipses the momentary flash of bright lines seen just before the beginning and immediately after the end of totality has been photographed. Reverting for a moment to our previous article, we would remind our readers that this end has been attained by the use of a slowly descending plate actuated by clock-work, which, since the flash has actually been photographed, will give its complete history, and enable us to determine the exact order in which the lines appeared and reappeared before and after totality.

"The telegram sent by the English observers, Messrs. Lawrence and Woods, to the Science and Art Department, supplies further particulars as to the results of the various attempts at recording the history of the eclipse. The first instrument on the official list is a Rutherford grating with 17,000 lines to the inch, which was used in conjunction with an equatorial telescope of six inches aperture. The grating was so arranged that the photographs of the green part of the first order spectrum on the one side and the same part of the second order spectrum on the other side should be attempted. This would give the region near F, one of the chief solar lines in the blue-green parts of the spectrum; but although the photographs were actually obtained, the observers do not seem to be very proud of them.

"The next instrument is a dense prism of 60°, mounted on a six-inch equatorial of very short focus. The object in view in employing a short focus was to

obtain a very small and intensely bright image of the corona, while the use of the prism of 60°, giving as small a dispersion as possible, still allowed a really useful amount to be secured. This instrument succeeded well. We do not know the number of photographs obtained by it, but if the instructions were carried out to the letter, seventeen should have been obtained.

"We come next to the instrument by means of which the photograph of the flash of bright lines to which we have referred was obtained. This on the official list is called the 'integrating Hilger.' It is a spectroscope armed with a collimator of very great focal length and directed merely to the sun's place, no image of the sun or corona, therefore, falling on its slit as is usually the case. The light from all the regions near the sun is mingled together, a photograph of the spectrum of this mixture being the special aim of the instrument. Messrs. Lawrence and Woods are evidently satisfied with the work in this direction, the code word they use indicating that they consider the results to be good ones. The moving plate with which the instrument is fitted was exposed two minutes before, and withdrawn from exposure two seconds after, totality. Knowing, therefore, as we do, that one flash was photographed, we may reasonably hope that this was the case also with the other, and as the instructions were to allow the plate to fall through one inch in eight minutes, we may also expect to get a comparison between the flash before and the flash after totality.

"The slit spectroscope armed with two prisms, which was provided by Captain Abney for the observations made last year in Egypt, was utilised also on this occasion with good results. Only one photograph was looked for from this instrument, one which would be exposed from the beginning until the end of totality.

"The prismatic camera, the instrument on the model of that used first in the eclipse of 1875, in which the corona forms its own slit, for some reason or other, does not appear to have been so successful in this eclipse, although it was tolerably so in that of last year.

"The attempt which has been least successful is that in which Prof. Rowland's concave grating was used as a prismatic camera, similar to that to which we have just referred. It was hoped to obtain a photograph of the blue end, both in the first and in the second order spectrum, but the results obtained are ciphered as bad. Seeing that Dr. Janssen was successful in his attempt to obtain large-scale photographs of the corona, we need not regret so much that our attempt to photograph it on a scale of four inches to the sun's diameter was unsuccessful.

"The small photoheliograph that was employed to such good purpose in Egypt last year has again given excellent results, which will be of the highest importance, as they will have been carefully executed, and the American party have taken no photographs themselves on the present occasion.

"The English observers telegraph that the lines obtained in the spectrum of the corona by these various methods are chiefly those of hydrogen. This, of course, does not apply to the flash we have spoken of. They add that the prominences were almost absent. This is an extremely important fact, because it shows what entire justification there was for the prediction made for the present eclipse after that of 1878, observed in the United States. That eclipse occurred at a *minimum* sunspot period, and the hydrogen lines were then seen only with difficulty, while the continuous spectrum of the corona was more or less brilliant. In the present eclipse the hydrogen lines were well seen with a very brilliant corona, as was anticipated would be the case at a period of sunspot *maximum*. This, perhaps, may explain the apparent absence of the prominences, because practically the lower part of the corona was itself made up of them.

"We have not, of course, any detailed information with regard to the results achieved by the other parties, but when our own two English observers have obtained such a rich harvest we are justified in concluding that the work of the American and French parties has been equally fruitful. In that case, the trouble which has been taken to secure the observation of this eclipse, which took place at a greater distance from home than any previously observed, will have been entirely justified.

"As we have said, the results of the other parties will take some time to reach us, but at least we may be sure of this—that the Americans, with their large experience of eclipses and their trained observers, will have much that is new and important to add to the results which our own English party has achieved."

It will be seen from what we have stated and from the extracts which we have made from the *Times* that the Royal Society and the Solar Physics Committee of the Science and Art Department are to be entirely congratulated on the result of their labours, and there is little doubt that in this, as in former eclipses, not only shall we have a most important explanation and verification of previous observations, but fresh questions will be raised to be included in future programmes. It should also be said that the indifferent success telegraphed in some cases may refer to the number of photographs taken rather than to the quality of some of them. It is not likely, for instance, that some photographs were not obtained of the bright lines before and after totality by means of the Rutherford grating, and if only two have been obtained at different epochs the greatest possible value must be attached to them.

The telegram does not state whether the observers have yet reached San Francisco, but in all probability they have, in which case they may be expected home in three weeks' time.

THE FERNS OF INDIA

Handbook to the Ferns of British India, Ceylon, and the Malay Peninsula. By Col. R. H. Beddome, F.L.S., late Conservator of Forests, Madras. Large 8vo, 500 Pages, with 300 Illustrations. (Calcutta: Thacker and Spink; London: W. Thacker and Co., 1883.)

FOR something like the last thirty years Col. Beddome has made a special study of Indian ferns under very favourable circumstances. Holding as he did till about a year ago the post of Chief Resident Conservator of the forests of the Madras presidency, he was brought into daily contact with them in his official work, and at his home at Ootacamund he formed a large collection of them under cultivation, many of which have never reached England in a living state. About 1860 he commenced his well known series of illustrations of Indian ferns, in continuation of Wight's "Icones," in which the ferns had been entirely neglected. His plates, like Wight's, were in quarto, uncoloured, and were mainly executed by native artists. His "Ferns of Southern India and Ceylon" contains plates of 271 species and varieties, and was issued in parts and finished in 1863. His "Ferns of British India," which was devoted to the species not found in the southern presidency, contains 345 plates and was finished in 1868. In 1876 he published a supplementary part, containing 45 additional plates, thus raising the total number to 660, and a revised general catalogue and summary of genera and species.

Now he has retired from his official position and come home to England, and the present work is the firstfruits of his leisure. It contains in a handy form a full description of all the Indian genera and species, and is illustrated by 300 uncoloured plates, reduced by means of photography from those of his larger books, one full page plate, with analytical details being given for each of the ninety-eight genera he adopts, and the others of smaller size interspersed amongst the letterpress. It is the first special book of portable size and moderate price which has been devoted to Indian ferns, and is in every way deserving of the extensive circulation it is sure to obtain.

India is one of the great fern-centres of the world, and it would not be an extravagant estimate to say that three-quarters of the genera and one-quarter of the whole number of ferns are known to grow within the area covered by the present work, which is precisely the same as that included in the "Flora of British India," by Sir J. D. Hooker, of which three volumes are now completed. Europe is not a rich fern-continent, and most of the European species extend their range to the Western Himalayas. The Malay Islands are very rich in ferns, and a large proportion of the Malayan species extend to the Eastern Himalayas and the mountains of the Peninsula and Ceylon; and there are in India a considerable number of endemic species. Col. Beddome deserves full credit for not making or admitting species upon insufficient grounds, and the number described in the present work does not fall far short of six hundred, all of which are Filices in the restricted sense, the Lycopodiaceæ and Rhizocarps, which would carry up the number a hundred more, not being included.

Ferns are plants that suffer very little in the drying process, and they are generally the first plants to be collected when a new country is explored. But on the other hand they are often far too large in size to be well represented in herbarium specimens, and often so extremely variable in habit, that it is very easy to mistake a mere casual variety for a genuine species. The first naming of Indian ferns on a large scale was in the great herbarium of Indian plants distributed by Wallich; but he gave no descriptions, simply names and numbers and localities, and very often confused together two or three totally different plants under the same number. In the five volumes of his "Species Filicum," the species were worked out and described by Sir William Hooker; and they were worked up again with abridged descriptions in the "Synopsis Filicum," which it fell to my lot to continue after his death. In England the other botanists who had specially attended to Indian ferns were Prof. David Don and Messrs. John Smith and Thomas Moore. So that till within a comparatively recent date no one had written upon Indian ferns who had had any chance of studying them in the field. But now the matter stands upon an entirely different footing. In 1880 Mr. C. B. Clarke, who, after working for five years at Kew on the "Flora Indica," has just returned to India, and who had paid special attention to ferns whilst collecting largely in the Himalayas, published in the first volume of the new botanical series of the *Transactions of the Linnean Society*, a revision of the North Indian species, illustrated by 36 plates; and now Col. Beddome, whose field experience has been mainly gained amongst the mountains of the Peninsula, has worked up the whole series, with a full opportunity of consulting the type-specimens of his predecessors, deposited at Kew, the Linnean Society, and the British Museum.

As regards details of generic and specific limitation of course no two authors who work independently but will vary considerably. In the matter of fern-genera systematists are divided into two parties—one regarding a difference in veining as sufficient in itself to found a genus upon, and the other maintaining substantially intact the time-honoured genera of Swartz and Willdenow, which are founded entirely on characters derived from fructification. Of the first party among modern writers, Presl, Fée, Smith, and Moore are the leading representatives; of the latter Hooker, Mettenius, and Eaton. Upon this matter I differ from Col. Beddome, and the difference amounts to wishing to use different names for perhaps half the species included in his book. Of course what he and Mr. Clarke have written about species-limitation and the distribution of the species through different parts of India will be a great accession to our knowledge; but I am rather amused to see that out of the limited number of new species which Mr. Clarke made he refuses to admit at least half; and that he totally rejects the only material innovation that Mr. Clarke proposed on the classification of our "Synopsis Filicum," the dividing of our *Asplenium umbrosum*, to establish out of part of it a new section of Asplenium, to be called Pseudallantodia, and characterised by a sausage-shaped involucre bursting irregularly. The only points on which I feel inclined to find fault with him are two. The first, that in his key to the genera he puts Hymenophylleæ under Polypodiaceæ, without taking any notice of the difference in the structure of the sporange,—but I see this is noticed in the detailed diagnosis at p. 28. It seems to me that Hymenophylleæ have excellent claims to be regarded as a distinct sub-order. The other point on which I wish to enter a decided objection is to the plan which he follows, or rather want of plan, in citing the authorities for the specific names. When he places a species under a different genus to that under which it was classified by its original describer, he moves backwards and forwards without any uniformity between four different ways of citing the authority. Sometimes he writes "*Gleichenia glauca* (Hook.)" for a plant described by Thunberg as *Polypodium glaucum*, and transferred by Hooker to Gleichenia, which is the plan usually adopted by botanists. But in many other cases he writes "*Botrychium Lunaria* (Linn. under Osmunda)" when the species was described by Linnæus as an Osmunda and transferred by Swartz to Botrychium; or "*Cyrtomium falcatum* (Sw.)" when Swartz called the plant *Aspidium falcatum* and Presl transferred it to Cyrtomium; or even "*Lastrea thelypteris* (Desv.)" for a plant published first by Linnæus as a Polypodium, transferred by Swartz to Aspidium, by Desvaux to Nephrodium, and Presl to Lastrea. And the same uncertainty vitiates his citations of books at the end of his descriptions. His citations refer to the plant, but according to the accepted usage amongst botanists they will be taken, and very often wrongly taken, as referring to the binomial name as used, so that if any one copies synonymy from the book without checking it off he will often find it leads him astray.

J. G. BAKER

OUR BOOK SHELF

Die Weich- und Schaltiere gemeinfasslich Dargestellt. Von Prof. Ed. von Martens. (Leipzig: G. Feytag; Prag: F. Tempsky, 1883.)

"CONCHOLOGY is ris!" was the pithy remark of the lamented Edward Forbes, made in his cheery way about forty years ago, when Mr. James Smith of Jordan Hill directed his attention to the arctic nature of some fossil shells in the Clyde district. Capt. Brown, however, had previously but unconsciously published the same hypothesis, which has been lately confirmed and extended by the discoveries of Messrs. Steele and Scott at Glasgow. Since the above remark was made by Forbes the study of the Mollusca has in a general point of view marvellously increased and become popularised by innumerable publications. We have now no fewer than six periodical works on the subject, English, French, Belgian, German, Italian, and American, besides four most useful manuals in English, French, German, and American. The German and latest manual, now before me, has been written by an experienced conchologist whose father (Georg von Martens) was favourably known to science nearly sixty years ago by his "Reise nach Venedig." The present author may therefore be considered an hereditary naturalist.

The manual of Prof. von Martens differs from that of Dr. Paul Fischer ("Manuel de Conchyliologie") which is in course of publication, as well as from Woodward's "Manual," in its plan and popular mode of treatment, although all these works are equally good. The present treatise on the soft or naked and shelly Mollusks forms a small octavo handbook of 327 pages, and is illustrated by 205 figures. The principal contents of the work are as follows:—

(1) Names and position in zoology; (2) The shell in general; (3) Organic structure of the Mollusca; (4) Cephalopods; (5) Univalve shells, Nudibranchs, Heteropoda, Pteropoda, and Solenoconchia; (6) Bivalves; (7) Habitat and geographical distribution; (8) Enemies and use of the Mollusca. The illustrations are excellent; they are not arranged in plates, as in the manuals of Woodward and Fischer, but are dispersed throughout the work in their appropriate places by way of explanation. This is in some respects an improvement, although it causes an unnecessary repetition of the same figures. For instance *Margaritana margaritifera* (why not *Unio margaritifer?*) is figured three times in pp. 196, 221, and 311.

The curious varieties or monstrosities of *Planorbis multiformis*, a tertiary shell from Steinheim, are well shown in Fig. 128. I am very glad to see that the author is by no means addicted to an excessive multiplication of genera and species, which is the normal failing of so many Continental conchologists, especially in the land and freshwater shells. In the Pteropoda he has rightly adopted Pallas's generic name *Clione* (1767-1774) for *C. borealis*, instead of Müller's name *Clio* (1776), which Fischer has used in the reverse sense. *Clio* of Linné (founded on Browne's genus and Jamaican species) is wrongly represented in the manuals of Fischer and von Martens by *Cleodora* of Lamarck. As no review or notice of any book is regarded as complete or satisfactory without a dash of criticism, however slight, I would venture to suggest a few corrigenda for the next edition. It is impossible to distinguish *Helix hortensis* from *H. nemoralis*, except as a variety, the former being more northern and the latter more southern in geographical distribution. *Hyalæa* of Lamarck (1810) ought to be *Cavolina* of Gioeni (1783) and Abildgaard (1791), not of Bruguière (1792); *Loripes* is not a synonym of *Lucina*, but a distinct genus, and *Sphærium* is a much older name than *Cyclas*. But I make these few remarks more for the consideration of the author than from any pretence on my part to be a judge. I can heartily and conscientiously recommend this manual not only to the scientific but to the ordinary class of readers. J. GWYN JEFFREYS

Notes on Qualitative Analysis, Concise and Explanatory. By H. G. H. Fenton. (Cambridge University Press, 1883.)

THESE are ordinary tables of reactions of the "more common metals and acids," and also of some of the "more common organic bodies." The organic bodies include carbohydrates and a few alkaloids.

It is very strange that the farce of common and rare elements is still maintained in nearly all the tables and books on qualitative analysis. Surely such elements as titanium and tungsten and molybdenum and selenium or lithium are common enough, at any rate in laboratories, to have a place given to them in analysis books, not to mention thallium, glucinum, and cerium, which do occur in minerals, to the no small mystification of the poor student crammed up with tables of analyses of "common metals." There are rather too many empty pages in these "Notes," and the size is inconveniently large for working with on a laboratory bench.

Practical Chemistry, with Notes and Questions on Theoretical Chemistry. By William Ripper, Science Master, Sheffield Board School. (London: Isbister, 1883.)

THESE notes and questions, mostly questions, have been, as the author explains, compiled to prepare students and teachers for the examinations of the Science and Art Department. It is to be regretted that such books are required, for although, as the author states in his preface, the arrangement may have been very successful in "passing" students, it is questionable whether the information and knowledge obtained are of such a nature as to be valuable afterwards. The book is well adapted for its purpose, that of cramming.

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts, No notice is taken of anonymous communications.*

[*The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to insure the appearance even of communications containing interesting and novel facts.*]

The Matter of Space

WILL you permit me to express my thanks to Prof. Herschel for his flattering review of my paper on "The Matter of Space," in NATURE, vol. xxvii. p. 349? It is certainly gratifying to find that the views which I deduced from the ordinary relations of moving matter are confirmed by the results of mathematical analysis, and it is a source of satisfaction to me to have called forth such a studied and thorough treatment of the subject as Prof. Herschel has given it. I cannot but retain my view of the unity in character of all substance, to which he objects, yet in that respect our opinions diverge but slightly, since I replace ether with excessively disintegrated matter, and he considers the particles of ponderable matter to consist of aggregates of ethereal substance. An ether whose condensation yields particled matter answers all the requirements of unity of substance.

As the subject is under discussion, there are some further points in the motor relations of particles which it may be well to indicate. It is highly improbable that the molecules of matter, even if it be in the state of a rare gas, wander at will, constantly changing their relations of position to other molecules. More probably there is very little independent change of place, each molecule being usually held as a close prisoner in a nest of surrounding molecules. The grouping of molecules may be changed by the action of external agencies, but a new molecular equilibrium tends to be quickly established. Such seems the general tendency of nature. If some of the molecules in a mass of substance have an independent motion, friction soon disseminates that motion, and brings them into harmonious conformity with

their fellows. This we know to be the case with all moving masses of matter. Their independent motion is gradually overcome by friction, and their motion brought into accordance with that of surrounding masses. The same principle applies to molecules. Friction, or molecular impact, its equivalent, must quickly reduce their discordant movements, and bring all the molecules of a mass into harmonious motor relations.

Molecules are related to their fellows in three distinct methods, those of the solid, the liquid, and the gas. We can only conjecture the character of these relations. In the solid there is perhaps no impact of molecules under normal conditions. Each molecule may lie in the centre of a nest of attractions, within which it describes a vibratory movement, without coming into contact with any of the similarly moving molecules that surround it. In the gas attraction also acts, but not vigorously enough to restrain the moving molecule, and cause vibration. Here, then, impact is incessant. Yet the molecule can seldom escape from its nest. It is driven backwards on all sides, and held captive within a contracted space. A certain harmony must arise between the motions of these gas molecules under conditions of equalised temperature, which must tend to produce an equal resistance to escape in every direction, and to confine each within a fixed space in relation to those surrounding. Such is not the case when in one gas a volume of a different gas is set free. The molecules, moving with different speeds, cannot harmonise in their impacts. The molecules of the second gas find open spaces in the net of the first, and rapidly disseminate themselves. But the probability continues that in every homogeneous gas, at a fixed temperature, there is little or no free excursion of molecules. In the liquid there are also probably harmonious relations of molecular motion. The character of this motion we do not know. It is possibly a rotation around general centres of gravity. However that be, each molecule must, under homogeneous conditions, move within fixed limits.

If such be the case, each molecule of a homogeneous mass occupies a fixed field, from which it cannot of itself escape, and whose boundaries it cannot change. These boundaries are absolutely fixed by two energies. One of these is the momentum of the molecule, by which it drives back those surrounding it. The other is the pressure bearing upon the mass of which it forms a part. This pressure is usually very great, so that the space occupied by each molecule is very minute, and its change of direction is necessarily very frequent. The pressure constitutes a tension, to which every molecule is subjected, and each has a normal rate of vibration, in accordance with this tension. Of course the weight of the molecule is a constant element in determining its vibratory pitch, which is therefore dependent upon the fixed element of weight and the varying element of pressure.

Such may be the condition of equilibrium of all material molecules, whatever their state of aggregation, namely, the confinement of each molecule within a limited field, within which it incessantly moves, but from which it cannot escape. And to this condition of absolute localisation of every particle all matter tends to come, according to the theory of dissipation of energy. But as nature now exists there are opposing influences which constantly overcome the tendency to localisation. One of these is the attraction of gravitation, which causes independent motion both in masses and in molecules. The other is the heterogeneity of momentum of molecules, or what is usually known as inequality of temperature.

So far as the first of these is concerned, matter is now nearly in equilibrium. The spheres are yet contracting, under the influence of gravity, but this contraction is so gradual as not to materially affect the relations of molecules. Their mutual localisation is but slightly disturbed by this cause. The inequality of temperature is a more vigorous disturbing cause. To this are chiefly due those transfers of energy through space, and of matter through other matter to which all life and activity must be ascribed. The inequality here referred to, as mentioned in my former paper, is not of absolute heat contents, but of temperature, which is a very different thing, since density affects the heat-containing powers of matter, and two masses of different density may be equal in temperature while very unlike in heat contents.

The accepted view of radiation is that there is everywhere an unceasing outward transfer of molecular motion, that each molecule constantly yields and constantly receives radiant heat, changing in temperature when these transfers are unequal, but not when they are equal. This seems to me an incorrect view of the subject. If two masses of matter of equal temperature be in contact there can be no radiation at all, instead of a double radiation. For no molecule can transfer any of its energy to others. If two molecules of equal momentum come into contact, neither can lose nor gain momentum. The momentum of each remains the same after as before the contact, and there cannot properly be said to have been a mutual transfer of energies. The only change that takes place is a change of direction of motion, and in this respect the change in the one case balances that in the other.

Thus properly we can speak of a transfer of motor energy only when the momentums of the molecules differ, and in this case the transfer is from the most to the least vigorous only. Heat is yielded outwardly, but not cold. This transfer is continually taking place, since the temperature of matter is very far from a state of equilibrium. The transfer takes place in two methods. One of these is through direct collision. The other is through vibratory impulse. We must consider these in succession.

Collision constantly takes place between the molecules of gases. It takes place also in solids and liquids when by any cause their molecular equilibrium is disturbed. The result of a transfer of energy in this manner is what we may call an impact radiation. Motion cannot lose or gain speed or change in direction except through the influence of counteracting energies. Thus every impact radiation must run directly outward until overcome by opposing energies of equal vigour. It is transferred from particle to particle, its speed changing in accordance with the weight of the particles, but its momentum continuing unchanged. Such impulses are constantly travelling in all directions. They are very frequently checked by equal opposite impulses, and thus become local motion of molecules. This is the ever-acting equilibriating tendency.

The other mode of molecular transfer is that supposed to be through exchange of vibrations—the radiation of light and heat. This transfer presents two relations. One is that of speed. This depends not on the speed of the motion, as in impact transfer, but on the tension of the conveying particles. As their tension increases, the radiant wave is conveyed more rapidly. As it diminishes, the wave travels more slowly. It is quite possible, indeed, that the waves of light may move with a different speed in interastral space from their known rapidity in the ether of the solar system, since ether may not be everywhere in the same state of tension. If so, certain astronomical conceptions would be affected. This equal speed of radiant transfer, whatever the rapidity of the vibration, indicates that radiance differs essentially from impact transfer. In fact, there is no special necessity in their character that transverse vibrations should be transferred. They may cease with any particle, and continue to exist as continuous vibration of that particle, or the energy of the vibration be yielded to it as direct motion. If in this case the particle move more vigorously than those surrounding, the vibratory transfer will be replaced by impact transfer.

This cessation of radiant transfer is constantly taking place. Every wave of light and heat that comes to the earth's surface is partly converted into local heat, partly transmitted through transparent substances and partly repelled from the surfaces of substances. Thus radiant transfer seems to be rather an accident than a necessity of matter, since the energy thus transferred can be immediately exchanged into local energy, without the agency of equal opposing energy, as in impact transfer. Whether the wave shall travel onwards, be absorbed, or be repelled, appears to depend on the tension of the substance which it affects. Each molecule of every mass has its normal pitch of vibration, in accordance with its weight and tension. If the radiant vibration be in complete accord with that pitch it will be retained as local heat vibration. If in imperfect accord it will be partly held. If discordant it will be transferred or repelled. In the latter case it probably follows the easiest channel. Although the direction of the ray is readily changed, yet probably it has a special vigour of movement in the direct channel. Other things being equal, it would follow the direct in preference to the reflected course. Therefore in cases of reflection there must be a resistance in the molecules of the reflecting substance which makes it easier for the wave to change its direction than to force itself on these molecules. This change of direction in the wave, however, is not a change in the direction of motion. The vibration continues in its original plane. It can only be changed from this plane by special influences within transparent substances, in which the wave vibrations, while acting upon the molecules of the substance, are in some way distorted by their interaction with the normal molecular motions.

There are two other methods of transfer of molecular motion to which brief allusion may be made. One of these is the electric transfer. The character of this we do not know, but we have reason to believe that it is vibratory, and that it bears certain analogies to light vibration. The other method is heat conduction. This is a transfer of energy by exchange of normal vibrations. It takes the place in solids of the impact transfer in gases. The molecule of the solid, when possessed of excess motive energy, cannot yield it to others by impact, and must therefore do so by a drag upon these others through the ties of attraction. This is the slowest of all modes of transfer of energy. For its proper action it is necessary that the substance should be homogeneous, and the vibrations of its molecules normal. The instant the tension changes, either by connection of unlike substances or a condensing twist in a homogeneous substance, the mode of transfer changes. The heat vibration of the molecule is offered to another of different pitch, which refuses to receive it as normal vibration, and at once the rapid electric transfer manifests itself. Normal heat vibration is thus converted into thermoelectricity.

Their brief review may help to give some idea of the relations between molecules. In their state of normal equilibrium, which they seek to regain after every excursion, they possess no independence of movement, but are rigidly confined within fixed limits. They may change place in common with all the molecules of the mass to which they belong, but not independently. Vigorous disturbing influences may break up the molecular grouping, but immediately a new stable grouping is assumed. The incessant molecular disturbances which occur do not usually cause a change of grouping. These consist of vibratory transmissions of energy, and of transfers of motion through impact of molecules, and their effect is but the production of momentary variations in the direction and vigour of the motion of the affected molecule. To the influences of this character above mentioned may be added those of the vibrations of sound, of magnetic energy, and of chemical affinity. The latter alone produces any marked variations of molecular grouping.

Philadelphia, U.S. CHARLES MORRIS

On the Morphology of the Pitcher of "Cephalotus follicularis"

I OBSERVE that the last sentence but one of my brief notice of Cephalotus, which appeared in NATURE last week, is calculated to convey an erroneous impression. The lid *g* of Fig. 1 is seen to be a conical structure with a relatively broad base and a narrower indented apex. In the matured pitcher the free portion of the lid is much broader than its more contracted base; and the developed and involuted margin referred to extends round the mouth of the pitcher *until it reaches that base, but does not cross it*, as by an oversight on my part my words imply.

Fallowfield, Manchester, June 8 W. C. WILLIAMSON

A Large Meteor

THE meteor seen by Mr. Hall of Shoreham (NATURE, vol. xxviii. p. 126) was also observed by Mr. James Cullen of the Stonyhurst Observatory. Its path, as seen from here, was from S.E. by E. to N.E. by E. (true), traversing an arc of about 70°. Its altitude was not more than from 12° to 15° above the horizon. It travelled exceedingly slowly, was visible for about 20 seconds, and was first seen at 10.30 p.m. G.M.T. Its size was that of the full moon, white in colour, and with a tail 10° to 12° in length. It burst into a shower of small pieces before it disappeared, presenting exactly the appearance described by your correspondent.

Owing to the twilight and to the haze which hung about the horizon, its position could not be referred to the stars, the only star visible being α Aquilæ, near which the meteor passed. From the compass bearings and altitude given above its approximate path was from Æ 18h. 50m., $\delta - 2°$, to Æ 22h. 35m., $\delta + 25°$.

Stonyhurst Observatory S. J. PERRY

YOUR correspondent, A. Hall, in your issue of June 7, records the appearance of a large meteor seen by him at Shoreham, Kent, on Sunday evening, June 3, at 10.40. I recorded the same meteor in the *Newcastle Daily Journal* as follows:—

"*An Enormous Meteor.*—Mr. Barkas informs us that on Sunday evening, June 3, at 10.40, an enormous meteor of great brilliancy moved slowly across the heavens from south to north, at an elevation of 30 degrees, and nearly horizontally. The colour was bright white, the apparent length 5 degrees; it had the form of an artist's brush; and the handle broke into many fragments. The head suddenly disappeared. This meteor was seen at Newcastle, Wreckington, and Cullercoats, and it would be interesting to know in what position it was observed at points far south of Northumberland."

Your correspondent does not say whether he saw it towards the south or north, nor does he state its elevation above the horizon. It would be interesting to know its apparent elevation at places north of Kent and south of Northumberland.

Newcastle-on-Tyne, June 8 T. P. BARKAS

Intelligence in Animals

IN NATURE (vol. xxviii. p. 82) is a letter headed "Intelligence in a Dog," which certainly shows that a power of reflection is sometimes possessed by the canine species far beyond what one ordinarily observes in them. Perhaps the following anecdote will interest some of your readers, in which it will be seen that the common crow of India exhibits (occasionally at least) an equal amount of a quality superior to what is usually styled *instinct* in animals.

In the summer of 1878, when I and a friend were travelling in the Himalayas, we marched from Dharmsala to Simla, passing through the native states of Mundi, Suket, Bilaspur, and Erki. One day, when we were about half way between Suket and Bilaspur, we rested two or three hours under the shadow of a rock whence there issued a spring of water most welcome to us thirsty and somewhat weary travellers. We drank our fill and threw ourselves down upon the ground. After we had been there a short time an old crow and its half-grown young one came also to slake their thirst. I happened to have a small piece of a stale chuppati (or unleavened bread which the natives eat) in my pocket, and I threw it to them; the old bird examined it, turned it over, and then called her young one to come and partake of it. The latter did his best to obey his parent, but the morsel was so hard and dry he could not manage to eat it, and said so in his own bird language. The old bird then as plainly replied "try again," which he did most obediently, but with no better success. The old bird then took up the rejected piece and deliberately placed it in one of the little streams formed by the water of the spring (perhaps about six feet beneath where I was lying); she then hopped off, followed by her young one, and here comes the most curious part of the story: in about a quarter of an hour or so both birds returned to this spot, the old one with her beak pointed to the piece of chuppati, which by that time had been rendered soft by the action of the water, and by signs and sounds seemed plainly to tell her young one, "There now, the food is soft; eat it, and no more nonsense." This the young bird immediately did.

Copenhagen, June 8 COSMOPOLITAN

MY big black Newfoundland retriever, "Faust," has a chivalrous habit of taking smaller and weaker dogs under his protection, and about two years ago he constituted himself champion of a wretched little thoroughbred mongrel whom we called the "Pauper," because it lived on charity in the garden opposite our house. "Faust" goes out marketing with the housekeeper, and as he has a passion for bread the baker's children always give him a stale roll. One day, for fun, they gave him two, which he picked up with some difficulty and then left the shop, followed by some of the children, one a lad of sixteen. "Faust" walked along the side of the garden railing till he met his pauper friend, to whom he gave one roll, and then ate the other himself, waving his tail vigorously in evident satisfaction. A neighbour of ours has a kitchen cat who was taken in out of charity himself, and who has several times brought in famishing friends for a meal. NELLIE MACLAGAN

Edinburgh, June 11

EASTERN ASIA AT THE FISHERIES EXHIBITION

THE sections of the Fisheries Exhibition devoted to China, Japan, and the British settlements and protected native states in the Malay Peninsula, are in some respects disappointing. The interest and beauty of the Chinese section are indeed unsurpassed; but the other

sections fall far below what might have been anticipated. At the Fisheries Exhibition in Berlin three years ago, Japan was excellently represented; and when we recollect that fish forms one of the principal—probably next to rice and millet, *the* principal—staples of Japanese food, that the fishing-grounds extend from the most northern Kuriles almost within the Arctic circle, through various zones down to the most southern islands of the Loochoo archipelago, where they approach the sub-tropical regions, and that the primitive methods of catching and preserving fish of more than one race are now daily practised in various parts of this chain of islands, known as the Japanese Empire, it will be seen what scope the Japanese authorities had to make their section of much practical and scientific interest. At Berlin their section did possess such interest, and the collection formed for exhibition there has, we believe, been made the nucleus for a domestic and permanent Fisheries Exhibition in Tokio. Failing the time or funds necessary to make a representative collection for London this year, it was open to the Japanese Government to take a single portion of their vast fishing-grounds—such as Yezo, or the Inland Sea, or the Loochoo Archipelago—and represent that only. This has been done by China with marked success. As it is, Japan is represented in the small space allotted to her by specimens of the fish tinned at the Government canning establishments at Sapporo in Yezo, and by a stall full of pictures on silk, lacquer, &c., of fish and fishing. These latter are all marked, "For sale at the close of the Exhibition." Doubtless the Japanese authorities had good reasons of their own for thus limiting their participation in the present Exhibition; still it is permissible to express regret that they did not add, as they undoubtedly could have done, more to its value and interest.

In the Malay States and the Straits Settlements fish is not such a staple of food as in Japan, and they are on the whole fairly represented. The curious Malay method of catching fish by constructing long and labyrinthine bamboo and cane fences, wide at the beginning and narrowing towards the end, where the fisherman's hut is placed aloft, is represented by two or three models. These long fences, sometimes stretching far out to sea, are familiar objects to every traveller in the Straits. They are protected by stringent local ordinances, and woe betide the unskilful shipmaster who runs his vessel through them.

The Chinese section, viewed from a popular standpoint, is certainly a success. No pains appear to have been spared to make it representative of the Celestial Empire in its decoration. The Chinese ambassador himself has contributed two scrolls in large characters containing verses of poetry. To the staff of the Imperial Customs under Sir Robert Hart—foremost in all that is for the welfare and good name of China—belongs the credit of this section. It would of course be impossible to represent in a single foreign contribution the fisheries of China, extending over more than 2000 miles of coast line, as well as many thousands of miles of rivers and canals, and accordingly it was decided to represent thoroughly one portion of the coast. At Berlin the Ningpo fisheries were so represented, and for this year, Swatow, a treaty port on a large estuary a little to the north of Canton, was selected. The nets, boats, lines, traps, and other implements used in fishing here, the dresses of the fishermen at various seasons, models of their huts, and a scientific classification of the fish caught in this district, form the bulk of the Chinese exhibits. In addition to Swatow, an attempt has been made also to represent Ichang, a port on the Yang-tsze, situated about 1000 miles from the sea, as well as the fisheries of South Formosa and the neighbouring islands. The collections were evidently made and catalogued in China and arranged here by experienced hands. The special catalogue published by the order of Sir Robert Hart forms a complete descriptive guide to the whole, and is most interesting and instructive. Speaking generally, it may be said that the observer is most struck in this section with the extraordinary ingenuity displayed in utilising the most ordinary and unpromising objects for the purpose of fishing. Thus in Swatow they employ a boat drawing a few inches of water, with the rail nearly level with the surface. A narrow plank fixed along one side is painted white, and the light of the moon falling on it causes the fish to mistake it for water. They jump over the plank into the boat, where they get entangled in moss or grass. At Ichang, a wild animal such as the otter is trained, not to catch fish, but to frighten them into the net; while at Ningpo, cormorants are regularly and systematically trained to fish. These and many other devices shown at the Exhibition mark the Chinese as the most ingenious and accomplished fishermen in the world. A large collection of corals, of crustaceans, mollusks, and other fish will attract the scientific observer, who will be much assisted in his examination by the special catalogue before mentioned.

NOTE ON THE INFLUENCE OF HIGH TEMPERATURE ON THE ELECTRICAL RESISTANCE OF THE HUMAN BODY

THE experiments which I have now for some years been carrying out as to the various forms of medical electricity have begun to furnish trustworthy results. Some of these, with the help of De Kilner, were incorporated in a paper read before the Society of Telegraph Engineers on March 9, 1882. We there stated that at present "we are hardly in a position to say how far the resistance of the body varies in health; but in disease it can be fairly stated that it sometimes diminishes and sometimes augments." Of this fact we gave illustrations.

It had often occurred to me that the temperature of the human body very probably influences its resistance; and some experiments had been made with a view of testing the amount of such influence. But in pathological researches it is often difficult to find a case not open to exception, and it is frequently necessary to wait a considerable time before, in the impossibility of experiment, accident presents one possessing the necessary conditions. Such a case I have now met with, and it is worth while to place it on record, if only to enable other observers to prosecute this line of investigation.

The patient is a young and intelligent gunsmith aged twenty-two. He had rheumatic fever severely twelve years ago, which, as is usual in young subjects, has left permanent heart disease behind it. This did not, however, prevent his following his trade until the beginning of April in the present year. He then began to suffer from morning rigors, occurring at first at the interval of from seven to ten days, but, since Easter, daily. He came into my ward in St. Thomas's Hospital on April 28. It is not necessary to detail the medical history of the case in a scientific periodical; it will be sufficient to state that about 8.30 a.m. he was in the habit of suffering from severe attacks not unlike those of ague, in the course of which the temperature rapidly rose to 105° F. In the afternoon it sank to the normal human temperature of 98° or 99° F. The cause of this remarkable symptom is still somewhat obscure; it has completely resisted the action of quinine and other antiperiodics, as well as salicylic acid, aconite, and other approved lowerers of temperature. It is probably due to ulcerative endocarditis slowly advancing. The most remarkable part of the case is that it causes the patient no suffering or inconvenience whatever. His mind is clear, and, except the feeling of chilliness during the period of heat, he makes no complaint. He is able to take interest in the determinations which I proceed to give.

It occurred to me that this unusual range of daily tem-

perature (7 F.) afforded the opportunity I had long been seeking. But it was some time before I could arrange suitable apparatus for its examination. A hospital ward is an awkward place for Wheatstone's bridge and delicate galvanometers. Moreover I had before found that from the peculiar conditions of the human body, the testing current, to produce accurate results, requires to be frequently reversed, for fear of opposition currents of polarisation. I am glad to see a confirmation of this observation in a verbal communication of Prof. Rosenthal to the Physiological Society of Berlin on April 13.

It was partly to overcome this difficulty that I devised, at Mr. Preece's suggestion, a dynamometer for alternating currents, of which the general arrangement was described in NATURE some time ago. It was also brought before the Physical Society at their June meeting in Oxford. Although severely criticised by some members of that learned body, it works extremely well, and may be, I hope, an addition to medico-electrical appliances. For the purpose of the present experiment I found that an ordinarily sensitive galvanometer, considerably damped by air-resistance, was sufficient, since by the zero methods of balancing, it is only necessary just to see the deflection before commutating; when balance is obtained, commutation has no effect on the needle of the bridge.

It would require more space than could probably be here afforded to give all details of the experiments, which, moreover, by the courtesy of Capt. Douglas Galton, I hope to bring before the British Association of this autumn. But a brief summary of results is as follows:—

On June 5 I reached the ward at 9.40 a.m. The rigor had begun at 8.30 and was beginning to decline; I had time, however, for the following determinations:—

9.40	R. 4140 ohms.
9.55	,, 3470 ,,
10.10	,, 2900 ,,

These measurements were taken with a very small E.M.F. of about 9 volts. On June 9 I succeeded in reaching the ward during the beginning of the rigor, and took the following measurements, this time with corresponding temperatures:—

10.30 a.m.	Temp. 102°·4 ...	R. 4550
10.40 ,, ...	,, 104°·2 ...	,, 4630
10.50 ,, ...	,, 104°·2 ...	,, 4930

At this point the rigor, temperature, and resistance began to descend. I visited the patient again at

2·15 p.m.	Temp. 103° ...	R. 2300

The apparatus in these observations was left untouched, so as to prevent any accidental change. The measurement was made with a double E.M.F. to those preceding, namely, 18 volts. I determined on each occasion the resistance of the leads and terminals, which I found to be on each occasion 2 ohms.

I cannot help thinking that the difference, which is as nearly as possible twice the smaller amount, is too great to be accounted for by any instrumental error, and that the human body, in spite of its large amount of liquid constituents, follows a similar thermal law of resistance to that influencing solid conductors, though in a very much higher ratio.

Only one other point requires comment, namely, the mode of making contact between the body and the testing apparatus. Prof. Rosenthal in the communication quoted above draws attention to the high insulating powers of the epidermis. In the above experiment I passed the current through the two legs, from one foot to the other, in alternate directions. The feet were previously soaked in salt and water; two large pans containing about a quart of brine each were then placed under the feet, and in each was immersed a plate of copper five inches square connected with the bridge by stout cables. I have found in other experiments that after half an hour the resistance ceases to decrease, and in this experiment it actually increased to the amount of 480 ohms. The whole foot was immersed, its sole resting directly on the copper plate. I have two other methods of making contact in use. The first consists of rubbing the skin with the oleate of mercury; which to the diffusion power of oleic acid adds the conductivity of its base, and then immersing the part in metallic mercury. The other consists of inserting small silver claw-forceps, known to surgeons as "serrefines," through the epidermis into the tissue below. This is rather painful, but not more so than I find medical students eager in the pursuit of knowledge can and will easily undergo.

W. H. STONE

THE AMBER FLORA [1]

THIS is the first volume of a work on the flora of the amber-bearing formations of East Prussia, and is devoted exclusively to Coniferæ. The introduction contains a sketch of the geological history of the order, and among much that is of interest we find an estimate that the existing Coniferæ occupy an area of about 3,000,000 square miles (500,000 German). The described fossil species are now almost as numerous as the living (400 to 450), though a revision might reduce their number by one-half. The colossal dimensions of some of the living Coniferæ are familiar to most, but it is not generally known how nearly these are rivalled by fossil species. Examples are given, as of a stem of *Cupressinoxylon ponderosum*, broken at both ends and 200 feet long, and another 12 to 14 feet across; a stem of Araucarites, 25 feet in circumference, and a silicified stem from California, 33 feet round the butt.

A considerable portion of the work is occupied with a minute and splendidly illustrated inquiry into and description of the microscopic structure of the tissue of existing and fossil Coniferæ, especially with regard to their resin-secreting organs. Goeppert claims to have originated this study forty years ago, and is certainly the chief authority in it. The result of his work shows that the Abietineæ, or fir tribe, have almost alone contributed the amber, and that at least six species produced it, the chief being close allies of the Common Spruce and the American *Pinus strobus*. These possess three separate sets of resin-producing organs chiefly situated in the cambium layers, which are in the form of cells and ducts running in both horizontal and vertical directions, and appearing at a very early stage of growth. Some Pines are liable to frost-cracks, and into these the resin collects and thence exudes, keeping the wound from healing and furnishing a perpetual supply. Very few specimens of amberwood preserve the bark layers with the resiniferous organs, but sufficient is seen to prove that these in no way differed from those of the Abietineæ at the present day, especially of the Spruce.

The most important section of the work probably is the research into the microscopic structure of the wood, which is, however, of an extremely technical nature. Five separate species of *Pinus* are recognised by their wood, and a very rare and doubtful wood-structure is referred to the Taxineæ.

More interesting perhaps to the general reader are the descriptions of fragments of foliage and fructification inclosed in the amber. Insignificant as the figured specimens appear, they are yet in so marvellous a state of preservation that their texture and microscopic structure, and even the glaucous colouring of the under sides of some of the leaves are visible. Twenty species are determined, with a tendency, it is pleasing to find, rather to curtail than to multiply the number previously described. They have been studied with extraordinary care, and the results are consequently unusually satisfactory.

[1] "Die Flora des Bernsteins." R. Goeppert and A. Menge, Naturforschenden Gesellschaft in Danzig. 4to, 1883.

The ABIETINEÆ are represented by 2 *Pines* of the *Tæda* and 2 of the *Pinaster* section, and by 2 *Firs*. It is impossible, with the material, to more than guess at the affinities of the fossil with the existing species in such an immense tribe, but 3 are compared with American, and 1 with a European species. The presence of 2 species of the *Parasol Fir* of Japan is of especial interest, if the appearance of a double midrib on the back of the leaf is a reliable character, but a doubt seems to be expressed in the altered termination of the name, "*Sciadopites*." Nearly all the rest are CUPRESSINEÆ, and many are represented by catkins and foliage. The *Widdringtonias*, a section of *Callitris* confined to the Cape and Madagascar, are represented by 2 species. The almost ubiquitous Tertiary *Libocedrus salicornioides*, allied to the Chilian Incense Cedar, is indisputably present, even its glaucous colour being preserved. Two *Thuyas* are indistinguishable from the Chinese and the American Arbor-Vitæ, and a more doubtful form is nearly related to the *Thuyopsis dolabrata* of Japan. A male catkin exactly resembling that of the Red Cedar of Virginia (from which pencils are made) represents the Junipers, and this extraordinary assemblage is completed by the presence of the common European *Cupressus*. The TAXODIEÆ, again, are represented by *Sequoia Langsdorfii*, a widespread and somewhat northern Tertiary Conifer, closely allied to the Californian Red Wood; *Taxodium distichum*, the Deciduous Swamp or Bald Cypress of Virginia, and the well-known Tertiary, *Glyptostrobus*, all but indistinguishable from the living Chinese species. The last described is an American type of *Ephedra*, or Jointed Fir.

A group of Coniferæ must therefore have existed in Europe, almost on one spot, comprising representatives from nearly every Geographical Province. There were present such magnificent representatives of the Californian Coniferæ as the Red Wood, the Sugar Pine, the Douglas Spruce, the scarcely less majestic Bald Cypress, Red Cedar, Thyua, and *Pinus rigida* of more eastern States, the Chilian Incense Cedar, the Parasol Fir, the Arbor-Vitæ, the *Glyptostrobus*, and the *Thuyopsis* of the Eastern Coasts of Asia, the Scotch Fir, the Spruce and the Cypress of Europe, and the Callitris of Southern Africa. Based on the careful research of a man who has made Coniferæ his especial study for fifty years, these determinations have a value which the haphazard methods of so many workers in Fossil Botany do not possess. The causes which led to the dispersion and extinction in Europe, in such relatively recent times, of so considerable a group of Coniferæ would be interesting to tracc out.

The similarity between the Amber Flora and the overlying Brown-coal Flora, described by Heer, lead to the inference that its age must be Middle Miocene. The deposits are uniformly sand, clay, and loam, in which are imbedded partly rolled stones of various kinds and sizes. The whole belongs to a vast and widely spread amber-bearing "diluvial formation" which stretches from the confines of the White Sea into Holland. The richest deposits are situated along a strip of coast between Memel and Dantzic, but the real home of amber has been supposed to lie in the bed of the Baltic between Bornholm and the mainland. It rests upon Cretaceous rocks, and consists chiefly of their debris, forming a peculiar mixture known as "blaue-Erde," which appears to exist throughout the Province of Samland at a depth of 80 to 100 feet, and to contain an almost inexhaustible supply of amber. The authors wish to correct the name to "blau-grün," to distinguish it from the blue earth which accompanies the brown coal in Silesia and elsewhere. Immense quantities of amber are washed out to sea from the coast, or brought down by rivulets and cast up again during storms or in certain winds. The expectations that amber-bearing beds of equal richness would be found at greater depths farther from the sea have not been realised, and these already priceless and apparently inexhaustible coast-deposits have thereby acquired an enhanced importance. It seems probable that the amber-beds of the North Sea belong to the same formation, and that these may even have been continuous to the east coast of Great Britain.

Though the greatest quantity of amber is found on the coast, the largest pieces, 6·5 and 9·5 kilos., were met with inland. It is never found in paying quantities at a greater depth than 4 to 6 feet, and chiefly in the "diluvial beds" with rolled fragments of brown coal, wood, and stones. It is rare in the brown-coal formation, and even when met with is almost confined to the Upper blue and plastic clays. The quantity, however, seems to be inexhaustible, for the rich and celebrated blue-earth of Samland extends along the coast for 60 miles, and possesses a breadth of about 12 miles and an average thickness of 10 feet. Runge estimates that each cubic foot contains $\frac{1}{13}$ lb. of amber, which gives a total of some 9,600,000,000 lbs. The actual yield at present is 200,000 to 300,000 lbs. per annum, or at least five times the quantity estimated to be cast up by the waves of the Baltic on this coast, so that it appears, at the present rate of quarrying, there is a supply for some 30,000 years to come. A good deal of amber, it must be remembered, is cast up on other Baltic shores and along the North Sea.

In an inquiry as to the probable extent of Pine forests that would be required to produce such a bulk of amber, the authors take the Norway Spruce (*Pinus abies*, Linn.) for the purpose of comparison. Estimating that the full age of the species is 120 years, sixty to seventy of which are resin-producing, they conclude that 6000 lbs. per acre would be the product of each generation, and therefore that the Baltic Sea, with its area of 6370 German miles, might yield, if covered with Norway Spruce, 8,408,400,000 lbs., or about an equivalent to the quantity contained in the 20 German square miles of the Samland "blue-earth" referred to above. It thus appears that if this amber in it had been produced on the spot, some 300 generations would have been required to furnish it, but it is of course far more probable that it has been collected together in its present position by the action of water. These estimates being founded on a species relatively poor in resin, even notoriously less resinous than *Pinus austriaca* and other existing species, it is likely that the amber yield was in excess.

The Amber Flora presents a group of cryptogams comprising 20 Fungi, 12 lichens, and about as many mosses—plants hardly represented in any other Tertiary Flora. It is united to other Miocene Floras, not only by its Coniferæ, but by the widely-spread *Cinnamonum polymorphum*. It contains 42 species of Conifers, Cupuliferæ, Betuleæ, Salicineæ, &c., a species of Hakea, in all 27 Monopetalæ and 12 Polypetalæ, including such rarely preserved orders as Scrophulariaceæ, Primulaceæ, Caprifoliaceæ, and Loranthaceæ, the gatherings from forest, meadow, and fen. These are to be described in a forthcoming work. The Coniferæ are, however, of chiefest interest, more especially as, while resembling the resinous species of the present day, their secretions differed so essentially in quality as to have left a product unknown in any other geological age.

J. STARKIE GARDNER

THE STORY OF A BOULDER

THE Warwickshire papers report a curious open-air service held on Sunday last at Stockton, near Rugby, to "consecrate" a large granite boulder which has been inscribed and railed in at the expense of the villagers. It lies on a bed of concrete in the centre of the little place, protected by a handsome iron railing; a few square inches are polished to show the grain; an inscription records that it was brought from Mount Sorrel, a distance

of sixty miles, by an iceberg or a glacier in the great Ice Age; and the ground around it is to be inclosed, turfed, planted, and set with rustic seats. A fine day, and the novel proceeding, drew a large and attentive crowd; a short, bright service was conducted with the aid of an unusually good village choir; and the big stone set up by Joshua at Shechem formed the text for a sermon intended to stamp the boulder as a religious no less than a scientific monument.

This charming little idyll is the closing chapter in a story which might claim to share the title made historic by a great geologist. Five years ago the present rector, coming to Stockton, found the boulder lying in a ditch, into which it had been rolled from its inconvenient position by the roadside. A hazy clerical belief that it was "Druidic" had saved it from complete destruction; but it was the cockshy of all the children, bonfires were lighted on it occasionally, and it lay at the mercy of every field club which might come hammering that way. Large, glaciated, and of granite, it was clearly worth preserving. The new rector told its probable history from the pulpit, and the village mind was roused. Reports came in of other big stones far and near, some of which were also of glacial origin; the quarrymen in the adjoining lime-works, digging down to a smaller piece of granite and some beautifully striated blocks of sandstone, protected instead of breaking them; and by following up the hint thus given, a fine bed of boulder clay was uncovered, shown to Dr. Crosskey, and inserted in the Boulder Committee Report of the British Association. The fame of the great stone spread; visitors came to see it; the Stocktonians, who had through frequent lectures learnt its scientific value, became proud of their "Pibble" and of their ability to instruct their neighbours; the subscription point was reached, and money found to move and rail in the treasure; the surrounding villages finally emptied themselves to attend the consecration service, and Stockton is at this moment, like douce Davie Deans, "as uplifted as a midden-cock on pattens."

The moral of the story is twofold. First: what has been done in Stockton ought to be done in scores of other villages. This boulder was the first link in a chain of evidence, lengthening ever since, in favour of a new and pregnant probability, the current of an ice-sheet from the Charnwood Forest heights across the table-land of South Warwickshire. In countless corners more lie similar monuments, unknown and doomed, which, if thus preserved and studied, would afford the keys to like problems in geology. And secondly: the clergy ought to do it. Our country parsons are, if they could be educated to see it, the natural discoverers and conservators of local relics; with the opportunities they have and the attainments they ought to possess, they might in their mere leisure write such a scientific history of England as no country has yet possessed. Let them read the delightful chapter in *Le Maudit*, which paints the Curé Julio in his Pyrenean parish, and in order that they may be qualified to imitate him, let the bishops be wise in their generation, and exact a knowledge of some branch of natural history from every candidate for Orders.

REPORT OF THE PARIS OBSERVATORY FOR THE YEAR 1882

WE have received from Admiral Mouchez, the Director of the Paris Observatory, the report on the state of that Observatory for the past year, and as we recently made reference to the state of our own Greenwich Observatory on the occasion of the visitation which took place at the beginning of the present month, we think it may interest our readers if we make a few extracts from this report of Admiral Mouchez.

The report opens with a complaint that the service of the Observatory has been very considerably deranged by the preparations for the transit of Venus. Not only did the various members of the expedition attend at the Observatory in order to be trained either in photography or in the use of the artificial transit, but no less than five of the *personnel* of the Observatory themselves took part in the work. At the same time, says Admiral Mouchez, the past year may take rank with any of its predecessors when the increased work of the Observatory is taken into account, for during this time an extension of ground has taken place, the equatorial coudé has been installed, and several underground chambers have been constructed for the purpose of studying magnetism and terrestrial physics generally. Curiously enough, one of the grounds on which the addition of magnetical studies to the work of the Observatory is urged is, that the cloudy skies of Paris so frequently interrupt the purely astronomical observations, that, without some such work as it is now proposed to add, the observers would frequently have little to do.

Among the purely astronomical work of the Observatory which has been going on for the last four years is that of the revision of Lalande's catalogue of stars, numbering 40,000. Concerning this work, we are informed that the General Catalogue, which will form eight volumes in quarto, is well in hand, and it is hoped that two volumes will be published each year, or at all events four volumes during the next three years. To assist in the construction of the catalogue, 110,000 meridian observations have been made during the last four years.

The employment of ordinary equatorials in an observatory, remarks Admiral Mouchez, necessitates a continual change of position of the observer, he being compelled to follow the movement of the eyepiece into positions which are often inconvenient and fatiguing, whilst the heavy dome of the observatory has also to be constantly rotated to follow the motion of the telescope. In order to obviate the necessity for this, M. Lœwy conceived the idea of adapting to the equatorial the system of "lunette brisée," employed first in England, and afterwards to a greater extent in Germany, especially in small transit instruments.

The new coudé equatorial may be thus described:—The polar axis of the instrument is supported at its extremities on two pillars like a meridian instrument. Round this axis the telescope turns, forming a right angle at the lower support. By means of a mirror placed at the summit of this angle the light is reflected along the pierced axis, at the end of which the eyepiece or the micrometer is placed. Under these conditions, with the telescope at rest, the equatorial stars pass across the observer's field of view. But of course the telescope must not be limited to the observation of equatorial stars. In order to secure the observation of other stars, a mirror free to rotate is placed before the object-glass and connected with the declination circle. The inclination of this mirror may be changed so as to throw the light coming from a star of any declination into the tube. This arrangement therefore permits the observer to explore every part of the heavens without quitting his position at one end of the polar axis. The telescope may, practically, by a rotation of this axis, be directed towards any part of the celestial equator, whilst a star of any declination may be made to throw its light down the broken telescope by means of the external mirror. It might be imagined that in this latter case the double reflection would result in the loss of a good deal of light, but we read that the preliminary experiments have shown that this is not the case, and that the polish and figure of the mirrors are very satisfactory. They are silvered, and of course can be easily repolished. We should add that this instrument, now one of the actualities of the Observatory, is due to the liberality of Mr. Bischoffsheim.

With regard to more strictly physical observations, those who have made their complaint respecting the

recent weather in England will perhaps find a grain of consolation in the statement that M. Thollon, who comes every summer to work in connection with this part of the Observatory, spent his whole summer there last year without being able to make a single observation. M. Egoroff, Professor of Physics at Warsaw, was, we learn, occupied during the months of July and August, as in preceding years, with the spectroscopic study of atmospheric absorption, working with a beam of electric light sent from Mont-Valérien to the Observatory.

Most of our readers are aware that the French Government has, as we think wisely, determined to separate the special meteorological investigations from the astronomical work of the Observatory. In consequence of this decision, Admiral Mouchez is now making meteorological observations of possibly a still higher value, with the special object of determining the different corrections, such as for refraction, to be applied to the astronomical observations.

The magnetical observatory which is now being completed will evidently be one of the first order. Six subterranean chambers of constant temperature have been built under the best possible conditions of isolation and stability. An outer wall of nearly 2 m. thickness incloses a rectangular space 40 m. in length, and 14 m. wide, completely impervious to moisture. The observing chambers, of which there are four of 5 m. by 4 m., and two of 6 m. by 5 m., are constructed in this space, being isolated from its walls by passages 2 m. wide. The walls of the observing chambers themselves are 80 centimetres thick; they communicate with each other by doors 1½ m. wide, and have a height of 3·65 m. The vaulted roof, 1 m. thick, is covered by earth to the thickness of 2 m., whilst grass and plants protect the soil from the direct rays of the sun, and from frost. The observing chambers can either be lighted by gas or by reflection from without.

Advantage has been taken of the existence of these chambers by placing in them the clocks from which the time is distributed throughout Paris, but in spite of all precautions it is unfortunately discovered that the chambers are not altogether free from minor trepidations resulting from the traffic of the streets. It is proposed therefore to place the apparatus for the study of the vertical and slow movements of the soil to a gallery in the Catacombs 27 m. below the surface. This apparatus has been constructed and is ready for use.

Among the meteorological work to be done with the object to which we have previously referred is included a series of observations from a captive balloon. This is of such a size that with ordinary gas in calm weather it can take self-registering barometers, thermometers, and hygrometers up to a height of 500 m., and with pure hydrogen it can ascend to a height of 800 m. It has been found by experiment that the balloon cannot be well managed if the air has a velocity greater than 4 m. or 5 m. per second; but this is not regarded as being inconvenient, because it is during complete calm that those great and frequent inversions of the law of decrease of temperature which most sensibly interfere with astronomical refraction, manifest themselves.

Simultaneous observations will be made on the meridian of the Observatory of Paris, north at the Observatory of Montmartre, and south at the Observatory of Montsouris.

The construction of the great refractor of 16 m. focal length with its dome of 20 m. in diameter is going on steadily. The object-glass worked by M. Martin is already finished, and the ground on which the Observatory is to be built is now prepared. There are some interesting details in the report touching the dome, the dimensions of which we learn will be the same as that of the Panthéon, and the largest ever attempted.

In insisting upon such a dome turning with ease, it must not be forgotten that it would be useless to construct one of such dimensions, unless steps were taken to prevent the ill effects which would arise from any displacement or deformation of the soil on which the Observatory is to be erected, or the walls of the Observatory itself. The arrangement which is to be adopted in the construction of this dome is that proposed by M. Eiffel. In order to reduce to a minimum the resistance due to friction on circular rollers, M. Eiffel proposes to float the dome by means of a circular caisson plunged in a receptacle of the same form, filled with a liquid which will not freeze, such as an aqueous solution of chloride of magnesium. An experiment made with a small model gave hopes of the most satisfactory results with this arrangement. There is much originality in the idea, and at the Paris Observatory more than anywhere else perhaps it is necessary that some such arrangement as this should be adopted, for it must not be forgotten that the Observatory is situated over the Catacombs, one result of which has been that for many years the pillars of the meridian circle erected in the gardens have gradually inclined towards the east in consequence of the displacement of the soil. If the same thing were to happen to the Observatory for the great equatorial, there is little doubt that before many years were over the dome would be quite immovable, whereas with Eiffel's floating arrangement, whatever be the change in level within season due to such a cause as we have named, the dome would still turn.

Another point which is engaging the attention of the Director is the erection of an astronomical observatory on the Pic du Midi, at a height of 2859 m. At this elevation, according to General de Nansouty, it is easy to read at night by the light of the stars alone, and fifteen or sixteen Pleiades are visible to the naked eye. It is indeed time that the importance of the possibility of observations at great heights received a fuller recognition. When the astronomical party were in the Rocky Mountains in 1878, to observe the eclipse of the sun there, three American observers had no difficulty in detecting the satellites of Jupiter every night with the naked eye. Nothing could show better the purity and transparency of the air than this, and to establish these facts is to establish also the necessity for utilising them. It is intended that any astronomer who wishes to make any special researches may take advantage of this Pic du Midi Observatory. At the same time, however, astronomers will be sent from the Paris Observatory to profit by the clear skies of the south at those times when the climate of Paris reduces the number of possible observations in the Observatory itself. It is pointed out that not only the science of astronomy, but physics, chemistry, and physiology, will probably derive great benefit from the institution of such an observatory as this.

NOTES

The Royal Society Soirée last Wednesday was as successful as usual, though the absence of the president, Mr. Spottiswoode, through illness, was to be regretted. From inquiries last night we are glad to learn that Mr. Spottiswoode, who is suffering from Roman fever, is going on very well.

The candidates selected by the Council of the Royal Society, whose names we gave in Nature, vol. xxvii. p. 614, were elected last Thursday.

Dr. Michael Foster has, in accordance with unanimous expectation, been elected to the newly established Professorship of Physiology in the University of Cambridge; and Dr. Alexander Macalister, F.R.S., Professor of Comparative Anatomy and Zoology in the University of Dublin, has been elected to the Professorship of Anatomy, vacant by the resignation of Prof. Humphry.

It will gratify our readers to learn that Her Majesty has subscribed 50*l.* and the Prince of Wales 26*l.* 5*s.* to the fund now being raised by the Scottish Meteorological Society for the establishment of a Meteorological Observatory on the top of Ben Nevis. At a meeting of the Council of the Society on Saturday last, we understand that plans and specifications and offers from several contractors for making a road from Fort William to the top of Ben Nevis were submitted, and it was resolved to commence the making of the road at once; Mr. Sydney Mitchell, architect, was instructed to make arrangements for the completion of the work within two months.

Prof. Morris has presented to University College, London, his valuable geological library.

The following subjects have been settled for conferences at the Fisheries Exhibition; the authors whose names are given have consented to read papers. Many gentlemen have consented to act in this capacity, but the complete list is not yet ready:—British Fisheries and Fishermen, by H.R.H. the Duke of Edinburgh; the Fisheries of the United States, by Prof. Brown Goode; the Fisheries of the Dominion; the Fisheries of other Countries (Commissioners for Sweden, Norway, Netherlands, China, &c., have promised to take part in these conferences); Herring Fisheries, by Mr. R. W. Duff, M.P.; Pilchard and Mackerel Fisheries, by Mr. J. Cornish; Salmon and Salmon Fisheries; Fresh Water Fisheries (including Trout), by Mr. Francis Francis; Seal Fisheries, by Capt. Temple; Oyster Culture and Fisheries, by Prof. Hubrecht; Mollusks, Mussels, Whelks, &c., used for Food or Bait, by Mr. Chas. Harding; Line Fishing, by Mr. C. M. Murdahl; Trawling; the Application of Steam Power to the Fishing Industry; Principles of Fishery Legislation, by the Right Hon. G. Shaw Lefevre, M.P.; Fish Culture and Acclimatisation of Fishes, by Sir James Maitland; Fish as Food, by Sir Henry Thompson; Fish Transport and Fish Markets, by His Excellency Spencer Walpole; Food of Fishes, by Dr. F. Day; Storm Warnings, by Mr. R. H. Scott; Fish Diseases, by Prof. Huxley; Economic Condition of Fishermen, by Prof. Leone Levi; Protection of Life of Fishermen; Scientific Results of the Exhibition, by Prof. Ray Lankester.

The Committee appointed by the French Parliament to consider the pension to M. Pasteur, have agreed to recommend its increase from 12,000 francs to 25,000, with reversion to the widow and children.

Prof. Lenz is about to organise, with the aid of the Russian Geographical Society, a series of observations on terrestrial currents along four lines of Russian telegraphs—Moscow to Kazan and to Kharkoff, and Tiflis to Rostoff and Baku. The necessary instruments are ordered, and the observations will begin as soon as these are ready. These observations were highly recommended, as is known, by the International Polar Committee at its Hamburg meeting, in connection with the magnetic observations of the circumpolar stations, as well as by the Electrical Congress at Paris. Germany has already begun these observations, whilst Austria, Sweden, and Finland are about to start them.

As the Russian Meteorological stations on Novaya Zemlya and at the mouth of the Lena were unable to begin regular magnetic observations on September 1, 1882, and their observations during the first months probably will not have the desired degree of accuracy, the Meteorological Committee of the Russian Geographical Society has applied for grants of money to continue these observations for one year more. Two new meteorological stations have been opened at Obdorsk and at Mezen, in order to connect the Novaya Zemlya observations with those of Central Russia.

On the 5th inst. the Emperor of Austria inaugurated the new Vienna Observatory, on the Turken Schanze, in the northern outskirts of the town. The new building took nine years to construct, and during that time the present director went all over Europe and America in order to study the fitting-up of the best observatories. The result is that the Vienna Observatory is probably one of the most complete in existence. For an account of the great telescope, constructed by Grubb of Dublin, see Nature, vol. xxiv. p. 11.

A competition has been opened by the Genevan Society of Physics and Natural History for the best unpublished monograph on a genus or family of plants. The MSS. may be in Latin, French, German (in Roman writing), English, or Italian, and should be sent to Prof. Alph. de Candolle, Cour St. Pierre, 3, Geneva, before October 1, 1884. Members of the Society are not admitted to the competition. The prize is 500 francs.

M. Marcel Deprez, the author of the experiments on the transmission of force to a distance, has offered himself as a candidate for the place in the Academy of Sciences vacated by the death of M. Bresse, in the section of Mechanics.

There was an interesting gathering at Newnham, Cambridge, on Saturday, to celebrate the success of the College for Women, started there some years ago, and to honour its first and as yet only Principal, Miss Clough, by presenting her portrait to the institution. The progress which has been made in the higher education of women since Newnham was founded is striking. Though only a few years ago the attempt was barely tolerated by the University authorities, now the students are all but nominally attached to the University, and there can be no doubt that ere very long they will obtain all that the friends of these institutions desire. Miss Clough deserved all the honour paid her on Saturday, for mainly to her courage, intelligence, and tact has the wonderful success of Newnham been due.

Apropos of the education of women and of the callings for which they are suited, it is a remarkable fact that the recently opened Brooklyn Bridge, of which we have heard so much as one of the greatest triumphs of engineering, owes its existence partly to the genius of a woman. Mrs. Washington Roebling, the wife of the great engineer who was intrusted with the construction of the Brooklyn Bridge, has been chief of the engineering staff ever since her husband first fell ill. When he was disabled and could not proceed with his great work, Mrs. Roebling began to study engineering, and her success was such that in a short time she was able to take her husband's place, and the enormous structure which Americans not incorrectly call "one of the most conspicuous marvels of the nineteenth century" was completed under her direction. The honour of being the first to drive across the new bridge was well earned by Mrs. Roebling, and the peculiar share which she had taken in its construction was rightly held to justify a disregard of the old superstition which dooms to ill luck the structure over which a woman has been the first to cross.

Prof. A. H. Keane has been elected a Corresponding Member of the Anthropological Society of Washington.

The fifth International Congress of Americanists will be held at Copenhagen, August 21–24. King Christian will be "Protector" of the Congress, while Prince Frederick Christian will be Honorary President. Prof. Worsaae is President of the Committee of Organisation. The subjects to be discussed cover a wide field, including history and geology, archæology, anthropology and ethnography, linguistics and palæography.

The Duke of Westminster has intimated to the Council of the National Smoke Abatement Institution that he purposes to contribute 500*l.* to the Smoke Abatement Fund recently opened.

WE learn from the Spanish papers that the Mining Exhibition now open at Madrid, is a great success. However incomplete, it still represents to a certain degree the present state of this wide branch of n tional welfare of Spain, and probably will give an impulse to the further development of geology, which is one of the most popular sciences with the Spanish *savants*.

THIRTY-TWO schemes were examined by the jury for the erection of a statue to Christopher Columbus at Barcelona. The most accredited opinion in Spanish artistic circles is, however, that none of them corresponds to the greatness of the event of the discovery of a new continent, which it has to commemorate. The statue will be erected on the seashore, facing the port of Barcelona.

A WRITER in the *Times* on the present Czar of Russia and his two predecessors refers to some improvements which have been made during the present reign. "The most hopeful of all recent signs in Russia," he says, "has been the entire cessation of the ecclesiastical censorship that was formerly exercised over scientific writings. The censors used to be attached to the Universities; many of them were nomine s of the higher clergy; and books that were considered unorthodox were never licensed by them unless the author paid fees which deprived him of all profit in his work. This, at least, was generally the case, and if an author evaded payment it could only be through the patronage of some very powerful man. M. Delyanoff has allayed much irritation among Russian *savants* and given a valuable stimulus to University education by limiting the jurisdiction of the censors to political works. So we shall hear no more of books being suppressed—as one was by Count Tolstoi—for inquiring too minutely into miracles alleged to have been wrought by bones of saints drawn from the catacombs of Kieff and sold by the clergy at high prices."

WE have received the sixteenth volume of the *Memoirs of the Society of Naturalists at the Kharkoff University*. It is mostly occupied by a long paper by A. W. Guroff, on the geology of the provinces of Kharkoff and Ekaterinoslav, being a valuable addition to the former work by Prof. Levakovsky, "On the Cretaceous and subsequent Formations," and to the works of MM. Borisyak, Briot, Klemm, and many others. The author describes at length the crystalline rocks of the rapids of the Dnieper, and gives a map of their extension, as well as a classification of this complicated series. He then mentions the littoral formations of the coal-basin of the Don, as well as the coal-measures of the Kalmius, and dwells at some length on the Bakhmut depression, describing the different stages of its Permian deposits. As to the much-discussed question of the upper members of this formation, which are considered by several Russian geologists to belong to the Trias, he is inclined too to consider them as intermediary between the Permian and the Jura, as they contain remains of the *Posidonomya* (*Estheria*) *minuta* and the *Equisetum arenaceum*, which both characterise the same formation on the Volga. The Jurassic deposits which unite the Jura of the Don with that of Kieff, as also the Rhetic group of the Donets, are then dealt with, and a complete list of the Donets Jurassic fauna is given. The Donets Jura proves to be more like the Jura of middle Europe than that of middle Russia. The Tertiary deposits of Kharkoff (the *Spondylus* beds) seem to belong to the Eocene formation, which is covered with sand and sandstone of the Miocene period. The other papers of the same volume are a supplementary list of 200 Diptera of Kharkoff, by W. A. Yaroshevsky, bringing their number up to 908 species; and a note on the parasites of the *Stauronotus vastator*.

THE researches as to the invertebrate fauna of the Black Sea, which were made from 1833 to 1863 by Rathke, Nordmann, Kessler, and Wagner, which researches had brought only to forty the number of species of crustaceans discovered in the Black Sea, have contributed towards spreading the idea as to the remarkable poverty of this fauna. This opinion was rapidly overthrown by the more recent researches of MM. Czerniawsky, Markusen, and Bobretsky, who discovered in the space of a few years no less than 130 new species of crustaceans in the Black Sea, and brought, in 1869, the total number of crustacean species discovered there up to 160. The subsequent dredgings of MM. Krichaguin, Grebnitsky, and Czerniawsky added to the number seventy-six species more, and this last explorer even affirmed in one of his memoirs that the crustacean fauna of a single bay of the Black Sea—the Bay of Yalta—is richer than that of the whole of the Belgian coast. The opinions, however, as to the kinship of the Black Sea fauna with the faunas of the neighbouring seas are still divided. MM. Markusen and Grebnitsky maintained that it is closely akin to that of the seas of the north, and proved it by the presence of such crustaceans as are not met with in the Mediterranean (various species of *Mysis* and representatives of the groups of the *Cumacea*, *Bathyporeia*, *Niphargus*, *Padocerus*, and *Siphonœcetes*), but are common in the seas of the north. They pointed out also the circumstance that the forms which are the most numerous in the Black Sea are either cosmopolite forms or such as are common in the northern seas, but not those which might have immigrated from the Mediterranean. In a notice published in the last volume of the *Memoirs of the Kieff Society of Naturalists*, M. Sovinsky points out, however, that the reverse opinion—which admits a close relationship of the Black Sea crustaceans with the Mediterranean ones—gains more and more ground during these last few years. The Mediterranean fauna was of course submitted to the influence of the northern faunas, and its northern forms might have found an appropriate medium in the less salt water of the Black Sea; but the Black Sea fauna looks rather as a part of the fauna of the Mediterranean basin, slowly modified by the medium it inhabits; this opinion is supported, in fact, by the kinship of several Black Sea forms with those of the Mediterranean and the Red Sea, and by the richness of the Black Sea fauna in mere varieties and in such forms as are purely local, the prevailing types of the fauna being still the cosmopolite ones. The Black Sea fauna would be thus but a part of the Mediterranean fauna, but much impoverished and modified to a great extent by the variety of local conditions.

RUSSIAN geologists do not seem to hesitate in admitting the aqueous origin of granitic rocks which formerly were unanimously considered as igneous and eruptive. The late Prof. Barbot de Marny adopted this theory during the last years of his life, and the same theory is supported now with regard to the Dneiper pegmatites, by M. Gouroff, in the *Memoirs of the Society of Naturalists* at the Kharkoff University (vol. xvi.). After a thorough study of the granitic rocks of the rapids of the Dnieper, he describes these rocks, consisting of orthoclase, plagioclase, quartz, and biotite (this last and the orthoclase being often substituted by chlorite and epidote) as granitites. These granitites always appear stratified, and alternate with granitic gneisses, their stratification being well developed with an inclination towards N. 70° E. at 60°. They are also crossed with numerous veins of pegmatite. The quartz of the pegmatite contains large microscopical inclusions of water, sometimes with carbonic acid, and with solutions of natrium and calcium chloride; it is coloured brown, which colour disappears when it is heated. The veins of pegmatite contain large crystals of quartz and orthoclase. They decrease towards their lower ends and terminate in accumulations of crystalline quartz. They very often interfere with veins of quartz, either crystalline (with microscopic inclusions of water), or opal-like. M. Gouroff proposes to discuss further the question as to the origin of the peg-

matite veins in another paper, but he meanwhile points out that these veins are subsequent to the formation of the granitite and gneiss, and that, like the quartz veins, they are of aqueous origin.

In connection with the celebration of the centenary of ballooning, some foolhardy aëronauts have been attempting to cross the Channel through the fickle air. One, named L'Hoste, who started on Friday night from Boulogne, was missing till yesterday, when news reached Paris from Antwerp that he had been rescued in the North Sea by a French lugger bound for that town.

The Swedish Academy of Agriculture has proposed to the Government that a sum of 50*l.* be granted to Dr. R. Lundberg for a visit to the International Fisheries Exhibition. The proposal will most likely be granted.

On May 28, between 6 and 7.30 p.m., a magnificent mirage was seen at Finsbo, in Norra Ryrs parish, Sweden. During nearly two hours, with intervals of three to four minutes, a panoramic landscape was seen, with mountains, lakes, forests, and farms. To the eye the view appeared as if only three-quarters to one (English) mile distant.

Last year several Swedish merchants contributed a sum of 100*l.* to enable the Swedish Doctor of Zoology, C. Bovallius, who has been travelling in Central America, to forward rare zoological specimens to the Upsala University. Herr Bovallius has from time to time sent some valuable collections of insects and birds to this institution.

The additions to the Zoological Society's Gardens during the past week include an Ourang-outang (*Simia satyrus* ♀) from Sumatra, presented by Mr. J. M. Vermont; two Duyker Boks (*Cephalophus mergens* ♂ ♀) from South Africa, presented by Mr. H. H. Trevor; a Philippine Paradoxure (*Paradoxurus prehensilis*) from the Philippine Islands, presented by Mr. A. Burgess; a King Parrakeet (*Aprosmictus scapulatus*) from Australia, presented by Mrs. Lewin; a Lesser Sulphur-crested Cockatoo (*Cacatua sulphurea*) from Moluccas, presented by J. Snowdon Henry, F.Z.S.; two Viperine Snakes (*Tropidonotus viperinus*), a Dark Green Snake (*Zamenis atrovirens*) from North Africa, presented by Mr. J. C. J. Church; two Aye-ayes (*Chiromys madagascariensis*) from Madagascar, a Carpet Snake (*Morelia variegata*) from Australia, received on approval; a Hybrid Luhdorf's Deer (between *Cervus leuhdorfi* ♂ and *C. canadensis* ♀), ten Australian Wild Ducks (*Anas superciliosa*), bred in the Gardens.

OUR ASTRONOMICAL COLUMN

Cometary Refraction.—M. W. Meyer, of the Observatory of Geneva, has published a discussion of three series of micrometrical observations, made during as many near approaches of the great comet of 1881 (1881 III.) to stars, when the latter were seen through the denser parts of the head of the comet, the immediate object of the micrometrical measures of distance between the nucleus and the star being the detection of any deflection or refraction of the light of the star in passing through the cometic nebulosity. This comet offered a great advantage in an investigation of the kind, inasmuch as its nucleus had perfectly the appearance of a fixed star. M. Gustave Cellérier had treated the question from a theoretical point of view in a memoir published in *Archives des Sciences Physiques et Naturelles*, of Geneva, of October 15, 1882; the conclusions are reproduced in abstract by M. Meyer, who has applied the resulting formulæ to the case in question. The first series of observations was made on June 29, 1881, when the comet passed close to the star 519 of *Durchmusterung* + 65°, which is No. 6594 in Oeltzen's Catalogue, of 7·8 mag. The second series, on July 13, when the comet approached the star ι Draconis (Hev.) within about 38″, and the third series on August 1, when it passed about 24″ from a star of 9·10 mag. For details of the method of treating the observations we must refer to M. Meyer's paper, which appears in *Mémoires de la Société de Physique et d'Histoire Naturelle de Genève*, t. xxviii.; he sums up his conclusions as follows:—"La substance dont la chevelure de la grande comète de 1881 a été composée s'est optiquement comportée comme un gaz, et sa puissance réfractive à une distance de 10,200 kil. du noyau a été pendant l'époque des observations de 0·0000093. La pression de ce gaz diminuait dans les régions étudiées proportionellement au carré de la distance au noyau." He does not venture to say, however, that this value exactly represents the refractive power of the comet, though he believes in a measurable force.

M. Meyer remarks that previous attempts to detect a deflection of light in traversing the substance of a comet had led only to negative results. Bessel availed himself of the conjunction of Halley's Comet with a star of the tenth magnitude on September 29, 1835, to discover by heliometric measures an effect of this kind, but without success. His measures have been subjected to a new reduction, in accordance with M. Cellérier's theory, by M. Meyer, though with similar negative result.

Kepler's Nova of 1604.—The position of this famous star is now favourably situated for observation. It is most readily found by reference to a star of 8·9 mag., which occurs in Argelander's southern zones, and which is No. 16872 of Oeltzen's Catalogue. The place of this star for 1883·0 is in right ascension 17h. 24m. 2·9s., declination – 21° 23′ 34″. By Prof. Schönfeld's reduction of the observations of Fabricius in 1604, Kepler's star would precede 25·3s. in R.A. 0′·8 to the north. There is a star 11·12m. preceding 17·9s. and 1′·6 south of Argelander's, and another 12m. preceding 33·2s. and 2′·7 north; it is to the latter object, which was observed by Prof. Winnecke in 1875, though not previously glimpsed with a refractor of 7 inches aperture, that attention may be chiefly directed. It is to be remarked that the position of Kepler's star is liable to greater uncertainty than that of Tycho's star in 1572. It is very desirable that whatever may be the result of examination of the vicinity, it should be put upon record (of course with the corresponding date) from time to time. The Chinese annals have references to more than one of their stellar class *Ke-sing* or "extraordinary stars," in earlier times, which must have been situate in the neighbourhood of Kepler's *Nova*.

The Binary Star, γ Coronæ Australis.—Several years since an orbit was calculated for this object by Prof. Schiaparelli, who made use of measures up to 1875, whence it appeared that the periastron passage would take place about the end of 1882. Mr. Downing, availing himself of measures to 1880, has applied small corrections to the elements found by the Milan astronomer, and fixes the periastron passage to 1883·203, the period of revolution being 54·985 years. The binary is therefore describing at present a critical portion of the orbit, and, it may be hoped, will not be neglected by those observers of double stars who can well command its position. The following are angles and distances calculated from the two orbits:—

	Downing Pos.	Dist.	Schiaparelli Pos.	Dist.
	°	″	°	″
1883·50	134·2	0·27	93·3	0·38
83·75	103·8	0·32	80·4	0·50
84·25	74·6	0·55	59·5	0·73

The Saturnian Satellite, Mimas.—M. Meyer, observing with the 10 inch refractor presented to the Observatory of Geneva by the late director, Prof. Plantamour, succeeded in obtaining, near the opposition of Saturn in 1881, several sets of measures of the faint satellite, *Mimas*, about the time of greatest elongation. Considering the small number of measures of this object which have been obtained even with the largest instruments, M. Meyer's success is worthy of attention. On two nights he secured complete series of measures, and on other occasions partial ones. We find, on adjusting the circular elements adopted in this column for prediction of the positions of *Mimas* (which were founded upon Washington observations and required but small correction) by Prof. Frisby's observation of the conjunction of the satellite with the minor axis of the ring southwards, on November 8, 1882, that M. Meyer's measures are closely represented.

GEOGRAPHICAL NOTES

AT the meeting of the Royal Geographical Society on Monday night Sir Henry Rawlinson read the following telegram, forwarded by the Eastern Telegraph Company from Zanzibar, with regard to the movements of Mr. Joseph Thomson:—"Thomson reached Dgare na Erobi, in Masai country, long. 37°, lat. 3·5, on May 5. Was compelled to flee during night to evade what could only have been a disastrous fight, through troubles raised by Fischer's caravan in front. Got safely back to Taveta, where he camped his men, and has come down to Mombasa with small party in seven marches to replenish his goods, which has become necessary in consequence of his retreat from Masai and prolonged detention at Taveta. Returns in a few days to Taveta to proceed by Arusha, probably in company of another caravan. Is in good health. Details by post."

THE Russian Geographical Society has awarded its great gold medal to H. W. Abich, Member of the Academy of Sciences, for his researches into the Geology of the Caucasus. The gold medal of Count Lütke has been awarded to W. K. Döllen, astronomer of the Pulkova Observatory, for his new improved instrument for the determination of latitudes and longitudes. Two other gold medals, for ethnographical and statistical works, have been awarded to M. Barsoff, for collections of songs of Northern Russia, and to M. Krasnoperoff for a statistical description of the Government of Perm. Small gold medals have been awarded to MM. Eklon and Roborovsk, who both accompanied M. Prshevalsky in his travels; to M. Oshanin, zoologist, for travels in Karategin, Darvaz, and Turkestan; and to M. Vitkovsky, for exploring graves of the Stone period about Irkutsk. Silver medals have been awarded to M. Lessar for levelling operations between Askabad and Seraks; to M. Schultz for the same between Orenburg and Lake Aral; to M. Brounoff, for researches on cyclones and anticyclones in Europe; to M. Gladysheff, for determinations of latitudes and longitudes in the Akhal-tekke oasis; to M. Kiseleff, for a journey to the Bi-han; to M. Rodionoff, for surveys in Karategin; to M. Slovtsoff, for a description of the district of Kokchetan; also four other medals for smaller ethnographical and statistical works.

M. LESSAR, who made so interesting a journey from Askabad to Mash had, continues to make a series of excursions in the same region. He went a second time to Mash-had *viâ* Khelat, and thence to Zurabad, Saraks, Merv, Charjui, and Khiva; then he made a barometric levelling from Askabad to Tejent, visited Merv a second time, and in December last journeyed in the mountain region of Khelat, Daraghez, and Attek, thus covering about 3300 miles from April to December.

THE proprietors of the *Melbourne Age* have despatched an exploring expedition to New Guinea.

WE mentioned in a preceding volume the late Barbot de Marny's theory as to the formation of the dunes (*barkhans*) in the steppe of Kyzyl-kum and the influence of the wind as a powerful agency in modifying the earth's surface in the steppes. We find now, in the *Zapiski* of the Kieff Society of Naturalists, several objections to this theory by M. Borschoff. Without denying the partial influence of the wind, he reduces it to a quite secondary agency, and decidedly opposes the wind-theory of the formation of *barkhans*. Wind may increase the *barkhans* to a certain amount, but their primary origin must be sought for elsewhere, and the *rôle* of the wind is far below what has been assumed. So hard a rock as the sandstone, permeated with iron and lime, of the Kara-kum and Kyzyl-kum steppes cannot be disintegrated by wind, unless it has been disintegrated beforehand by rains and rapid changes of temperature —both which conditions are missing in the steppes. Therefore on the Emba, the Ilik, the Irghiz, where the same sandstones occur—as devoid of vegetation as in the Kyzyl-kum, there are no such surfaces covered with *barkhans* as in the neighbourhood of Lake Aral. As a rule the dunes appear only where there are remains of former lakes, and in such cases they assume the directions of the shores of these former basins. Far from being dependent on the direction of the prevailing winds, the direction of the *barkhans* varies, even within short distances, and it follows the windings of the coast of Lake Aral. Thus, they inclose the Sary Cheganak bay, like the parallel steps of an amphitheatre, the same directions being also taken by the rocky ledges of the terraces of sandstone, even beneath the water of the bay. The close connection of these ridges with the former action of the interior sea is the more obvious, as these dunes—sometimes stratified in their interior—often contain remains of Aral mollusks, such as *Cardium rusticum*, *Dreissena polymorpha*, as also *Adacna vitrea*. Whole banks, from a quarter to half a foot thick, of these shells are found in the *barkhans*, and they are met with at distances of 27 miles from Lake Aral, and 70 feet above its level (at Sopak), or even 80 miles north of it, and 100 to 120 feet above its level (at the Toulagai hill), whilst the depressions between the *barkhans* contain deposits of salt, with the same shells, or with an alga similar to the Aralian *Zostera*. The primary origin of the *barkhans*, M. Borschoff says, can be discovered even now in the low coast ridges. These ridges once formed slowly increase afterwards by the accumulation of vegetation on their summits, and vegetation plays a most important part in their growth. Several Solaneæ, such as *Caroxylon*, or *Halostachys*, and Gramineæ, such as *Ærulops lævis*, grow on their summit, which is covered subsequently with various species of *Tamarix* and *Calligonum*. When deeply covered with vegetation, their further increase is due to the sand brought by the wind, the organic life still remaining a powerful agency of increase. But their original appearance must be sought for, it is contended, in the agency of water. M. Severtsoff's remarks on the influence of vegetation on the growth of the dunes, and those of the Turkestan railway expedition on the immobility of the dunes (already analysed in NATURE), go far to sustain M. Borschoff's conclusions.

AN expedition, under the direction of Col. Prshevalsky, is being organised for the purpose of scientific researches in Central Asia and Thibet. The expedition is expected to start in August next.

A SERIES of valuable papers on the island of Yezo now appearing in the *Japan Gazette* deserve the attention of geographers. They are from the pen of Capt. Blakiston, who received the gold medal of the Royal Geographical Society in 1861 for exploration on the Yang-tsze, and who has for many years resided at Hakodate, the principal port of Yezo. The papers, which have reached their fifteenth part, are so varied and complete that they may fairly be called an encyclopædia of the island. The geography, geology, fauna and flora, the progress made during the past twenty years by the Japanese administrators, the Ainos, the mineral productions are all treated, and in addition the records of numerous journeys over all parts of the island are given. It is to be hoped that these valuable papers will be published in a collected form, for no future account of Yezo will be complete in which copious reference is not made to them. The numerous reports of the *employés* of the Japanese Government to the Colonisation Department in Tokio, which are now so difficult to obtain, are largely quoted in notes

ACCORDING to a new survey of the rapids of the Dnieper, the total fall of the river, on a stretch of forty-six miles, from Ekaterinoslav to the Khortitsa Island at Alexandrovsk, is 106·5 feet. The aggregate fall of the nine rapids is 60·3 feet, and their aggregate length is 5335 yards, the greatest rapid being that of Nenasytetsky (the Insatiable) which has a fall of 19·5 feet and a length of 1867 yards. The discharge of water at the head of the rapids has been found, at a level 2·5 feet below the average, to be 27,934 cubic feet per second.

THE CAUSE OF EVIDENT MAGNETISM IN IRON, STEEL, AND OTHER MAGNETIC METALS[1]

THE extreme sensitiveness of the induction balance to all molecular changes in the structure of metals was remarked in my first paper on this subject to the Royal Society;[2] and in the case of iron and steel it is most remarkable, as the addition or subtraction of 1/500,000th part, or the addition of the smallest iron filing to an already large balanced mass of iron, is at once rendered evident and measurable.

Possessing such an invaluable instrument of research, I was desirous of investigating the molecular construction of iron and steel, but at once I met with a difficulty, viz. that magnetism itself completely changed the character of any piece of iron under investigation; consequently, finding no help or explanation of the effects produced from any accepted theories of magnetism, I was forced to investigate, by means of the induction balance, the

[1] Paper read before the Society of Telegraph Engineers and of Electricians, on May 24, 1883, by Prof. D. E. Hughes, F.R.S., Vice-President.
[2] "On an Induction Current Balance, and Experimental Researches made therewith."—*Proceedings Royal Society*, March 29, 1879, p. 56.

whole question of magnetism as existing in the interior of a magnet, and to determine the particular structure for each case, such as neutrality and polarity.

In a recent paper to the Royal Society, upon the theory of magnetism (*Proceedings Royal Society*, May 10, 1883), I described the use of and demonstrations obtained by the induction balance. In this paper I propose to confine myself to demonstrations that can be repeated without it, and whose effects can be observed by the aid of ordinary magnetic direction needles.

That magnetism is of a molecular nature has long been accepted, for it is evident that, no matter how much we divide a magnet, we still have its two poles in each separate portion, consequently we can easily imagine this division carried so far, that we should at last arrive at the molecule itself possessing its two distinctive poles, consequently all theories of magnetism attempt some explanation of the cause of this molecular polarity, and the reason for apparent neutrality in a mass of iron.

Coulomb and Poisson assume that each molecule is a sphere containing two distinct magnetic fluids, which in the state of neutrality are mixed together, but when polarised are separated from each other at opposite sides; and, in order to explain why these fluids are kept apart as in a permanent magnet, they had to assume, again, that each molecule contained a peculiar coercive force, whose functions were to prevent any change or mixing of these fluids when separated.

There is not one experimental evidence to prove the truth of this assumption; and as regards coercive force, we have direct experimental proof opposing this view, as we know that molecular rigidity or hardness, as in tempered steel, and molecular freedom of softness, as in soft iron, fulfil all the conditions of this assumed coercive force.

Ampère's theory, based upon the analogy of electric currents, supposes elementary currents flowing around each molecule, and that in the neutral state these molecules are arranged haphazard in all directions, but that magnetisation consists in arranging them symmetrically.

The objections to Ampère's theory are numerous. 1. We have no knowledge or experimental proof of any elementary electric currents continually flowing without any expenditure of energy. 2. If we admit the assumption of electric currents around each molecule, the molecule itself would then be electromagnetic, and the question still remains, What is polarity? Have the supposed electric currents separated the two assumed magnetic fluids contained in the molecule, as in Poisson's theory? or are the electric currents themselves magnetic, independent of the iron molecule?

In order to produce the supposed heterogeneous arrangement of neutrality, Ampère's currents would have either to change their position upon the molecule and have no fixed axis of rotation, or else the molecule, with its currents and polarities, would rotate, and thus be acting in accordance with the theory of De la Rive. 3. This theory does not explain why (as in the case of soft iron) polarity should disappear whenever the exciting cause is removed, as in the case of transient magnetisation. It would thus require a coercive force in iron to cause exactly one-half of the molecules to instantly reverse their direction in order to pass from apparent external polarity to that of neutrality.

The influence of mechanical vibrations and stress upon iron in facilitating or discharging its magnetism, as proved by Matteucci, 1847, in addition to the discovery by Page, 1837, of a molecular movement taking place in iron during its magnetisation, producing audible sounds, and the discovery by Dr. Joule, 1842, of the elongation of iron when magnetised, led De la Rive, in his remarkable "Treatise on Electricity," 1853, to give his theoretical views upon magnetism in the following remarkable words:—

"The whole of the magnetic molecular phenomena that we have been studying lead us to believe that the magnetisation of a body is due to a particular arrangement of its molecules, originally endowed with magnetic virtue, but which in the natural state are so arranged that the magnetism of the body that they constitute is not apparent. Magnetism would therefore consist in disturbing this state of equilibrium, or in giving to the particles an arrangement that makes manifest the property with which they are endowed, and not in developing it in them. The coercitive force should be the resistance of the molecules to change their relative positions."

Wiedemann, in 1861, gives a theory in which he admits the fluids of Poisson, or the elementary currents of Ampère, as the cause of polarity of the molecule, but believes that the molecules are turned in a general direction in the case of polarity, and that in neutrality, like Ampère's, the magnetic axes of the molecules are turned in all directions.

Maxwell, in his remarkable treatise on "Electricity and Magnetism," 1881, page 75, gives the following *résumé* of Weber's theory:—

"Weber's theory differs from Poisson's in assuming that the molecules of the iron are always magnets, even before the application of the magnetising force, but that in ordinary iron the magnetic axes of the molecules are turned indifferently in every direction, so that the iron, as a whole, exhibits no magnetic properties." And again, page 429, Maxwell says he agrees with Weber's views, and that neutrality, or unmagnetised iron, has the axes of its molecules placed indifferently in all directions, and that the act of magnetisation consists in turning all the molecules so that their axes are either rendered all parallel to one direction, or at least deflected in that direction.

I have quoted these several theories which admit of the inherent polarity of the molecule, and in that respect they entirely agree with my own; but the induction balance at once shows that they are erroneous in the most important part, for my researches have proved that neutrality is perfectly symmetrical, that there is no case of neutrality where the axes of the molecules are turned indifferently in all directions, and that we cannot obtain perfect neutrality except when the molecules form a complete closed circuit of attraction.

I believe that a true theory of magnetism should admit of complete demonstration, that it should present no anomalies, and that all the known effects should at once be explained by it.

From numerous researches I have gradually formed a theory of magnetism entirely based upon experimental results, and these have led me to the following conclusions:—

1. That each molecule of a piece of iron, steel, or other magnetic metal is a separate and independent magnet, having its two poles and distribution of magnetic polarity exactly the same as its total evident magnetism when noticed upon a steel bar-magnet.

2. That each molecule, or its polarity, can be rotated in either direction upon its axis by torsion, stress, or by physical forces such as magnetism and electricity.

3. That the inherent polarity or magnetism of each molecule is a constant quantity like gravity; that it can neither be augmented nor destroyed.

4. That when we have external neutrality, or no apparent magnetism, the molecules, or their polarities, arrange themselves so as to satisfy their mutual attraction by the shortest path, and thus form a complete closed circuit of attraction.

5. That when magnetism becomes evident, the molecules or their polarities have all rotated symmetrically in a given direction, producing a north pole if rotated in that direction as regards the piece of steel, or a south pole if rotated in the opposite direction. Also, that in evident magnetism we have still a symmetrical arrangement, but one whose circles of attraction are not completed except through an external armature joining both poles.

6. That we have permanent magnetism when the molecular rigidity, as in tempered steel, retains them in a given direction, and transient magnetism whenever the molecules rotate in comparative freedom, as in soft iron.

Experimental Evidences.—In the above theory the coercive force of Poisson is replaced by molecular rigidity and freedom; and as the effects of mechanical vibrations, torsion, and stress upon the apparent destruction and facilitation of magnetism is well known, I will, before demonstrating the more serious parts of the theory, cite a few experiments to prove that molecular rigidity fulfils all the requirements of an assumed coercive force.

The influence of vibrations, torsion, or stress of any kind upon a magnetised steel or iron rod may be seen by striking with a wooden mallet rods of hard and soft steel, also hard and soft iron previously magnetised to a known degree. The tempered steel, owing to its molecular rigidity, will lose but 5 per cent., the soft steel 60, hard iron 50, and soft Swedish iron 99 per cent. of its magnetism, the amount of loss depending not so much upon whether the metal be steel or iron, as upon its degree of hardness and softness; and as hard steel requires far more power to magnetise it to the same force than iron, it is possible to imagine a steel so hard that its molecules could not rotate, and that consequently no magnetism could be manifested from a given inducing cause, whilst a perfectly soft iron would give the maximum effect, and instantly return to its previous state. From this we might in error suppose that soft Swedish iron could not

retain its magnetism, and that its natural state would be zero, or neutrality. The apparent disappearance of magnetism, however, is here due to the extreme freedom of motion of its molecules allowing them at once to follow the comparatively feeble directing force of the earth's magnetism. We can demonstrate this by feebly magnetising a rod of soft iron held vertically, so that its north pole is at the lower portion. Upon removing the inducing magnet, or electromagnetic coil, we find that the rod retains a powerful north polarity; but if magnetised in a contrary sense, then we have only *traces* of magnetism left upon the withdrawal of the inducing cause. To succeed in this experiment, as in all others where soft iron is mentioned, we should use the best Swedish charcoal iron, thoroughly annealed at high temperature.

We find, again, that rods of steel or iron will lose far less magnetism when vibrated in the magnetic dip, or vertically, when their north poles are at the lowest extremity, than when horizontal, or still less than when their poles are contrary to those of the earth's field, and also that they will acquire their maximum magnetism from a given exciting cause when held vertically as described, and the molecules allowed greater freedom of motion to obey the directing influence by vibrations, torsion, stress, or blows upon the iron. Any influence that would tend to give greater freedom of motion, such as heat or mechanical trepidations, gives a far higher magnetic force to the iron than could be obtained without these aids.

In order to render visible the effects of motion upon magnetism, we may take two glass tubes, or ordinary phials, of any length or diameter, say, 10 centimetres in length by 2 centimetres in diameter. If we now put iron filings in these tubes, leaving about one-third vacant, so as to allow complete freedom in the filings when shaken, we find that each tube, when magnetised, retains an equal amount of residual magnetism, and that this all disappears upon slightly shaking the tube. We are thus imitating the effects of vibration. But if in one of these tubes we pour melted resin (in fact, any slightly viscous liquid, such as petroleum, suffices), we then render these filings more rigid, and then we can no longer produce by shaking the disappearance of its residual magnetism. In pouring in petroleum we have apparently been introducing a strong coercitive force, but we know that it can only have the mechanical effect of rendering the iron filings less free to turn, and so comparatively rigid. If we desire to see the effect of torsion, we have only to shake the filings so that when the tube is held horizontally the vacant space is above, and rotate it slightly (but without shaking) about a horizontal axis. Its remaining magnetism instantly disappears upon rotation, although we evidently have not changed the longitudinal position of its particles. A similar effect takes place upon a soft iron rod, for if we magnetise it and observe its remaining magnetism, we find that upon giving a slight torsion to this wire its remaining magnetism instantly disappears—a similar effect to that in the rotating tube of iron filings. But if the iron is rendered more rigid by hammering, or steel rendered hard and rigid by tempering, torsions or vibrations have but little effect, as in the case of the filings rendered rigid as above mentioned. Thus we have no longer need of an assumed mysterious coercive force to account for the retention of magnetism, for once knowing the mechanical qualities of iron and steel and their degree of molecular rigidity or hardness, we can at once predict their retentive magnetic powers.[1]

Rotation of Inherent Polarised Molecules.—Torsion, as well as mechanical vibrations, has, as we have seen, a powerful influence in aiding the molecules to overcome their inertia, and thus aid them to rotate in the direction of the inducing influence; and we may thus polarise strongly a flat, soft iron rod by simply bending or vibrating it when held vertically, and if we measure the magnetic force obtained we shall notice that the force is strictly relative to the degree of softness of the iron. Thus, with hard steel we should obtain only *traces* of polarisation, whilst with extremely pure, soft Swedish iron we obtain the maximum of force. The bar of iron or steel, being held in the earth's magnetic field, of infinite size compared with the bar, and infinitely homogeneous, cannot deflect or weaken its surrounding field. Its lower portion, being north, apparently strengthens it by its reaction, whilst its upper, south, apparently weakens the field; but, as Maxwell has shown, "the two poles of each molecule are equal and opposite, consequently the sum of each molecule and the whole mass must be zero."

[1] "On the Molecular Rigidity of Tempered Steel," by Prof. D. E. Hughes, F.R.S. (*Proceedings Institution of Mechanical Engineers*, pp 72-79, January, 1883.)

We have a far greater induced polarity in iron or steel when the iron is in thin bars or small wires, and this we should expect, as the external molecules rotate directly under the influence of the earth's magnetism, whilst those forming the interior of the bar either rotate feebly, or, as in the case of very thick bars, actually act as an armature, preventing by their influence free rotation of the exterior molecules.

Thus, as the sum of the two and equal polarities in a bar of iron is zero, it is evident that its polarity must be inherent. I have some remarkably pure soft Swedish iron wire, one millimetre in diameter, and as its inherent polar force seemed great when held vertically in the earth's magnetic field, I measured in the induction balance this force compared with a similar column of the magnetic atmosphere which it displaced. The inherent polarity of this wire, simply rendered evident by the earth's magnetism, was 15,600 times greater than the column it displaced.

We cannot, either by induction, conduction, or concentration, produce a greater force in another body of similar displacement or size, otherwise we could easily create power from a feeble source. Thus the enormously greater magnetic power observed in iron than the same column of air which it displaces must be due to the *inherent* polarity of its molecules.

Amongst numerous bars of iron upon which I have experimented, one of ordinary hoop-iron, 2 centimetres wide, 40 centimetres long, and $1\frac{1}{2}$ millimetre thick, not softened, possesses sufficient molecular rigidity to be apparently uninfluenced by the earth's magnetism. When this rod is rendered neutral we have but feeble polarity—mere traces when it is held vertically under the earth's magnetic influence; but if we apply a few successive torsions or vibrations to it when thus held, we have at once several thousand times greater polarity than before. Now, if iron had the power of deflecting or concentrating the earth's magnetism upon itself, it should not require the mechanical aid to molecular rotation given to it by these torsions or vibrations. Thus we are forced to conclude at least the existence of the inherent polarity of the molecules; and, if we admit this, we must also as a necessary consequence, admit the rotation of these molecules, else we cannot explain why mechanical vibrations allowing freedom of motion should always produce the polarity in accordance with the directing cause. I have already shown that torsion and vibrations *per se* are apparently destructive of magnetism; consequently in this case Poisson's two fluids and Ampère's parallel currents should, according to their theory, be mixed or heterogeneous, whilst, according to the views I am sustaining, the polarised molecules should obey, as compass needles, any magnetic directing cause whenever sufficient molecular freedom of motion allows free rotation.

The inherent polarity of iron may again be observed by drawing a flat rod of soft iron over one or both poles of a permanent magnet. This rod will then be powerfully magnetised, its remaining magnetism, when separated from the magnet, being sufficiently powerful to strongly deflect a suspended direction needle. A few slight torsions or vibrations will then completely discharge it. Now, suppose this operation repeated successively many thousand times, if there was no inherent polarity we should have gradually drawn all the polarity out of the magnet, and discharged it into the atmosphere. Nothing of the kind takes place. The molecules of the iron are simply rotated each time, and the only energy in work expended or lost comes from the arm of the experimenter, and the energy required would be strictly in accordance with the molecular freedom, or softness and hardness of the iron and steel; thus, whilst soft iron could be easily polarised and discharged by mechanical torsions, hard tempered steel would require a far greater amount.

Dr. Warren de la Rue, F.R.S., kindly aided me in this part of the research by passing a current from his well-known chloride of silver battery through iron and steel wires. A condenser of 42·8 microfarad capacity, charged by 3·360 cells, was used. We passed this enormous electric charge longitudinally through the wires, and observations were made as to whether any change whatever was produced in their quality or inherent polarity, the result being that these wires gave exactly the same magnetic polarity from a given directing or inducing cause as before, being similar in nature and degree, consequently this enormous electric force had not changed or destroyed the original inherent polarity.

If the molecules possess inherent polarity and rotate upon their axes, similar to a series of compass needles having a slight

degree of frictional rigidity, then, upon passing one pole of a magnet above them, they would turn symmetrically in one direction, and drawing the same pole of the magnet in the contrary direction would rotate them, and they would then remain symmetrically in the opposite direction.

A precisely similar effect takes place in a soft iron rod, placed east and west a few inches above a direction needle. Upon drawing the south pole of a powerful natural magnet at a few centimetres distance above the wire from east to west, the north polarities of the molecules successively turn in the direction of west, following the attraction of the south pole, as previously seen on the small compass needles. The rod is now magnetised with its north pole west, as indicated by the direction needle below any portion of this rod. Upon passing the same south pole of the natural magnet in a contrary direction, the molecules all rotate, their north poles still turning successively to the south pole of the permanent magnet until its arrival at the end from which the first magnetisation commenced. The rod has now entirely changed its polarity, and its north pole is east.

This phenomenon is well known in the ordinary magnetisation of rod , where care is taken to draw the magnet always in a similar direction, or the poles would be reversed at each to and fro drawing. To account for this on Coulomb-Poisson's theory it would be requisite that, first, all the fluids be separated with their north fluids symmetrically in one direction, but on drawing back the magnet these fluids would have to mix together, the north fluid passing through its south fluid to be finally opposite to its previous position, its coercive force doing the double work of allowing both fluids to mix and pass through each other, and finally keep them entirely apart. Ampère's theory would require that from a haphazard arrangement the molecules should become symmetrically arranged upon the first passage of the magnet, then upon its reversed direction one-half of the electric elementary currents should successively revolve in a contrary direction to arrive at neutrality before, finally, the other half followed the direction of the first half, and now all these currents would be revolving in the opposite direction to that upon the first magnetisation. We thus see that both these theories, whilst resting altogether upon assumption, are extremely complicated and improbable.

We might suppose, from the theory which I am advocating, that upon the rotation of the molecules there would be some disturbance or mechanical trepidation ; and such is found to be the case, as first observed by Page and afterwards verified by Dr. Joule and De la Rive, in the molecular sounds produced in iron upon its magnetisation. Reis's first telephone was founded upon these sounds, and Du Moncel has made numerous researches upon this subject.

In the last of my experiments cited the sounds are too feeble to be heard, but by the application of the microphone these trepidations at once become audible.

That molecules of iron and other metals rotate with time, whose period becomes shortened by mechanical vibrations, is well known in metallurgy, the ultimate result being generally the passage from a fibrous condition, as in iron wires, to a high degree of crystallisation. For many years I employed a circular vibrating spring as the regulator of speed of my printing telegraph instrument, and although this spring was so regulated by means of a frictional break, or "Frein," as not to surpass its limits of elasticity, these springs were constantly breaking after a few days' use, and as a matter of urgent necessity I made special researches into the cause of this breaking after a few days' constant vibratory action. I found at the point of rupture a high state of crystallisation. Fibrous iron would thus become thoroughly crystallised and break in one day ; the number of vibrations for an instrument in constant use during 24 hours being 1,209,600. Thus we could roughly estimate the life of iron in the form of one of these springs at 1,000,000 vibrations. Copper crystallised in one hour, and all metals and alloys were inferior to steel, except aluminium bronze. The latter springs would stand six weeks' constant use, or some 50,000,000 of vibrations. I finally resolved this problem by spreading the amount of vibrating work over a spiral spring containing 3 metres of steel rod wound into the same space as previously held by the straight rod of 30 centimetres ; by this means the average life of these springs has become five years. Evidently the molecules of these fibrous springs must have rotated under the vibrations, in order to produce crystals. The same phenomenon is observed in axles of carriages receiving constant trepidations, large crystals being always found at the point of fracture. Again, if we rapidly magnetise and demagnetise an iron rod, we have the production of evident heat, due to the constant motion of its molecules.

Maxwell describes an experiment of Beetz, in which an exceedingly small filament of iron was deposited by electrotype, under the influence of a strong magnetic field, in order to arrive at the inherent polarity of comparatively few molecules, and, as its magnetic force was very great, he regards the experiment as conclusive. My own experiments show that we have far less external magnetic force from a solid bar than from a thin tube or flat bar of the same surface exposed to a limited exciting cause. We know that magnetism does not penetrate to a very great depth, and we also know that, if to a thin steel permanent magnet we place another piece unmagnetised, or, better still, a rod of soft iron, its external polarity is greatly reduced, consequently the external evidence of polarity is not a direct measure of the degree of rotation, nor of the total inherent polarity of its mass. We may have a great superficial *external* rotation superposed upon rotations of an opposite nature, as will be seen later ; and thus the internal molecules of a magnet often act more or less as an external armature in closing its circle of attractions.

I have stated my belief that the molecule itself possesses its inherent polarity, which, like gravity, is an endowed quality for which we have no more reason to suspect the cause to be elementary electric currents than that elementary currents should be the cause of gravity, chemical affinity, or cohesion, and its polar power of crystallisation, most of which are affected by an electric current. We have a certain analogy between electric currents and magnetism, but not so great as the analogy between the magnetic polarity of a molecule and its other endowed qualities.

Magnetism, like chemical affinity, cohesion, and crystallisation, has its critical points. Faraday discovered that at red-yellow heat iron instantly lost its apparent polar magnetic power, to be as instantly restored at red heat, the critical point varying in iron, steel, &c., and being the lowest in nickel. This would be difficult to explain upon Ampère's theory, as we should have to admit the instant destruction or cessation of the elementary currents, to be again restored at a few degrees less temperature. It would be equally difficult to explain under my view, if it did not belong to a whole class of phenomena due to the possession by the molecules of various endowed qualities, of which chemistry and all our means of research can only teach us their critical points, without attempting to explain why, for instance, iron has a greater affinity for oxygen than gold. We know that it is so ; we know that the molecules of all matter are endowed with certain qualities having certain critical points, and I can see no reason for separating their magnetic inherent polarity from their numerous other qualities.

(*To be continued.*)

METERS FOR POWER AND ELECTRICITY[1]

THE subject of this evening's discourse, "Meters for Power and Electricity," is unfortunately, from a lecturer's point of view, one of extreme difficulty ; for it is impossible to fully describe any single instrument of the class without diving into technical and mathematical niceties which this audience might well consider more scientific than entertaining. If then in my endeavour to explain these instruments and the purposes which they are intended to fulfil in language as simple and as untechnical as possible, I am not as successful as you have a right to expect, I must ask you to lay some of the blame on my subject and not all on myself.

I shall at once explain what I mean by the term "meter," and I shall take the flow of water in a trough as an illustration of my meaning. If we hang in a trough a weighted board, then when the water flows past it the board will be pushed back ; when the current of water is strong the board will be pushed back a long way ; when the current is less it will not be pushed so far ; when the water runs the other way the board will be pushed the other way. So by observing the position of the board we can tell how strong the current of water is at any time. Now suppose we wish to know, not how strong the current of water is at this time or at that, but how much water altogether has passed through the trough *during* any time, as for instance one hour. Then if we have no better instrument than the weighted board, it will be necessary to observe its

[1] Lecture at the Royal Institution, by Mr. C. Vernon Boys, March 2.

position continuously, to keep an exact record of the corresponding rates at which the water is passing, every minute, or, better, every second, and to add up all the values obtained This would of course be a very troublesome process. There is another kind of instrument which may be used to measure the flow of the water: a paddle-wheel or screw. When the water is flowing rapidly the wheel will turn rapidly; when slowly, the wheel will turn slowly; and when the water flows the other way the wheel will turn the other way, so that if we observe how fast the wheel is turning we can tell how fast the water is flowing. If now we wish to know how much water altogether has passed through the trough, the number of turns of the wheel, which may be shown by a counter, will at once tell us. There are therefore in the case of water two kinds of instruments, one which measures *at* a time, and the other *during* a time. The term meter should be confined to instruments of the second class only.

As with water so with electricity, there are two kinds of measuring instruments, one, of which the galvanometer may be taken as a type, which shows by the position of a magnet how strong a current of electricity is *at* a time, and the other, which shows how much electricity has passed *during* any time. Of the first, which are well understood, I shall say nothing; the second, the new electric meters and the corresponding meters for power, are what I have to speak of to-night.

It is hardly necessary for me to mention the object of making electric meters. Every one who has had to pay his gas bill once a quarter probably quite appreciates what the electric meters are going to do, and why they are at the present time attracting so much attention. So soon as you have electricity laid on in your houses, as gas and water are laid on now, so soon will a meter of some sort be necessary in order that the companies which supply the electricity may be able to make out their quarterly bills, and refer complaining customers to the faithful indications of their extravagance in the mysterious cupboard in which the meter is placed.

The urgent necessity for a good meter has called such a host of inventors into the field that a complete account of their labours is more than any one could hope to give in an hour. Since I am one of this host I hardly like to pick out those inventions which I consider of value. I cannot describe all, I cannot act as a judge and say these only are worthy of your attention, and I do not think I should be acting fairly if I were to describe my own instruments only and ignore those of every one else. The only way I see out of the difficulty is to speak more particularly about my own work in this direction, and to speak generally on the work of others.

I must now ask you to give your attention for a few minutes to a little abstract geometry. We may represent any changing quantity, as for instance the strength of an electrical current, by a crooked line. For this purpose we must draw a straight line to represent time, and make the distance of each point of the crooked line above the straight line a measure of the strength of the current at the corresponding time. The size of the figure will then measure the quantity of electricity that has passed, for the stronger the current is the taller the figure will be, and the longer it lasts the longer the figure will be, either cause makes both the quantity of electricity and the size of the figure greater and in the same proportion; so the one is a measure of the other. Now it is not an easy thing to measure the size of a figure: the distance round it tells nothing; there is, however, a geometrical method by which its size may be found Draw another line, with a great steepness where the figure is tall, and with a less steepness where the height is less, and with no steepness or horizontal where the figure has no height. If this is done accurately, the height to which the new line reaches will measure the size of the figure first drawn; for the taller the figure is, the steeper the hill will be; the longer the figure, the longer the hill; either cause makes both the size of the figure and the height of the hill greater, and in the same proportion; so the one is a measure of the other; and so, moreover, is the height of the hill, which can be measured by a scale, a measure of the quantity of electricity that has passed.

The first instrument that I made, which I have called a "cart" integrator, is a machine which, if the lower figure is traced out, will describe the upper. I will trace a circle; the instrument follows the curious bracket-shaped line that I have already made sufficiently black to be seen at a distance, the height of the new line measures the size of the circle, the instrument has squared the circle. This machine is a thing of mainly theoretical interest, my only object in showing it is to explain the means by which I have developed a practical and automatic instrument of which I shall speak presently. The guiding principle in the cart integrator is a little three-wheeled cart, whose front wheel is controlled by the machine. This, of course, is invisible at a distance, and therefore I have here a large front wheel alone. On moving this along the table, any twisting of its direction instantly causes it to deviate from its straight path; now suppose I do not let it deviate, but compel it to go straight, then at once a great strain is put upon the table which is urged the other way. If the table can move it will instantly do so. A table on rollers is inconvenient as an instrument, let us therefore roll it round into a roller, then on moving the wheel along it the roller will turn, and the amount by which it turns will correspond to the height of the second figure drawn by the cart integrator. If, therefore, the wheel is inclined by a magnet under the influence

FIG. 1.

of an electric current, or by any other cause, the whole amount of which we wish to know, then the number of turns of the roller will tell us this amount; or to go back to our water analogy, if we had the weighted board to show current strength, and had not the paddle-wheel to show total quantity, we might use the board to incline a disk in contact with a roller, and then drag the roller steadily along by clockwork. The number of turns of the roller would give the quantity of water. Instruments that will thus add up continuously indications at a time, and so find amounts during a time, are called integrators.

The most important application that I have made at present of the integrator described is what I have called an engine-power meter. The instrument is on the table, but as it is far too small to be seen at a distance, I have arranged a large model to illustrate its action. The object of this machine is to measure how much work an engine has done during any time, and show

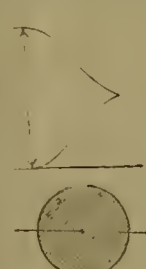


FIG. 2.

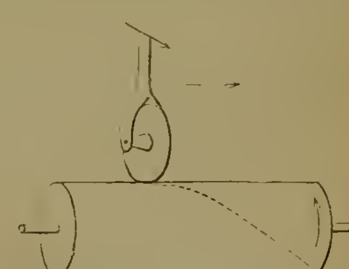


FIG. 3.

the result on a dial, so that a workman may read it off at once without having to make any calculations.

Before I can explain how work is measured, perhaps I had better say a few words about the meaning of the word "work." Work is done when pressure overcomes resistance, producing motion. Neither motion nor pressure alone is work. The two factors, pressure and motion, must occur together. The work done is found by multiplying the pressure by the distance moved. In an engine, steam pushes the piston first one way, then the other, overcomes resistance, and does work. To find this, we must multiply the pressure by the motion at every instant, and add all the products together. This is what the engine-power meter does, and it shows the continuously growing result on a dial. When the piston moves it drags the cylinder along, where the steam presses the wheel is inclined. Neither action alone causes the cylinder to turn, but when they occur together the cylinder turns, and the number of turns registered on a dial shows with mathematical accuracy how much work has been done.

In the steam-engine work is done in an alternating manner,

and it so happens that this alternating action exactly suits the integrator. Suppose, however, that the action whatever it may be, which we wish to estimate is of a continuous kind, such for instance as the continuous passage of an electric current. Then, if by means of any device, we can suitably incline the wheel, so long as we keep pushing the cylinder along, so long will its rotation measure and indicate the result; but there must come a time when the end of the cylinder is reached. If then we drag it back again, instead of going on adding up, it will begin to take off from the result, and the hands on the dial will go backwards, which is clearly wrong. So long as the current continues, so long must the hands on the dial turn in one direction. This effect is obtained in the instrument now on the table, the electric energy meter, in this way. Clockwork causes the cylinder to travel backwards and forwards by means of what is called a

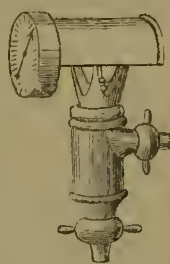


Fig. 4.

mangle motion, but instead of moving always in contact with one wheel, the cylinder goes forward in contact with one, and back in contact with another on its opposite side. In this instrument the inclination of the wheels is effected by an arrangement of coils of wire, the main current passing through two fixed concentric solenoids, and a shunt current through a great length of fine wire on a movable solenoid, hanging in the space between the others. The movable portion has an equal number of turns in opposite directions, and is therefore unaffected by magnets held near it. The effect of this arrangement is that the energy of the current, that is, the quantity multiplied by the force driving it, or the electrical equivalent of mechanical power, is measured by the slope of the wheels, and the amount of work done by the current during any time, by the number of turns of the cylinder, which are registered on a dial. Professors Ayrton and

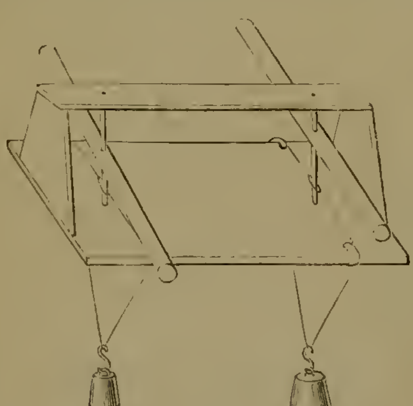


Fig. 5.

Perry have devised an instrument which is intended to show the same thing. They make use of a clock, and cause it to go too fast or too slow by the action of the main on the shunt current; the amount of wrongness of the clock, and not the time shown, is said to measure the work done by the current. This method of measuring the electricity by the work it has done is one which has been proposed to enable the electrical companies to make out their bills.

The other method is to measure the amount of electricity that has passed without regard to the work done. There are three lines on which inventors have worked for this purpose. The first, which has been used in every laboratory ever since electricity has been understood, is the chemical method. When electricity passes through a salt solution, it carries metal with it, and deposits it on the plate by which the electricity leaves the liquid. The amount of metal deposited is a measure of the quantity of electricity. Mr. Sprague and Mr. Edison have adopted this method; but as it is impossible to allow the whole of a strong current to pass through a liquid, the current is divided; a small proportion only is allowed to pass through. Provided that the proportion does not vary, and that the metal never has any motions on its own account, the increase in the weight of one of the metal plates measures the quantity of electricity.

The next method depends on the use of some sort of integrating machine, and this being the most obvious method, has been attempted by a large number of inventors. Any machine of this kind is sure to go, and is sure to indicate something, which will be more nearly a measure of the electricity, as the skill of the inventor is greater.

Meters for electricity of the third class are dynamical in their action, and I believe that what I have called the vibrating meter was the first of its class. It is well known that a current passing round iron makes it magnetic. The force which such a magnet exerts is greater when the current is greater, but it is not simply proportional; if the current is twice or three times as strong, the force is four times or nine times as great, or generally the force is proportional to the square of the current. Again, when a body vibrates under the influence of a controlling force, as a pendulum under the influence of gravity, four times as much force is necessary to make it vibrate twice as fast, and nine times to make it vibrate three times as fast; or generally the square of the number measures the force. I will illustrate this by a model. Here are two sticks nicely balanced on points, and drawn into a middle position by pieces of tape to which weights may be hung. They are identical in every respect. I will now hang a 1 lb. weight to each tape, and let the pieces of wood swing. They

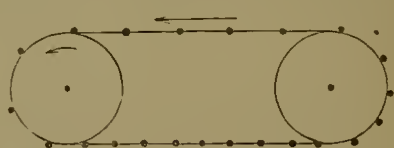


Fig. 6.

keep time together absolutely. I will now put 2 lbs. on one tape. It is clear that the corresponding stick is going faster, but certainly not twice as fast. I will now hang on 4 lbs. One stick is going at exactly twice the pace of the other. To make one go three times as fast, it is obviously useless to put on 3 lbs., for it takes 4 to make it go twice as fast. I will hang on 9 lbs. One now goes exactly three times as fast as the other. I will now put 4 lbs. on the first, and leave the 9 lbs. on the second; the first goes twice while the second goes three times. If instead of a weight we use electromagnetic force to control the vibrations of a body, then twice the current produces four times the force, four times the force produces twice the rate; three times the current produces nine times the force, nine times the force produces three times the rate, and so on; or the rate is directly proportional to the current strength. There is on the table a working meter made on this principle. I allow the current that passes through to pass also through a galvanometer of special construction, so that you can tell by the position of a spot of light on a scale the strength of the current. At the present time there is no current; the light is on the zero of the scale, the meter is at rest. I now allow a current to pass from a battery of the new Faure-Sellon-Volckmar cells which the Storage Company have kindly lent me for this occasion. The light moves through one division on the scale, and the meter has started. I will ask you to observe its rate of vibration. I will now double the current; this is indicated by the light moving to the end of the second division on the scale: the meter vibrates twice as fast. Now the current is three times as strong, now four times, and so on. You will observe that the position of the spot of light and the rate of vibration always correspond. Every vibration of the meter corresponds to a definite quantity of electricity, and causes a hand on a dial to move on one step. By looking at the dial, we can see how many vibrations there have been, and therefore how much electricity has passed. Just as the vibrating sticks in the model in time come to rest, so the vibrating part of the meter would in time do the same, if it were not kept going by an impulse automatically given to it

when required. Also, just as the vibrating sticks can be timed to one another by sliding weights along them, so the vibrating electric meters can be regulated to one another, so that all shall indicate the same value for the same current, by changing the position or weight of the bobs attached to the vibrating arm.

The other meter of this class, Dr. Hopkinson's, depends on the fact that centrifugal force is proportional to the square of the angular velocity. He therefore allows a little motor to drive a shaft faster and faster, until centrifugal force overcomes electromagnetic attraction, when the action of the motor ceases. The number of turns of the motor is a measure of the quantity of electricity that has passed.

I will now pass on to the measurement of power transmitted by belting. The transmission of power by a strap is familiar to every one in a treadle sewing-machine or an ordinary lathe. The driving force depends on the difference in the tightness of the two sides of the belt, and the power transmitted is equal to this difference multiplied by the speed; a power-meter must, therefore, solve this problem—it must subtract the tightness of one side from the tightness of the other side, multiply the difference by the speed at every instant, and add all the products together, continuously representing the growing amount on a dial. I shall now show for the first time an instrument that I have devised, that will do all this in the simplest possible manner. I have here two wheels connected by a driving band of indiarubber, round which I have tied every few inches a piece of white silk ribbon. I shall turn one a little way, and hold the other. The driving force is indicated by a difference of stretching, the pieces of silk are much further apart on the tight side than they are on the loose. I shall now turn the handle and cause the wheels to revolve; the motion of the band is visible to all. The indiarubber is travelling faster on the tight side than on the loose side, nearly twice as fast; this must be so, for as there is less material on the tight side than on the loose, there would be a gradual accumulation of the indiarubber round the driven pulley, if they travelled at the same speed; since there is no accumulation, the tight side must travel the fastest. Now it may be shown mathematically that the difference in the speeds is proportional both to the actual speed and to the driving strain; it is therefore a measure of the power or work being transmitted, and the difference in the distance travelled is a measure of the work done. I have here a working machine which shows directly on a dial the amount of work done; this I will show in action directly. Instead of indiarubber, elastic steel is used. Since the driving-pulley has the velocity of the tight side, and the driven of the loose side of the belt, the difference in the number of their turns, if they are of equal size, will measure the work. This difference I measure by differential gearing which actuates a hand on a dial. I may turn the handle as fast as I please; the index does not move, for no work is being done. I may hold the wheel and produce a great driving strain; again the index remains at rest, for no work is being done. I now turn the handle quickly, and lightly touch the driven wheel with my finger. The resistance, small though it is, has to be overcome; a minute amount of work is being done, the index creeps round gently. I will now put more pressure on my finger, more work is being done, the index is moving faster; whether I increase the speed or the resistance the index turns faster; its rate of motion measures the power, and the distance it has moved, or the number of turns, measures the work done. That this is so I will show by an experiment. I will wind up in front of a scale a 7 lb. weight; the band has turned one-third round. I will now wind a 28 lb. weight up the same height; the hand has turned four-thirds of a turn. There are other points of a practical nature with regard to this invention which I cannot now describe.

There is one other class of instruments which I have developed of which time will let me say very little. The object of this class of instruments is to divide the speed with which two registrations are being effected, and continuously record the quotient. In the instrument on the table two iron cones are caused to rotate in time with the registration; a magnetised steel reel hangs on below. This reel turns about, and runs up or down the cones until it finds a place at which it can roll at ease. Its position at once indicates the ratio of the speeds which will be efficiency, horse-power per hour, or one thing in terms of another. Just as the integrators are derived from the steering of an ordinary bicycle, so this instrument is derived from the double steering of the "Otto" bicycle.

Though I am afraid that I have not succeeded in the short time at my disposal in making clear all the points on which I have touched, yet I hope that I have done something to remove the very prevalent opinion that meters for power and electricity do not exist.

THE PERMIAN SYSTEM IN RUSSIA [1]

A QUESTION which has during the last few years occupied Russian geologists is whether the upper horizon of the "mottled marls," which were considered by Murchison as Permian, must be still regarded as such, or rather as a member of the Trias—an opinion strongly advocated by several eminent geologists in Russia. The question is a large one, the mottled marls being the most widely-spread member of Murchison's Permian formation in Russia, and covering it almost on the whole of the surface it occupies in Russia in Europe. Were the Triassic origin of the mottled marls an established fact, the whole aspect of a geological map of Russia would be changed, and so it was on the map published in 1870 by a late member of the Academy, M. Helmersen, and on the map of the western slope of the Ural Mountains by Prof. Meller. The question is thus the subject of much controversy, and a whole series of papers is devoted to it in the *Memoirs of the Kazan Society of Naturalists* and elsewhere. The last of this series is a paper by Prof. Stuckenberg, which states the present aspect of the question and enables us to summarise the controversy in its broad features.

Murchison's Permian system covers, as is known, no less than 6600 square miles in eastern Russia, from the province of Archangel in the north to that of Ufa in the south, and from Nijni-Novgorod in the west to Perm and Orenburg in the east; isolated islands of it appear on the surface in the provinces of Astrakhan, Kharkov, and Ekaterinoslav. The evidence itself of the basin where the Permian formation was deposited necessarily implies a great variety of lithological characters, and in fact it includes, besides the dolomitic limestones, a very great variety of marls, clays, sandstones, and conglomerates, the limestones occupying separate basins in the middle parts, whilst the marls, clays, sandstones, and conglomerates have the appearance of coast deposits of the Permian Sea.

In the central parts of the basin (Kazan, Samara), the dolomitic limestones are covered with a thick sheet of mottled marls, with sandstones, conglomerates, clays, and isolated thinner sheets of a tuff-like limestone. This series covers, however, not only the dolomitic limestone but also, as has been said, nearly the whole of the Permian deposits of European Russia, confounding itself with the Permian marls and sandstones, as is, for instance, the case—M. Stuckenberg says—in the provinces of Vyatka, Nijni-Novgorod, Kazan, and Samara. Palæontological evidence, however, is scarce as to the upper mottled marls, so that Murchison himself made the suggestion that they may belong perhaps to a more recent formation; he even proposed to give them on his map a lighter colour than the remainder of the Permian formation.

The mottled marls were considered as Permian until 1855, when Prof. Wagner published a geological map in which he classified them as Triassic. Later on, Marcou, in 1858, and Ludwig, in his "Dias and Trias," in 1859, arrived, independently of Prof. Wagner, at the same conclusion. In 1864 Prof. Barbot de Marny discovered in a sandstone belonging to this group a fragment of an *Equisetites columnaris*, Sternb. (*Calamites arenaceus*, Brongn.), and this discovery, confirming former stratigraphical and lithological considerations, induced the majority of Russian geologists to consider since the mottled marls as a part of the Trias. This view was adopted, as said, by Helmersen and by Prof. Meller. But still, as the mottled marls are very poor in organic remains, and the whole question beset with difficulties, the controversy continued. Murchison found in these marls small *Cytherinæ* and shells like *Cyclas*, together with some remains of fishes and casts of *Mytilus*. Prof. Golovkinsky discovered microscopic remains of crustaceans and some fragments of shells, whilst the late M. Eichwald found remains of *Estheria exigua* and *Beyrichia Pyrrhæ*, in a deposit which M. Stuckenberg considers as belonging to the same group. As to the find by Prof. Wagner of the *Voltzia heterophylla* at Abdi, close by Mamadysh, together with remains of the fishes *Amblypterus Alberti* and *Saurichthys Mougeoti*, M. Stuckenberg

[1] "The Upper Mottled Marls and their Relations to other Members of the Permian System," by A. Stuckenberg. (*Memoirs of the Kazan Societ of Naturalists*, vol. xi. fasc. 2; Kazan, 1882.

doubts, first, the accuracy of the determination, and adds that the *Voltzia* was not found in the mottled marls, but in deposits "parallel to the Permian limestone."

The Zechstein (dolomites, dolomitic limestones, oolite, and gypsum), which reaches a great thickness in the provinces of Kazan and Samara, is a formation which was contemporary with the Permian marls, sandstones, and conglomerates which are widely spread in the provinces of Kazan, Nijni, Vyatka, Perm, Ufa, and Orenburg. On the places where both meet together, the Zechstein penetrates in the shape of thinner sheets into the marls. The copper sandstones of the Ural also would be, according to the same author, contemporary with the Zechstein. These marls and sandstones have a characteristic fauna, and MM. Stuckenberg and Zaïtseff discovered in them the following fossils:—*Lingula orientalis*, Golovk.; *Unio umbonatus*, Fisch.; *Unio castor*, Eichw.; *Aucella Hausmanni*, Goldf.; *Estheria exigua*, Eichw.; *Beyrichia Pyrrhæ*, Eichw.; and remains of ganoid fishes and lizards. These fossils are characteristic of the group, but it contains also the Zechstein fossils, *Stenopora columnaris*, Schl., *Schizodus obscurus*, Gein., *Schizodus rossicus*, Vern., *Nucula Beyrichi*, Bron., *Murchisonia subangulata*, Vern., *Gervillia sulcata*, Gein., *Gervillia seratophaga*, Schl., *Hinnites* (*Avicula*) *speluncaria*, Schloth., *Arca Kingiana*, Vern., *Clidophorus Pallasi*, Vern., *Terebratula elongata*, Schl., *Productus Cancrini*, Vern., *Camarophoria Schlotheimi*, Buch., and *Spirifer rugulatus*, Kut. The flora of this series is characterised by many Coniferæ (among others, the *Uhlmannia Bronnii* and *brevifolia*) Noeggerathiæ (*expansa* and *cuneifolia*), ferns, &c. These deposits are thus Permian, and it is worthy of notice that they contain the *Unio umbonatus* and *castor*, the *Estheria exigua*, and the *Beyrichia Pyrrhæ*.

As to the upper mottled marls, which are precisely the subject of the controversy, there was discovered in them but a very few fossils, by MM. Krotoff and Stuckenberg, namely, the four just mentioned (*Unio umbonatus*, *Unio castor*, *Estheria exigua*, and *Beyrichia Pyrrhæ*), on the Volga at Tetushi, and the same in the Government of Vyatka, where the marls contain sheets of limestone; besides, M. Krotoff found Zechstein fossils, as *Arca Kingiana*, in the tuff-like limestone on the Volga, which M. Stuckenberg considers as belonging to the same series. Finally, there was discovered during a boring at Mount Bogdo (Astrakhan), in sandstones and conglomerates, a series of Permian fossils (*Matica minima*, Brown, *Turbonila volgensis*, Golowk,, *Gervillia antiqua*, Mün., *Clidophorus Hollebeni*, *Clidophorus Pallasi*, Vern., *Schizodus rossicus*, Vern., *S. obscurus*, Gein., *Nucula Beyrichi*, Brown, *Leda speluncaria*, Gein., and *Hinnites* (*Avicula*) *speluncaria*, Schloth. M. Stuckenburg, considering the Bogdo sandstones as contemporary with the upper mottled marls, gives to it great weight; but it must be observed that the contemporaneity of the Bogdo marls with the upper mottled marls of the Volga is all but established.

As to the palæontological evidence produced for considering the upper mottled marls as Triassic, namely, those found of the Triassic, *Equisetites columnaris* (*Calamites arenaceus*), *Voltzia heterophylla*, and *Estheria minuta*, M. Stuckenburg considers it unsatisfactory, and points out that the *Voltzia heterophylla* was found rather in Permian deposits; and that Mr. Jones, in his "Monograph of the Fossil Estheriæ," considers the *Estheria minuta* of the Russian mottled marls as different from the *E. minuta*, Brongn., and rather like to the *E. tenella* of Jordan, which last belongs to the Permian and Carboniferous of Western Europe. As to the *Calamites arenaceus*, found by Barbot de Marny, F. Römer, in the last edition of his "Lethea geognostica," remarks that it is too badly preserved to be a decisive evidence. He concludes, therefore, that contrary to the opinion of almost all Russian geologists, that the mottled marls ought to be considered again as Permian. But, as seen from the above summary, it will be much more prudent to conclude that the whole question still remains open for further investigation.

UNIVERSITY AND EDUCATIONAL INTELLIGENCE

CAMBRIDGE.—The Rede Lecture was delivered on Tuesday in the Senate House by Prof. Huxley, the subject being "The Origin of the Existing Forms of Animal Life, Construction or Evolution?" There were at least eleven hundred persons present, and amongst them nearly all the University dignitaries now in residence.

In the second part of the Natural Sciences Tripos sixteen men and one lady are placed in the first class; of this Mr. Harmer of King's College is distinguished in Zoology and Comparative Anatomy; Mr. Reid of Cavendish College in Human Anatomy; and Mr. Sharrington of Caius College in Physiology.

Prof. Hughes has been elected to a Professorial Fellowship at Clare College.

Messrs. P. Frost, I. Todhunter, and Joseph Wolstenholme are to receive the degree of Doctor in Science.

The Woodwardian Professor dissents strongly from the proposal to place the Sedgwick Museum on the Downing Street site in front of the new museums.

SCIENTIFIC SERIALS

THE *Journal of Anatomy and Physiology* for April, 1883, contains:—A contribution to the study of Spina bifida, encephalocele, and anencephalus, by Prof. Cleland (Plates 11 and 12).—On the minute structure of the palatine nerves of the frog, and the termination of nerves in blood-vessels and glands, by Prof. W. Stirling and J. F. Macdonald (Plate 13).—On the lymphatics of Periosteum, by Drs. George and F. Elizabeth Hoggan (Plate 14).—The brachial plexus of the macaque monkey, and its analogy with that of man, by W. T. Brooks.—A case of primary sarcoma of the pleura, by R. W. Greenish (Plate 15).—Infiltrating carcinoma of the breast, by Dr. G. Barling.—Observations of the diameters of human vertebræ in different regions, by Dr. R. J. Anderson.—On a simple form of Lippman's capillary electrometer useful to physiologists, by Prof. McKendrick.—On so-called sponge-grafting, by Drs. K. Franks and P. S. Abraham (Plate 16).—The valvular action of the larynx, by Drs. T. L. Brunton and T. Cash.—Origin of the internal circumflex from the deep epigastric artery, by Dr. A. Thomson.—On cervical ribs and the so-called bicipital ribs in man in relation to the corresponding structures in the Cetacea, by Prof. Turner.—On a new form of ether microtome, by Dr. Cathcart.—On right-sided sigmoid flexure and rectum, by Dr. E. E. Maddox.—A note to Mr. Haswell's paper on myology of pigeon.

THE *Quarterly Journal of Microscopical Science* for April, 1883, contains:—On the anatomy and development of *Peripatus capensis*, by the late Prof. F. M. Balfour, edited by Professors Moseley and Sedgwick (Plates 13 to 20).—On a morphological variety of *Bacillus anthracis*, by Dr. E. Klein, with notes thereon by Prof. Ray Lankester (Plate 21).—Note on a pink Torula, by H. Marshall Ward (Plate 22).—On double staining nucleated blood corpuscles with anilin dyes, by Dr. V. Harris.—Some recent researches on the continuity of the protoplasm through the walls of vegetable cells, by W. Gardiner.—Review of recent researches on Spermatogenesis, by J. E. Bloomfield.—Note on a minute point in the structure of the spermatozoon of the newt, by G. F. Dowdeswell.—On the existence of Spengel's olfactory organ and of paired genital ducts in the pearly nautilus, by Prof. Ray Lankester and A. G. Bourne.

SOCIETIES AND ACADEMIES

LONDON

Royal Society, April 12.—"On a New Crinoid from the Southern Sea." By P. Herbert Carpenter, M.A., Assistant Master at Eton College. Communicated by W. B. Carpenter, C.B., M.D., F.R.S.

Among the collections of the late Sir Wyville Thomson, a small *Comatula* has recently been discovered which was dredged by the *Challenger* at a depth of 1800 fathoms in the Southern Sea. Although it is unusually small, the diameter of the calyx being only 2 mm., the characters presented by this form are such as to render it by far the most remarkable among all the types of recent Crinoids, whether stalked or free. The name proposed for it is *Thaumatocrinus renovatus*.

But it is distinguished by four striking peculiarities:—

(1.) The presence of a closed ring of basals upon the exterior of the calyx.

(2.) The persistence of the oral plates of the larva, as in *Hyocrinus* and *Rhizocrinus*.

(3.) The separation of the primary radials by interradials which rest on the basals.

(4.) The presence of an arm-like appendage on the interradial plate of the anal side.

Taking these in order—

(1.) No adult *Comatula*, except the recent *Atelecrinus* and

some little-known fossils, has a closed ring of basals; and even in *Atelecrinus* they are quite small and insignificant.

(2.) In all recent *Comatulæ*, in the *Pentacrinidæ*, and in *Bathycrinus*, the oral plates of the larva become resorbed as maturity is approached. In *Thaumatocrinus*, however, they are retained, as in *Hyocrinus*, *Rhizocrinus*, and *Holopus*, representatives of three different families of Neocrinoids.

(3.) There is no Neocrinoid, either stalked or free, in which the primary radials remain permanently separated as they are in *Thaumatocrinus*, and for a short time after their first appearance in the larva of ordinary Crinoids. The only Palæocrinoids presenting this feature are certain of the *Rhodocrinidæ*, *e.g.* *Reteocrinus*, *Rhodocrinus*, *Thylacocrinus*, &c. In the two latter and in the other genera which have been grouped together with them into the section *Rhodocrinites* there is a single interradial intervening between every two radials, and resting on a basal just as in *Thaumatocrinus*. But in the Lower Silurian *Reteocrinus* the interradial areas contain a large number of minute pieces of irregular form and arrangement.

(4.) It is only, however, in *Reteocrinus* and in the allied genus *Xenocrinus*, Miller, which is also of Lower Silurian age, that an anal appendage similar to that of *Thaumatocrinus* is to be met with.

Of the four distinguishing characters of *Thaumatocrinus*, therefore, one appears in one or perhaps in two genera of *Comatulæ*; another is not to be met with in any *Comatula*, though occurring in certain stalked Crinoids; while the two remaining characters are limited to one family of the Palæocrinoids, one of them being peculiar to one or at most two genera which are confined to the Lower Silurian rocks.

Their reappearance in such a specialised type as a recent *Comatula* is therefore all the more striking.

Geological Society, May 23.—J. W. Hulke, F.R.S., president, in the chair.—Ernest Le Neve Foster and Richard Bullen Newton were elected Fellows of the Society.—The following communications were read:—On the basalt glass (tachylyte) of the Western Isles of Scotland, by Prof. J. W. Judd, F.R.S., Sec.G.S., and G. A. J. Cole, F.G.S. Basalt glass or tachylyte is a rare rock, although very widely distributed. In the Western Isles of Scotland it has, by the authors of the paper, been detected in five localities only, namely, Lamlash (Holy Isle) near Arran, the Beal near Portree in Skye, Gribun and Sorne in Mull, and Screpidale in Raasay. Basalt glass is always found in the Hebrides as a selvage to dykes, though elsewhere it has been described as occurring under other conditions where rapid cooling of basaltic lava has taken place. Some of the varieties of basalt glass in the Hebrides differ from any hitherto described by their high specific gravity (2·8 to 2·9) and by their low percentage of silica (45 to 50). This basalt glass is frequently traversed by numerous joints; it is occasionally finely columnar, and sometimes perlitic in structure. From the acid glasses (obsidian) it is distinguished by its density, its opacity, its magnetic properties, and especially by its easy fusibility, from which the name of tachylyte is derived. By its greater hardness it is readily distinguished from its hydrated forms (palagonite, &c.). In its microscopic characters basalt glass is found to resemble other vitreous rocks; thus it exhibits the porphyritic, the banded and fluidal, the spherulitic, and the perlitic structures. In the gradual transition of this rock into basalt, all the stages of devitrification can be well studied. The difference between these locally developed basalt glasses and the similar materials forming whole lava-streams in the Sandwich Islands was pointed out in the paper, and the causes of this difference were discussed. It was argued that the distinction between tachylyte and hyalomelane, founded on their respective behaviour when treated with acids, must be abandoned, and that these substances must be classed as rocks and not as mineral species; the name basalt glass was adopted as best expressing their relations to ordinary basalt, the term tachylyte being applied to all glasses of basic composition and being used in contradistinction to obsidian.—On a section recently exposed in Baron Hill Park, near Beaumaris, by Prof. T. G. Bonney, F.R.S., Sec.G.S. The author, about three years since, observed some imperfect exposures of a felsitic grit in the immediate vicinity of the normal schists of the district in a road which leads from Beaumaris cemetery to Llandegfan; but last summer had the opportunity, through the courtesy of Sir R. B. Williams, of examining the cuttings made in constructing a new drive, which runs through Baron Hill Park, very near the above outcrops. After tracing the normal schists along the steep scarp of the hill, he came, after an interval of about 60 yards, covered by soil and vegetation, to a massive gray grit consisting of quartz, felspar, and minute fragments of compact felsite, which now and then attain a larger size, being an inch or so across. These grits, which pass occasionally into hard compact mudstones (probably more or less of volcanic origin), can be traced for some 350 yards to the neighbourhood of the above-mentioned road, which is crossed by a bridge; and a short distance on the other side of this is a considerable outcrop of the grit, which in places becomes coarsely conglomeratic, containing large fragments of the reddish quartz-felsite so common on the other side of the straits, in the beds at or below the base of the Cambrian series. The schists appear to dip about 20° E.S.E., the grits about 25° E. The author, after describing the microscopic structure of the various rocks noticed, pointed out that this section, though the junction of the two rocks is probably a faulted one, has an important bearing on the question of the age of the Anglesey schists, micaceous and chloritic. The Survey regards them as altered Cambrian; it has even been suggested that they may be of Bala age; others have regarded them as Pebidian. Now the felsitic grits and conglomerates cannot be newer than the Cambrian conglomerate of the mainland, regarded by Prof. Hughes as the base of the true Cambrian, and are probably older, corresponding with some part of the series between it and the great masses of quartz-felsite which are developed near Llyn Padarn and Port Dinorwig, which series lithologically and stratigraphically corresponds with the typical Pebidian of Pembrokeshire. Hence, as the Anglesey schists are in the full sense of the term metamorphic rocks, and the "Pebidian" but slightly altered, this section shows that the former must be much older than the latter, and so be distinctly Archæan.—On the rocks between the quartz-felsite and the Cambrian series in the neighbourhood of Bangor, by Prof. T. G. Bonney, F.R.S., Sec.G.S. This district has already been the subject of papers by the author (*Quart. Journ. Geog. Soc.*, vol. xxxiv. p. 137), and by Prof. Hughes (vol. xxxv. p. 682), who differs from him in restricting the series between the quartz-felsite and Cambrian conglomerate to little more than the bastard slates and green breccias of Bangor mountain. The author has traced on the south-east side of the Bangor-Caernarvon road a well-marked breccia containing fragments of purple slate mixed with volcanic materials, below the above-named Bangor series, for more than a mile. At a lower level he has traced another well-marked breccia, chiefly of volcanic materials, for half a mile; and, lastly, a grit and conglomerate, apparently resting on the quartz-felsite named above, composed of materials derived from it. This has been traced on both sides of the road mentioned above for nearly two miles. For these and for other reasons given in the paper, the author is of opinion that, as he formerly maintained, there is a continuous upward succession on the south-east side of the road, from the quartz-felsite at Brithdir to the Cambrian conglomerate on Bangor mountain. The district on the north-west side of the road is so faulted that he can come to no satisfactory conclusions. The author is in favour of incorporating the above-named quartz-felsites with the overlying beds as one series, corresponding generally with the Pebidian of South Wales; older than the Cambrian, though probably not separated from it by an immense interval of time. An analysis of the Brithdir quartz-felsite by Mr. J. S. Teall, was given, from which it appeared that the rock corresponds very closely with the "devitrified pitchstone" of Lea rock in the Wrekin district, described by Mr. Allport, but differs considerably in composition from those in the Ordovician rocks of North Wales.

EDINBURGH

Royal Society, May 21.—Mr. Robert Gray, vice-president, in the chair.—Obituary notices were read of Sheriff Hallard, Dr. John Muir, Friedrich Wöhler, Sir J. Rose Cormack, M.P., and Dr. Morehead. Mr. John Aitken, in a note on the moon and the weather, observed that the phenomenon of the old moon in the new moon's arms was not always visible in a very clear atmosphere, but that other meteorological conditions seem to be requisite. He suggested that the earthshine might be greatly intensified by a cloud-laden atmosphere to the west of the observer.—Mr. D. B. Dott read a paper on the acids of opium, in which he stated that, contrary to the general opinion, the principal acid in opium, judged by its acidifying powers, is sulphuric and not meconic acid, a considerable portion of the

morphia being always combined with the sulphuric acid.—Prof. Tait gave some results of direct observations of the effect of pressure on the maximum density-point of water. The experiments were a modification of the well-known Hope experiment. A glass vessel of water with a self-registering thermometer at the bottom and a mass of ice at the top was placed inside the water (at 50° F.) of the large hydraulic press. Under a pressure of 3 tons weight per square inch, the thermometer fell to 33° F., whereas at the ordinary atmospheric pressure, but under otherwise similar circumstances, the temperature registered never fell below 41° F.

PARIS

Academy of Sciences, May 21.—M. E. Blanchard, president, in the chair.—Observations of the small planets made with the large meridian of the Paris observatory during the first quarter of the year 1883, communicated by M. Lœwy.—On the critical point of soluble gases, by J. Jamin. The critical point is defined to be the temperature at which a liquid and its saturated vapour have the same density. At and beyond this point the fluid and vapour become fused in one, and all latent heat disappears.—Extract from a memoir on the composition of combustible mineral substances, by M. Boussingault. The proportions are given of the carbon, hydrogen, oxygen, and nitrogen contained in the substances analysed—the bitumen of the Chinese fire-pits and Dead Sea, the Egyptian asphalt, fossil resins, and the anthracites and other coals of South America and France. A method is proposed for eliminating the hydrogen, oxygen, and other impurities from graphite, and thus reducing it, like the diamond, to pure carbon.—The scientific expedition of the *Ta'isman* to the Atlantic Ocean, by M. Alphonse Milne-Edwards. The *Talisman* sailed from Rochefort on June 1, and will visit the Canaries, Cape Verd Islands, Azores, and intermediate waters.—On the discussion recently raised by the Commission of the Turin Veterinary School, touching the state of the blood of a sheep subjected to carbonic vaccination when examined within a few hours of death and the day after death, by M. Pa teur.—Note by Admiral Pâris accompanying the presentation of his work on the "Naval Museum in the Louvre."—A new method of synthesis for the alkylnitrous acids, by G. Chancel.—On the part respectively played by oxygen and heat in attenuating the carbonic virus by the method of M. Pasteur, by M. A. Chauveau.—On the treatment of the water used in wool-washing, by MM. Delattre. This water yields 4·50 per 100 of a very dry potassium, or a total of 270,000 kilograms, valued at 4300*l.*, on the 6,000,000 kilograms of wool annually washed in France. But from this must be deducted 1000*l.* for the cost of treatment.—On the algebraic functions of Fuchs, by M. H. Poincaré.—On the theory of Euler's integrals, by M. Bourguet.—On the resistance of the atmosphere in very slow oscillatory movements, by M. J. B. Baille.—On the deformation of polarised electrodes, by M. Gouy.—On the electrodynamic interference of alternant currents, by M. A. Oberbeck.—On the sesquisulphuret of phosphorus, by M. Isambert.—On some double salts of lead, by M. G. André.—On the solubility of strychnine in acids, by MM. Hanriot and Blarez.—On a saccharine substance extracted from the lungs and phlegm of consumptive patients, by M. A. G. Pouchet.—On a deposit of quaternary mammals in the neighbourhood of Argenteuil (Seine-et-Oise), by M. Stan. Meunier. Here the author recently discovered remains of the elephant, *Rhinoceros tichorhinus*, cave hyæna, horse, reindeer, and a member of the ox family, apparently *Bison priscus*.—Vegetation of the vine at Calèves, near Nyon, Switzerland, by M. Eugène Risler.

June 4.—M. Blanchard in the chair.—The following papers were read :—On the possibility of increasing in a great measure the precision of the observations of the eclipses of Jupiter's satellites, by A. Cornu.—On the solubility of sulphide of copper in alkaline sulphomolybdates, by M. Debray.—On the solution of certain problems of cosmography by means of graphic tables, by MM. Lalanne and Ed. Collignon.—M. A. Caligny presented to the Academy his work entitled : "Theoretical and Experimental Researches concerning the Oscillations of Water, and the Hydraulic Machines with Oscillating Liquid Columns."—On recent scientific results obtained regarding the etiology of and preventatives from cholera, by A. Fauvel.—Researches on typhoid fever in Paris during the period October 19, 1882, to May 15 a.c., by M. de Pietra-Santa.—On an apparatus for obtaining low temperatures which can be graduated at pleasure, by P. Gibier.—On the hyposulphides of phosphorus, by M. Isambert.—On the sesquisulphide of phosphorus, by G. Lemoine.—Reply to M. le Goarant de Tromelin regarding the electric log, by M. Fleuriais.—On glass-blowing by means of mechanically-compressed air, by M. Appert.—On the observations of Brooks-Swift comet (*a* 1883) made at Paris Observatory, by G. Bigourdan.—On the development of the perturbating function, by B. Baillaud.—On the uniform functions of two analytical points which are left invariable by an infinity of rational transformations, by M. Appell.—On uniform functions, by J. Farkas.—On a correction of the stereotyped formula in the preface of Callet, by M. Em. Barbier.—Practical rules for substituting certain closed curves for a given arc, by H. Léauté.—On passive mechanical power, interior resistance, and other points relating to electro-magnetism, by G. Cabanellas.—On the freezing point of acid solutions, by F. M. Raoult.—Comparison of the evaporation of fresh water and sea water at different degrees of concentration ; consequences relating to a sea in the interior of Algeria, by M. Dieulafait.—Notes on the preceding communication, by M. Jamin.—Thermical studies on the solution of hydrofluoric acid in water, by M. Guntz.—On the transformation of glycol into glycolic acid, by M. de Forcrand.—Researches on the production of crystallised borates in the wet way, by A. Ditte.—On the reaction of sulphide of lead upon metallic chlorides, by A. Levallois.—On the burning of gypsum, by H. le Chatelier.—On an acid resulting from the oxidation of strychnia, by M. Hanriot.—On the life-capacity of the monstrous embryos of chickens, by M. Dareste.—On the artificial production of the inversion of the viscera or "heterotaxy" in chicken embryos, by MM. Hermann Fol and St. Warynski.—Observations on blastogenesis and alternating generation in Salpa and Pyrosoma, by L. Joliet.—On the localisation of virus in wounds and on the mode of its dissemination in the organism, by G. Colin.—Experimental researches on the lesion of the spinal marrow, determined by the hemisection of that organ, by E. A. Homén.—On the mechanical organisation of the pollen-grain, by J. Vesque.—Note on the life and work of Prof. da Costa Simoïdes of Coimbra, by Eduardo Abreu.—On a method of utilising sewage water, by MM. Delattre and Pinot.

CONTENTS

THURSDAY, JUNE 21, 1883

"THE NEW PRINCIPLES OF NATURAL PHILOSOPHY"

The New Principles of Natural Philosophy. By W. L. Jordan, F.R.G.S. (London: David Bogue, 1883.)

IN the Preface to this large and handsome volume we are told that "The Third Chapter, ... and more especially the first nine Parts of that chapter, are the justification for the title of this work." This sort of intimation is unusual, but timely and useful, as it enables us to go at once to the root of the m tter, and to study "*The New Principles*" in themselves, before we commence the perusal of the formidable array of arguments, examples, and demonstrations which constitutes the bulk of the volume. The chapter referred to is formally dedicated to the memory of Descartes and Newton, "as it shows the connexion between the Cartesian Vortices and the Newtonian Laws (*sic*) of Gravitation."

The peculiar employment of the definite article in the title of the work at once arrests the attention of the reader. New principles in Natural Philosophy, some of them coextensive with our whole knowledge of the subject, have been happily introduced even in comparatively recent times. The Conservation of Energy, and Carnot's principle of Reversibility, are notable examples. But such principles consist of generalisations of our former knowledge, or additions to it, and are in no way subversive of the fundamental Laws of Motion (or *Axiomata*) as they were systematized by Newton.

"*The New Principles*," however, are not of this class. They involve, essentially, somewhat extensive modifications of Newton's Laws, and the consequences we have been accustomed to draw from them. So far as we understand our author, he seems to allow that Newton and his followers have correctly deduced the consequences which follow from the assumption of the Laws of Motion and the principle of Gravitation. But the accordance of the results, so deduced, with observed facts is a remarkable coincidence only:—arising from a compensation of errors, where incorrect ideas as to the laws of motion are rectified (so far as results go) by an omission of some of the more powerful causes which are really at work. Thus the true statement of the First Law of Motion is that a body gradually comes to rest when the motive force ceases to act; while "Gravitation is merely the act by which the material universe resists, or endeavours to resist, the motive forces acting on it. And, therefore, if matter were not inherently inert there would be no such force as gravitation" (pp. 306-7). It follows, of course, from the new principles that some cause, hitherto not taken into account, is required to explain the persistence of the earth's rotation and of its revolution in its orbit. Of course this cause must suffice for the same results in the case of each planet; and it must also maintain the rotation of the sun itself. This is found in "Astral Gravitation." How so immensely effective a factor in all the physics of the universe has hitherto been entirely overlooked it is not for us to say. We content ourselves with a humble effort to disseminate "*The New Principle*" as widely as our circulation permits, if not as widely as its intrinsic importance demands. We would merely premise that our author distinctly points out that, after all, Astrology was wrong only because its votaries recognised "personal influences," whatever these may be, and not the gigantic physical forces exerted on us and our surroundings by the stars:—

"Reason also tells us that whatever the apparent magnitude of any given star may be, the greater its actual distance from the earth, the greater is the force which it exerts on the earth; and therefore the argument that the stars are too far off for their power to be felt, even if it merit the designation of common sense, is absolutely refuted by reason.

"It is not possible in the present state of knowledge to make a reliable (*sic*) estimate regarding the actual force exerted by any star.

"Sir William Herschel assures us that the star Capella has an apparent diameter of 2½ seconds. Its distance as indicated by parallax is 4½ million times greater than that of the sun.

"Accepting these measurements as accurate, and supposing Capella to be of the same density as the sun, the force of gravitation which it exerts on the earth is equal to one hundredth part of that exerted by the sun. If, however, any star of equal apparent magnitude whose distance is so great as not to be indicated by parallax, be as many times more distant than Capella as Capella is more distant than the sun, then the force exerted on the earth by that star, according to the foregoing measurements, would be 45,000 times greater than that exerted by the sun."

"It is argued that none of the stars can have so great an apparent diameter as asserted by Herschel, because Huygens has estimated that the sun gives us 2,000 million times more light than we receive from Capella, and this, supposing the two bodies to be of equal brilliancy, would make Capella much smaller than estimated by Herschel. For a star of the apparent size estimated in this manner to exert as much force on the earth as is exerted by the sun, its distance would have to be 20 million times greater than the actual distance of Capella as indicated by parallax.

"Neither of these estimates can be regarded as better than vague guesses at the real size of the stars; but when, on the one hand, we consider the evidence which seems to necessitate the existence of some great controlling force of gravitation far distant from our solar system, and, on the other hand, the fact that the laws of optics and of gravitation make it quite possible that some one of the visible stars may actually be exerting a force far more than sufficient for the purpose indicated, reason is almost forced to the conviction that stars of the requisite magnitude do exist in the heavens."

How this, and "The" other "New Principles" account for Trade Winds, Ocean Circulation, Comets, The Zodiacal Light, The Secular Acceleration of the Moon's Motion, &c., must be learned from an attentive perusal of the work itself. The careful reader will have an ample and varied treat; for mixed with these weighty contributions to science, we have full details of a more strictly human character, such as "Replies to Critics," "Remarks on the Admiralty Current Charts," "A Public Challenge to the Council of the Royal Society," &c. We quote (from p. 365) a few words to show how very serious indeed is the state of matters with our great scientific Society.

"I last year publicly challenged the leaders of the scientific world in London to open discussion; and, in the second of the public lectures I then gave, I made it clear that the question at issue had ceased to be merely a

question of scientific opinion but had become also one of honour. And until the Council of the Royal Society take measures to refute or to atone for (!) the charges I then made, it is evident that courtesy and chivalrous conduct are at a discount in the scientific world, and it is not surprising that the deterioration of the tone of thought should be such as to have at length attracted the attention of the editor of the *Times*."

At p. 486 we have again the old question of the moon's non-rotation about its axis. This latest follower of Mr. Jellinger Symons gives a new and rather amusing argument in support of the heresy. For he says it would require us to suppose that a ship which sails round the world must have turned a complete somersault, while it is quite obvious that she has not done so!! Here we fear we must part company with our amusing instructor; and, though to many it may appear the blackest ingratitude, we must conclude with a hearty wish that Mr. Jordan's work had been published some twenty years sooner. Had it been then given to the world it would, like a fly in amber, have secured immortality in the pages of De Morgan's unique, because inimitable, *Budget of Paradoxes*.

It has many of the distinctive charms of the celebrated works of Mr. James Smith (of Liverpool), Mr. James Reddie, and Baron von Gumpach. All these great men, in their turn, tilted at Philosophers or Scientific Bodies, the Astronomer-Royal, the Royal Society, the British Association, &c., and complained, as Mr. Jordan now does, of the bigotry and malevolence in high places which depreciated the value of the gifts they were bestowing on the world. Some of them were hopelessly illogical and stupid, others merely ignorant. Mr. Jordan appears to belong to the second category. He is evidently untaught, though presumably not unteachable. But he should not attempt to teach.

P. G. T.

THE BRITISH MUSEUM CATALOGUE OF BATRACHIA

Catalogue of the Batrachia gradientia s. caudata, and B. apoda in the Collection of the British Museum. Second Edition. By G. A. Boulenger. (London: By Order of the Trustees, 1882.)

THIS volume completes the second edition of the Catalogue of the Batrachians in the British Museum. The former volume, which appeared in the spring of last year, we have already noticed. The first edition (1850) was prepared by Dr. J. E. Gray, and contained descriptions of 72 species; 132 species are described in the present work, the great majority of which have been actually examined by Mr. Boulenger. The number of species of tailed Batrachians in the British Museum collection now amounts to 78 against 38 in 1850. Several of the species which are wanting in the collection are natives of America, and in the interests of science we hope some of the distinguished herpetologists of the New World will generously supply these desiderata. The number of footless Batrachians in the Collection is 19 against 5 in 1850, and in this group also nearly all the species unrepresented in the national collection are natives of America. In addition to very full synopses of the families and subfamilies, of detailed diagnoses of the genera and species with synonyms, we have appended to this volume a summary of the principal facts of the geographical distribution of Batrachians generally, which adds immensely to the value of this catalogue to the general biologist. Of the various primary geographical divisions which have been proposed, Mr. Boulenger finds that that recognised by Dr. Günther for freshwater fishes into Northern Equatorial and Southern Zones, agrees best with the facts deducible from the study of Batrachians, but with one modification, for a Southern Zone does not exist for Batrachians. Tasmania and Patagonia do not differ in any point regarding their frog-fauna from Australia and South America respectively. The following are the principal conclusions:—(1) In the Northern Zone there is an abundance of tailed and an absence of footless forms. A. In the Old World division (Europo-Asiatic or Palæarctic Region) there are numerous Salamandrinæ, with a single exception an absence of Hylidæ, but Discoglossidæ are present. B. In the North American division we find Sirenidæ, few Salamandrinæ, Plethodontinæ, Amblystomatinæ, and Hylidæ numerous, Desmognathinæ. (2) In the Equatorial Southern Zone there is an absence of tailed Batrachians and an equally characteristic presence of footless forms. Dividing (A) the Old World region into Indian and African, we find in both the frogs numerous (260 species out of 300), an absence of Hylidæ and Cystignathidæ, while in the former there are no Aglossæ or Dendrobatidæ, while in the latter there are Dactylethridæ or Dendrobatidæ. Dividing (B) the New World region into Tropical America and Australia, the former is rich in footless forms (21 species), has very many tailless forms, some small families quite peculiar to the region, but above all is it rich in the Arcifera, it has also a few tailed forms; the latter is divided into three subregions: the Australian proper is chiefly remarkable for a negative character, there are no footless or tailed forms, almost no toads or frogs, its fauna consisting mainly of the two families, Cystignathidæ and Hylidæ; the Austro-Malayan subregion presents an interesting blending of Indian and Australian forms, a curious fact is the occurrence, according to Peters, of a third species of the African genus Phrynomantis in Amboyna and Batavia, New Caledonia does not yield a single Batrachian; the third or New Zealand subregion possesses but a single species, *Liopelma hochstetteri*, very curiously a member of the family Discoglossidæ, which is otherwise restricted to the Europo-Asiatic region. The following new species are described for the first time, and there are excellent illustrations of most of them in the plates accompanying this volume: *Hynobius peropus*, China and Japan, *Spelerpes yucatanicus*, Yucatan, *Uræotyphlus africanus*, West Africa, *Hypogeophis guentheri*, Zanzibar, *Dermophis albiceps*, Ecuador, and *Chthonerpeton petersii*, the Upper Amazon.

The keeper of the Department of Zoology in the British Museum may be congratulated on the Batrachian collection having held pace with the progress made in this branch of science during the last thirty years.

OUR BOOK SHELF

The Cinchona Planter's Manual. By T. C. Owen. (Colombo: A. M. and J. Ferguson, 1881.)

FEW plants have been so fortunate or unfortunate in having so much written about them as the *Cinchonas*. Ever since their successful introduction into India, now

some twenty years since, the Cinchonas have had showered upon them books and pamphlets innumerable, and where we find such voluminous writings, it would be strange indeed were there not matter of varied quality, and some that could be dispensed with altogether. Mr. Owen's book is very complete in the several branches of Cinchona literature, facts gathered from various authentic sources, such as the works of Dr. King, Dr. Bidie, Mr. McIvor, and the reports of the Indian and Javan Governments, all of which are acknowledged by the author.

The book is divided into six parts, the first part being devoted to the physiology of plants, gathered, as we are told, from Church and Dyer's "How Crops Grow." The second part treats of the alkaloids, the species and varieties, to which a large space is given, and the next part on the choice of land, felling, clearing, weeding, planting, &c. In the fourth part manuring and harvesting are considered; and in parts 5 and 6 the diseases to which Cinchonas are liable, and the estimates of Cinchona planting are digested. In all these matters careful details are given.

The book no doubt will be very useful to Cinchona planters, more particularly the practical part. Its greatest fault, perhaps, is the extent of the book, numbering 203 pages, too voluminous for many planters to wade through; but on the other hand it appeals also to those who, though not actual planters, are interested in the progress of the Cinchona culture.

Kallos, a Treatise on the Scientific Culture of Personal Beauty and the Cure of Ugliness. By a Fellow of the Royal College of Surgeons. (London: Simpkin Marshall and Co., 1883.)

THE author desires his book to be taken seriously. He shows that good looks and manners have a commercial value, since those are more likely to succeed in obtaining the prizes of life who can make favourable first impre sions than those who cannot. The first start greatly depends on patronage, and obscure youths who have won wealth and position have almost always been helped by their good looks, good address, and good voice. These are aids of considerable importance to every candidate, whether it be for a place behind a counter or for the suffrages of a constituency. The author considers from a medical point of view how ill-favoured individuals may palliate their defects. He treats ugliness as a disease, classifying its various forms and indicating such remedies as he can. His classes are coarseness, thinness, obesity, vulgarity, wrinkles, defects of circulation, of complexion, and of the hair. Then he takes the features in detail, eyes, nose, mouth, &c. His recipes are not numerous. We learn incidentally that what is sold as lime juice and glycerine for the hair contains no glycerine at all, and that a very popular dressing is castor oil and rum. This would have harmonised with the toilette of the Syrian beauty of old times, whose "garments smelt of myrrh, aloes, and cassia," a very apothecary-like fragrance. The book does not contain practical advice of much novelty, but its interest chiefly lies in directing attention to much that we already know but are too apt to forget; such as that dissipation, gross feeding, and indolent ways create ugliness of various forms. We know there are bad schools where the boys are slouching, ill complexioned, furtive in expression, and generally ugly, and we also know that there are good schools where, owing to healthy habits and keen and varied interests, the boys are bright, vivacious. and attractive. Similar differences due to different habits of life exist in men; they are pre-eminently shown by the good effect of drill on a plough-boy or street lounger. We may be sure that those who habitually cultivate a healthy mind in a healthy body, and who study how to please, cannot fail to add to the total happiness of the world and to secure for themselves a better chance of succeeding in it than their more negligent rivals.

The Nat Basket. (Printed for the Editress and Publisher, Mrs. Eleanor Mason, at the Albion Press, Rangoon, Burmah.)

WE hope that the subscribers to this extraordinary publication are content to give to it their money and nothing more. It is designed, we are told, to show the natives that there is no contradiction between Scripture and science, but if they believe that what is presented to them in the *Basket* is science they are much to be pitied. Such a medley of misstatements, absurd etymologies, and false astronomy was never before met with out of Bedlam. If this is the stuff that is taught the Burmese by our missionaries, the sooner the latter return home the better.

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts, No notice is taken of anonymous communications.*

[*The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to insure the appearance even of communications containing interesting and novel facts.*]

Deductive Biology

IN the few remarks which I communicated to this journal (vol. xxvii. p. 554) under the above heading, I protested against the deductive method used in a purely literary manner as a mischievous way of attacking biological problems. Mr. William White objects that if I am right the deductive method must be excluded altogether "as a false and dangerous element of philosophy." I do not myself see that this necessarily follows. The pith of what I said simply amounts to this—the biological sciences not having reached the deductive stage, it is not possible to enlarge our knowledge in them by mere ratiocination. This is I apprehend no more than is laid down by Mr. Mill himself. Writing of the conditions under which the deductive method is applicable, he expressly says that without one indispensable adjunct "all the results it can give have little other value than that of guesswork" ("System of Logic," 4th ed. vol. i. p. 498). The indispensable adjunct is verification, which requires the substitution of the work-table for the desk. When the former has put the stamp of confirmation on the speculations elaborated at the latter we get a scientific result which commands attention. Without this confirmation I am still of opinion that the deductive result is only "a literary performance." It is worth noting that the able writer whose papers and method I took the liberty of criticising so far admitted the validity of what I said, that he promised to have some experiments made which would go a considerable way towards demolishing or sustaining the results at which he had so far arrived only deductively.

As it would be a rather arduous undertaking to follow Mr. White over all the other ground covered by his letter, I will only refer to one point. He asks whether "comparative embryology" is not "founded entirely upon the method of deductive analogy." I am certainly myself under the impression that it would be difficult to pitch upon any area in science in which the knowledge we possess has been more conspicuously gained by persistent investigation or one in which generalisations have more often crumbled under the pressure of fresh results of observation. It is the section-cutter, and not the desk, which has won the victories in this field. At the present moment two of the most skilled of our younger embryologists (with funds furnished by the Royal Society) are on the point of starting, one for the Cape, to study the embryology of *Peripatus*, the other to make a similar attempt in Australia on the earliest phases of the life-history of *Ornithorynchus* and *Ceratodus*. They would hardly perhaps engage in so laborious a quest if it would answer equally well to stay at home with a ream of paper, and, say—without any disrespect to the eminent author—a copy of the writings of Mr. Herbert Spencer as "a base of fundamental truth" to start from in the analogical deduction of the embryology of these organisms.

W. T. THISELTON DYER

YOUR correspondent, Mr. William White, has not, it seems to me, a correct appreciation of the words "deductive" and

"induction," as used in reference to the investigation of the causes of phenomena. The mistake which he makes is a very frequent one, and is due to the ambiguity of the words themselves, and to the inaccessibility of a treatise on modern logic.

The "deductive method," as formulated by John Mill, is one method by which the mental process known as induction—"the operation of discovering and proving general propositions"—is accomplished. An "induction" may be a simple inference from an observation; it must be an inference in which the conclusion is *wider* or *more general* than the premises from which it is drawn. A "deduction" (as the term is commonly used) is a result of ratiocination solely, or, in other words, of a "train of reasoning," by which from a general proposition (not alone, but by combining it with other propositions), we infer a proposition of the *same* degree of generality with itself, or a *less* general proposition. The "deductive method" receives its name from the fact that ratiocination is combined in it with induction.

"In order to di cover the cause of any phenomenon by the deductive method, the process must consist of three parts:—(1) Induction; (2) Ratiocination; (3) Verification;" or in common language: (1) A generalisation from observed facts [or a deduction from a previous generalisation]; (2) A deduction from this generalisation [or from an initial deduction]; (3) The testing or verification of the final deduction.

The "hypothetical method" is a special and very usual form of the deductive method in which in place of an induction or primary deduction we have substitnted a hypothesis. Under proper safeguards this is the most valuable and fertile method of investigating the causes of complex phenomena. Hypotheses are legitimate or illegitimate. The cause suggested by the hypothesis, if not already known as existing, ought to be capable of being known, and, until the cause suggested is shown to exist, the hypothesis, although verified, constitutes only a plausible conjecture. Further, the hypothesis must be such that no other hypothesis substituted for it would lead to verification.

A hypothesis, as distinguished from a proposition resulting from a complete induction or a correctly formulated deduction, is "a supposition without actual evidence or with evidence avowedly insufficient." The whole value of a hypothesis lies in the final carrying through with it of the deductive method. It must be made the starting-point of deductions, and these must be (one or more) brought to the test of observation or experiment—the final process of verification.

So much by way of preliminary.

The objection which my friend Mr. Thiselton Dyer has made to the essay of Mr. Grant Allen upon the forms of leaves does not, it appears to me, consist in a depreciation of the "deductive method" as Mr. William White is led to believe. Nothing can be further from the real state of the case.

What Mr. Dyer objects to is that the method is *not carried out* by Mr. Allen. Mr. Allen gives us hypotheses—suppositions with insufficient evidence—and deductions from the generalisation of evolution, but he is relatively deficient in "verification." He also fails in the condition insisted on by Mill, who holds that the hypothetical method is valueless (or relatively so) unless it be proved that no other hypothesis than that formulated can be similarly verified. He further, in the case of the supposed exhaustion of the carbonic acid in atmospheric air, appears to fail in another respect indicated by Mill, in so far as he does not demonstrate the actual existence of the cause which he assumes in his hypothesis. His proposition on this head is no more than "a plausible conjecture" at the best, and is not a legitimate conclusion arrived at by the deductive method.

I do not think that there is any ground for discountenancing either a "purely deductive" or a "purely inductive" method in the treatment of biological topics, so long as the method is soundly and thoroughly carried out and its logical results truly and clearly stated. Still less is there any shadow of reason for not fully accepting *the* "deductive method" (so named) as the method of biological research. What we have to deprecate in some modern speculative essays is the tendency to put forward suppositions as though they were propositions which have been demonstrated, and to employ the printing press in launching hypotheses which are neither legitimate inductions nor deductions, and should be kept unpublished until their originator has thoroughly examined them by the accepted "deductive method."

E. RAY LANKESTER

11, Wellington Mansions, North Bank, N.W.

The Peak of Teneriffe *not* very Active again

WITH reference to my notice in NATURE, vol. xxvii. p. 315, stating, on the authority of a native lady in Santa Cruz, that the Peak of Teneriffe was active again, even to the extent of exuding a red-hot lava stream from near its summit, I am informed now from a higher scientific authority, viz. a Cambridge man, and high Wrangler there in his day, but since then resident in Teneriffe, near Puerto Oratava, for fifty years, that that view was exaggerated. I hasten, therefore, to present to your readers exactly what this venerable and experienced man has to say, without altering a word, so far as the extract goes:—

"The facts of the case," says he, "are simply these. On a clear day of south weather, about the latter end of December or the beginning of January last, I happened to be looking at the Peak (as I often do) and observed several distinct and very copious gushes of steam issuing from the summit. In similar weather I had often seen a similar phenomenon, but never to anything like the same extent. I watched these steam gushes several times that day, and very remarkable they were. On going down to Port the following day, I found they had been seen by several people there, who declared that the peak was pouring forth volumes of smoke and flames. The so-called smoke was simply the steam gushes I have mentioned, and what were mistaken for flames I am convinced were nothing but the same steam gushes illumined by the rays of the setting sun. All agreed that after dark nothing was to be seen there, which certainly would not have been the case had there been fire or flames. As for the lava stream, *that* was a pure fiction of an excited brain. I have looked carefully at the Peak through my telescope, and see nothing but the old, black lava streams that I have known for the last fifty years, and I have spoken with one of the guides who has been lately with a party to the summit, and he declares he saw no trace of any eruption, or of anything different from what he has always seen there."

Then follow some other topics to the end of the letter proper; but to that there is appended the following P.S., which may be interesting to intending travellers this summer:—

"Last night (May 27), about an hour and a quarter after midnight, we had a smart shock of an earthquake which woke me out of a sound sleep and rather frightened us all. However, no damage was done, but here people say that eruptions of the volcano are always preceded by earthquakes; so who knows but that our eccentric friend's vision of the three bonfires and the lava stream may come to be verified after all. If the Peak has any intention of erupting again, *I* should be personally obliged to it if it would do so while I am still in the body. It would be a grand sight from our Sitio."

To any of the previously mentioned intending visitors to the island I would beg to recommend that they carry Dr. Marcet's recent neat little book on "Southern and Swiss Health Resorts." His descriptions of Teneriffe, and especially of Guajara on the great crater, and Alta Vista on the high peak, are graphic, and true though terse. Indeed the only point of difference I have with him is his reason for there not being forthwith erected a grand hotel on the elevated Canadas, high above the summer cloud level, in the driest air, strongest sunshine, and most curative conditions for the moist kinds of consumptive disease, which the whole of this planet world has to offer. The reason he gives is, that there is nothing to interest the invalids, or ordinary lady and gentleman travellers up there.

Yet there have long been mineralogy, geology, a peculiar, though scanty, botany, meteorology of a most commanding type, and astronomy under special advantages, inviting all the readers of NATURE to go there and participate in the mental feast; while now the probabilities each morning of witnessing from a distance a little real eruption, will add an exciting topic to the breakfast conversation and the noonday ramble.

C. PIAZZI SMYTH,
Astronomer-Royal for Scotland

15, Royal Terrace, Edinburgh, June 19

"Devil on Two Sticks"

WHY a game at once so graceful and attractive should have received such a christening I do not know, and I am equally at a loss to imagine how an outdoor sport like this, requiring skill and promoting a healthful exercise of the muscles, should have passed out of sight and become almost forgotten. Like Clerk Maxwell, I have played the game many a time some twenty years since, and hasten without further preliminary to describe it.

Imagine two cones joined together at their small ends like an hourglass, and that a solid of such a shape and size is turned out of walnut or cherry or boxwood or of ebony or ivory: this is the devil. Now take the last half-yard from the taper ends of two billiard-sticks, and let them be connected from these ends by a limp silken cord or string half a yard long: these are the sticks.

To play the game hold the thick ends of the sticks one in each hand, bring the cord under the narrow neck of the solid and try to elevate it in the air: it drops directly on the grass. If a brisk rotary motion is given to it however, it will not only remain on the cord, but several dexterous manœuvres may be accomplished with it: the variety of which and the skill displayed in performing them constitute the game. To produce the revolutions the sticks are moved rapidly up and down alternately, and when the spinning is once established, "Diabolus" may be allowed to run up and down the stick, or he may be projected high up in the air, and, still spinning all the while, be caught on the cord again and again in rapid succession. Two may be engaged in the same game. It was fashionable many years since in these parts, and I recollect seeing a picture in a Tunbridge-ware shop in this place of the lords and ladies engaged in playing this game on the Pantiles at Tunbridge Wells more than half a century ago. JOHN GORHAM

Bordyke Lodge, Tunbridge, June 4

[This is a nearly complete answer to our correspondent's query. The behaviour of the "devil" is an excellent example of that property of the axis of greatest or least moment of inertia of a body which is utilised in a well-thrown quoit or an elongated rifle-bullet. The mode of producing the rotation is easy to learn by trial, but not very easy to describe. The sticks are kept moving so that one end of the cord is always at a greater tension than the other.—ED.]

Channel Ballooning

As I have shown in my pamphlet, "Les Grands Ascensions Maritimes," the British Channel is the proper field for trying maritime ascents and determining which are the best means for rendering balloons serviceable on sea as well as on land. I have had several conversations with Lhoste, my aëronautical pupil, on the circumstances of his audacious trip. I will confine myself to the scientific teaching of this expedition.

The cone anchor was lowered by Lhoste when he saw that the *Noëmi* was running after the balloon, and diminished so much the velocity of the run that it was possible to catch him with a rowing boat. But although the wind was rather mild, the balloon inclined so much that the car was plunged into the water, and the waves drenched the occupant. It is evident that in stormy weather the lowering of the cone anchor would lead to the destruction of the balloon by pressure of the wind. To avoid this it is necessary to reserve the cone anchor for ordinary winds, and to use in other circumstances guide-ropes with wooden nuts passed through them in order to increase friction as much as possible.

The balloon went to the great altitude of 15,000 feet through the activity of the sun, which can be resisted very easily by taking some litres of water out of the sea, when nearing it, with a very simple apparatus that Lhoste has invented.

Any balloon attempting to cross the Channel should be bound to take on board half a dozen carrier pigeons to indicate the place where it has anchored, or has been rescued or landed, so that help could be sent to it without any delay. With such easy precautions, and the throwing out of a sufficient number of pilot balloons, the experiments can be conducted on scientific principles, and exert the most useful influence on the study of aërial currents on these seas, where several such currents combine, and where the constitution of the air is so peculiar that mirages of every description have been frequently seen even at the present season.

Lhoste, on the morning of June 9, saw at an altitude of 1200 feet a regular halo, which surrounded the sun. The fog was so heavy that he could not see the sea, except when it was almost ready to send him to a watery grave. The steam whistling from vessels in different seaports, reached him at every altitude; but he did not know what was the cause of the extraordinary noise.

If it had not been for the fog, Lhoste would have succeeded in a scientific sport which will become fashionable, and ultimately lead astronomy to try the air, and to trust to the winds in spite of Shakespeare.

Lhoste's audacity seems to have given rise to a competition in the Mediterranean from Marseilles, but I believe that the Mediterranean must only be tried when the British Channel has been traversed without difficulty in every direction, and that this part of the ocean will play an important part in the development of aërial navigation in the second century of its existence.

Boulogne, June 17 W. DE FONVIELLE

Geology of Cephalonia

CAN any of your readers kindly inform me whether geological investigations were ever made in the island of Cephalonia, one of the Ionian Islands that were under British protection up to the year 1863?

The following are the names of the fossils that I have been able to determine out of those that were given to me and brought last year from Climatzias, Thermanti, and Leacas, localities in the neighbourhood of Mount Cephalos, in the island of Cephalonia:—

Mitra fusiformis, Brocc.	*Cerithium* ——
M. Bronni, Mich.	*Turritella turris*, Bast.
M. Michelotii, Hörnes.	*Turritella* ——
Buccinum costulatum, Brocc.	*Turritella subangulata*, Brocc.
B. prismaticum, Brocc.	*Vermetus intortus*, Lam.
Chenopus pes pelecani, Phil.	*Natica millepunctata*, Lam.
Triton Turbellianum, Grat.	*Natica* ——
T. corrugatum, Lam.	*Dentalium* ——
Murex spinicosta, Bronn.	*Venus multilamella*, Lam.
Murex ——	*V. plicata*, Gmel.
Murex ——	*Chama gryphoides*, Linn.
Fusus mitræformis, Brocc.	*Cardita Jouanneti*, Bast.
F. virgineus, Grat.	*Pectunculus pilosus*, Linn.
F. longirostris, Brocc.	*Limopsis calabra*, Seguenza.
Turbinella subreticulata, d'Orb.	

Bucuresti (Roumania), June 2 J. P. LICHERDOPOL

Lightning Phenomenon

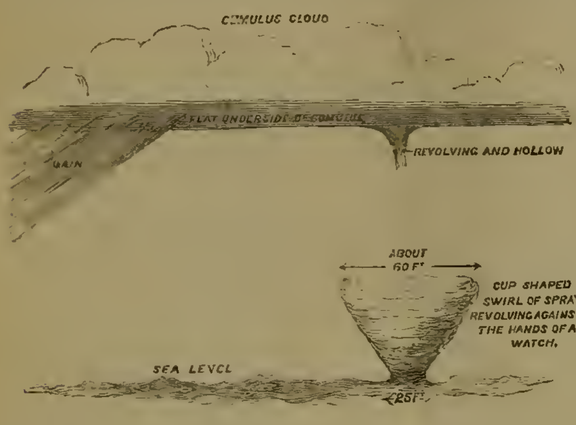
WHILE watching the incessant play of vivid lightning during the progress of a thunderstorm which was raging close by in the country towards Novara, Arona being just on the northern limit, my wife observed the following curious spectacle, the account of which she wrote down immediately afterwards:—At 9.35 p.m. on Sunday, June 3, a meteor-like object was seen to pass apparently from south to north (window facing due east), coming from the side of the storm and disappearing behind a mass of cloud which capped the high hill of Monte Val Grande above Lago Varese. It was oblately spheroid in form and apparently about the size of a fire-balloon, and with the velocity of a rocket was travelling slowly, for it left no visible track. It was of a bright, clear, whitish yellow, with a bright, pale green colour showing on the northern side when it passed behind the dark cloud. It was about three times as high above the horizon as the low hills opposite Arona, and traversed an angle of 45° horizontally from the point where first seen to its disappearance. The next day (June 4) when visiting friends at the Villa Frauzosine, near Tutra, we ascertained that this meteor-like body had also been seen by two or three persons who were sitting on a terrace watching the brilliant lightning to the south; they observed it moving also from south to north, disappearing behind the mountains to the northward. W. H. GODWIN-AUSTEN

Waterspout

ON April 28 last the Cunard steamship *Servia*, in making her outward voyage to New York, fell in with several small waterspouts. Being on deck at the time I made a rough sketch and some notes of the occurrence, which I now venture to send you, having learned that many officers of these steamers have sailed the North Atlantic for years without having witnessed any similar phenomenon there. The ship's position may be easily deduced from the fact that her latitude was 42° 24′ and longitude 51° 3′ at noon on the 27th, while these were 41° 42′ and 59° 53′ respectively at noon on the 28th, at 8.30 a.m. of which day we met the waterspouts. There was hardly any wind at the time, and

the sky, which had been generally overcast, was rapidly breaking up into masses of cumulus clouds separated by wide spaces of blue. About a dozen waterspouts were seen in all, the ship passing right through one of them and thus enabling me to estimate its diameter by direct comparison with the known beam of the *Servia*.

The swirls of spray rose from the sea in a cup-like shape, and revolving rapidly in a direction opposite to that of the hands of a watch. It was only after such a swirl had become well defined that the lower surface of the cumulus cloud above it began to descend as if to meet it, spinning at the same time. Indeed, so inconspicuous was this feature of the phenomenon that many of the passengers, intent on watching the spray-cups sparkling brightly in the sunshine, failed to notice it at all. In no case did the cloud swirl nearly meet the sea swirl, nor did the double-



funneled stem of whirling mist, so generally shown in books, appear. Some spray fell heavily on deck from the swirl through which the *Servia* passed, but the wind, which struck us at one moment on this, at the other on that, side of the face was not brisk enough to carry off any one's hat. The sight was remarkably beautiful whether closely or distantly viewed. In the one case the spray-cup seemed made of rustling jewels which sparkled in the bright sunshine; in the other, the sea horizon appeared as if here and there set with boiling and steaming caldrons, whose rope-like handles hung from the dark undersides of white billowy clouds. D. PIDGEON

Hartford, Mass., U.S.A., May 22

Meteors of June 3

THE large meteor seen by Mr. Hall and others (NATURE, vol. xxviii. pp. 126, 150) was also observed here by Mr. Paul Mathews and myself. We estimated the length of its path while visible as 120° with the middle part due east, the direction of its motion as parallel to the horizon, elevation as 20°, and length of tail as 25°; its apparent brilliance I put at six times, Mr. Mathews at twice, the greatest brilliance of Venus, and the pieces into which it broke up (about six in number) as equal to the brightest planets. The time I should have put at 10.50, but did not note it (Mr. Mathews 10.40 to 10.45). The colour was golden. This was moreover in a very clear and brilliant sky, as about 10 we had observed that the light in the east was so intense that it cast quite a dark shadow as we passed through a somewhat shady part of the road.

Ripon W. W. TAYLOR

IN the correspondence on the large meteor seen on June 3 I have not seen any notice of another curious meteor seen later on the same night. A flash of light in the sky drew attention to it, and when first seen it was moving in nearly a straight line from 102 Herculis to α Aquilæ. In five seconds it travelled slightly more than half the distance to the latter star, and then disappeared without any outburst. It was about a lunar diameter in length, and between 3' and 4' wide at the widest part, a point distant one-third of its entire length from the head. In fact it was not at all unlike a comet with a bushy tail tapering off to a point. The colour was a pale yellow. P. F. D.

London, W.

Intelligence in Animals

SOME years since, when calling on the late Hon. Marmaduke Maxwell of Terregles, our conversation happened to turn on the subject of intelligence and instinct of animals. Mr. Maxwell said if I would walk down to the stables with him he would show me a curious instance. On reaching the stable he pointed out an empty stall in which five well grown young rats were running about—a board had been fixed at the end of the stall to prevent the rats getting out. Some time before the cat had a litter of five kittens, *three* were taken from her and drowned; the following morning it was found she had brought in *three* young rats, which she suckled with the two kittens that had been left; a few days afterwards the *two* kittens were destroyed, and the next morning it was found the cat had brought in *two* more young rats. While we were looking at this strange foster family the cat came into the stable, jumped over the board and lay down, when the rats at once ran under her and commenced sucking. What makes the matter the more singular is, the coachman told me the cat was a particularly good ratter, and was kept in the stable for the purpose of keeping down rats.

Cargen, Dumfries P. DUDGEON

AMERICAN ETHNOLOGY[1]

UNDER the able management of Major J. W. Powell the Bureau of Ethnology, recently attached to the Smithsonian Institution, has already done much useful work in the wide field of American anthropology. This first annual report, however, of its proceedings for the year ending July, 1880, appears to be somewhat behind time for, although bearing on the title-page the date of 1881, it was not issued to the public till the beginning of the present year. But the delay is doubtless due to the large amount of preliminary work required to be got through in organising the department, and future reports may be expected to appear more punctually. The title, "Annual Report," is itself somewhat misleading, the actual report of the director really occupying no more than thirty-three introductory pages, and consisting mainly of a digest of the rich materials filling a large quarto volume of over 600 pages. Hence this is, strictly speaking, a first volume of the *Proceedings* or *Transactions* of the Bureau, and as such gives fair promise of a long and useful career in an anthropological domain which may be regarded as practically unlimited.

From the director's introductory remarks we gather that, after the fusion in 1879 of the various geological and geographical surveys in the general "United States Geological Survey," the Bureau of Ethnology was created and attached to the Smithsonian Institution for the purpose of continuing the anthropological work which had hitherto been prosecuted in a somewhat desultory way by those Surveys. The management of this newly-organised department was intrusted to Major Powell, who, as former Director of the Survey of the Rocky Mountain Region, had already shown special aptitude for ethnological investigation. The direct object of the Bureau, we are told, is to systematise anthropological research in America, and this it is proposed to effect both by the prosecution of research through the direct employment of students and specialists, and by the general encouragement and guidance of original observers co-operating throughout the continent. "It has been the effort of the Bureau to prosecute work in the various branches of North American anthropology on a systematic plan, so that every important field should be cultivated, limited only by the amount appropriated by Congress" (xiv.).

How closely this wide programme has been so far adhered to is evident from the varied contents of this

[1] "First Annual Report of the Bureau of Ethnology, Smithsonian Institution, 1879-80." By J. W. Powell, Director. (Washington Government Printing Office, 1881).

sumptuous volume, which comprises sundry contributions by the director on the "Mythology of the North American Indians," on the "Evolution of Language," on "Wyandot Government," and on "Limitations to the Use of some Anthropological Data"; a valuable and profusely illustrated treatise on the "Mortuary Customs of the North American Indians," by H. C. Yarrow; a preliminary attempt to decipher "Central American Picture Writing," by E. S. Holden; a paper by C. C. Royce on "Cessions of Land by Indian Tribes to the United States;" Col. Garrick Mallery's important treatise on "Sign Language among North American Indians," which has already appeared as a "Separat-Abdruck"; a "Catalogue of Linguistic MSS. in the Library of the Bureau of Ethnology," by J. C. Pilling; lastly, "Illustrations of the Method of Recording Indian Languages from the MSS. of Major J. O. Dorsey, A. S. Gatschet, and S. R. Riggs." Should the department continue to be administered on these broad lines and in this enlightened spirit, a school of anthropology must soon be developed in America, with which, without liberal State subvention, our European societies will find it difficult to keep pace. But with our petty rivalries, our heavy public burdens and constantly increasing armaments, the prospect of such State subvention seems at present at least somewhat remote.

The papers contributed by the director to this volume touch briefly on several important topics of a general character, and often express views regarding the origin and evolution of speech, mythologies, religious and tribal institutions, which will scarcely go unchallenged in some quarters. That these psychological phenomena have hitherto been studied from a somewhat too subjective standpoint, and that many metaphysical subtleties have consequently been grafted on the theogonies and early philosophies of savage man may readily be admitted. In a paper on the mythology of the Indian Aryans recently read before the English Folk-Lore Society, Mr. Andrew Lang dwelt on the necessity of distinguishing between the old and comparatively modern hymns in the Vedas. He pointed out that the Vedas themselves do not embody the most primitive theories on the origin of man and the universe, that they contain ideas at once very old and very new, very mythological and very philosophical, and he adduced several instances of crude and childish savage myths overlapping the more profound and advanced concepts of the Aryan Hindus. In the same way Major Powell argues that philosophy passes in its upward evolution through two stages—the mythologic, in which all outward phenomena are interpreted by analogy with subjective experience, and the scientific, in which they are treated as orderly successions of events. The mythologic necessarily precedes the scientific stage, for "without mythology there could be no science, as without childhood there could be no ultimate forms." It follows that the views of primitive men are simple, childish, and incoherent, and that it is illogical to credit his theogonies, as is often done, with profound and abstruse concepts of the universe. Here, as in all other evolutions, the progress is from the simple and homogeneous to the complex and heterogeneous; the "unknown known" of savage philosophy antedates the "known unknown" of later science. In the primitive stage all things are known, that is, supposed to be known; later on some few things are really discovered, and these when properly understood throw doubt on all the rest. The era of the known unknown is thus reached; to crude and offhand explanations succeeds the critical period of investigation and discovery; science is born; civilisation begins. This upward growth is illustrated by many examples, such as that of the rainbow—which for the Shoshoni (Snake Indian) is a beautiful serpent abrading the icy firmament to give us snow and rain; which in the Norse myth is the bridge Bifrost stretching from earth to heaven; which in the *Iliad* becomes the Goddess Iris, Messenger of Olympus; in Genesis a witness to the Covenant; in science an analysis of white light into its constituent colours.

North America, it is aptly remarked, presents a magnificent field for the study of savage and barbaric philosophies from this fresh standpoint. Formerly attention was paid almost exclusively to the more advanced peoples, Aryans, Semites, Hamites, Chinese. Now it is felt that the complex mythologic, religious, linguistic systems of these peoples are the outcome of earlier and simpler phases of thought, consequently that the study of barbarous and savage communities can no longer be neglected. But in North America alone we have our seventy-five ethnical groups speaking seventy-five stock languages and more than five hundred well-marked dialects, each linguistic stock with a philosophy of its own, or rather as many philosophic systems as it has distinct languages and dialects.

To account for this astounding diversity of speech, Major Powell holds with one or two distinguished European philologists that the fundamental languages must have been evolved in independent centres, that in fact "mankind was widely scattered over the earth anterior to the development of articulate speech, and that the languages of which we are cognisant sprang from innumerable centres as each little tribe developed its own language" (p. 28). He fails to see that this view, in itself to the last degree improbable, is wholly unnecessary and even inadequate to explain the actual conditions. It is unnecessary because the present diversity of speech may be sufficiently accounted for by its vast antiquity and extremely evanescent character. Time, acting in combination with the phonetic growth and decay inherent in all speech, must inevitably effect an indefinite amount of specific change, even supposing that all languages started from a single centre. No evolutionist can deny this, for he admits that time combined with a tendency to modification in altered environments, has brought about an indefinite amount of specific and generic change in the biological world. But animals and vegetables are certainly more persistent, *cæteris paribus*, than linguistic types. *Ergo*. The theory is moreover inadequate to explain the actual conditions in America alone. Here we have doubtless a vast number of specifically distinct languages; but the mechanism of all is very much alike; all are cast, as it were, in the same mould; all belong to the polysynthetic or at least to the agglutinating order. But if speech had in America been evolved in many different centres, it may be asked how this striking uniformity is to be explained? Why have we not here, as elsewhere, representatives of the isolating[1] and inflecting, as well as of the polysynthetic order of speech? Does not their common structure point at a common centre of dispersion, while their specific diversity within this common groove is amply explained by time and evanescence?

But if Major Powell does not always reason conclusively, he is a good observer, and describes in vivid language the scenes of savage life of which he has been a spectator, as witness the subjoined account of oral narrative in the Indian community:—

"On winter nights the Indians gather about the campfire, and then the doings of the gods are recounted in many a mythic tale. I have heard the venerable and impassioned orator on the camp-meeting stand rehearse the story of the crucifixion, and have seen the thousands gathered there weep in contemplation of the story of divine suffering, and heard their shouts roll down the forest aisles as they gave vent to their joy at the contemplation of redemption. But the scene was not a whit

[1] The Othomi of the Anahuac tableland has been cited as an instance of an isolating language in America. But M. de Charancey rightly regards Othomi rather as "une langue primitivement incorporante [polysynthetic], qui, parvenue au dernier degré d'usure et de délabrement, a fini par prendre les allures d'un dialecte à juxtaposition [isolation] ("Mélanges de Philologie," &c., p. 80, Paris, 1883).

more dramatic than another I have witnessed in an evergreen forest of the Rocky Mountain region, where a tribe was gathered under the great pines, and the temple of light from the blazing fire was walled by the darkness of midnight, and in the midst of the temple stood the wise old man, telling in simple, savage language the story of *Ta-wdts*, when he conquered the sun and established the seasons and the days. In that pre-Columbian time, before the advent of white men, all the Indian tribes of North America gathered on winter nights by the shores of the seas, where the tides beat in solemn rhythm, by the shores of the great lakes, where the waves dashed against frozen beaches, and by the banks of the rivers flowing ever in solemn mystery—each in its own temple of illumined space—and listened to the story of its own supreme gods, the ancients of time" (p. 40).

A detailed notice of the other more important papers in this volume must be reserved for a future occasion.

A. H. KEANE

THE FISHERIES EXHIBITION

WE are gratified to see the very thorough way in which the management of the Fisheries Exhibition are endeavouring to carry out their plans. It is evident that the scientific aspects of the wide and important subject will have a fair amount of attention; and we are glad to think that in this direction advice has been sought in the right quarter. In the Exhibition itself those interested in the science of the subject will find much to attract them. Last week (p. 156 we gave a list of subjects which have been settled for conferences, and among those who have consented to read papers, we find such names as Professor Huxley on Fish Diseases, Professor Ray Lankester on the Scientific Results of the Exhibition, Professor Brown Goode on the Fisheries of the United States, Professor Hubrecht on Oyster Culture and Fisheries, Sir Henry Thompson on Fish as Food, Dr. F. Day on the Food of Fishes, Mr. R. H. Scott on Storm Warnings. Further, we are glad to see that a series of handbooks has been arranged for on subjects cognate to the Exhibition. Among them are a few by men of scientific standing, and likely to be of real scientific importance; we hope it may not yet be too late to secure the preparation of a few more handbooks or reports of a similar character. Among the handbooks arranged for, six will be published this month, and the remainder in July. Those of special interest to science are, "The Life History of Fishes," by Prof. H. N. Moseley; "Fish Culture" and "Indian Fish and Fishing," by Dr. Francis Day; "Food Fishes," by Mr. G. B. Howes; "Marine and Freshwater Fishes of the British Isles," by Mr. Saville Kent; "Curious Sea Creatures," by Mr. Henry Lee.

The conferences were introduced on Monday by an interesting lecture by Prof. Huxley, a report of which we give below, and this was followed on Tuesday by a carefully prepared paper by the Duke of Edinburgh, on British Fisheries and Fishermen, read by the Prince of Wales. The real interest which the leading members of the Royal family take in the Exhibition has no doubt done much to contribute to its success. It was to be expected that the German Ambassador would show his appreciation of the importance of science to an industry of such magnitude as that of fishing, and he aptly pointed out how important was the didactic and scientific work at last commenced.

With the general concurrence of opinion in high quarters as to the value of the scientific aspects of the Exhibition, and of the great services which science may render in bringing about the practical objects which are aimed at, we of course heartily concur. It is admitted on all hands that the haphazard way in which our fisheries have hitherto been carried on has led to the worst results, the extinction almost of some important fishes and mollusks, the bad condition of others, and the dearness of what might be the cheapest and most plentiful of foods. In recent years science has done something to remedy this state of things, and it will be well for our fisheries, and therefore for the welfare of a large portion of our population, if the Fisheries Exhibition leads to still more being done in this direction. So far the Exhibition has been an immense success; half a million of people have already visited it, and thus the educational results are likely to be widespread.

Prof. Huxley, in opening the proceedings, said:—

It is doubtful whether any branch of industry can lay claim to greater antiquity than that of fishery. The origin would seem to be coeval with the earliest efforts of human ingenuity; for the oldest monuments of antiquity show us the fisherman in full possession of the implements of his calling; and even those tribes of savages who have reached neither the pastoral nor the agricultural stages of civilisation are skilled in the fabrication and in the use of the hook, the fish-spear, and the net. Nor is it easy to exaggerate the influence which the industry thus early practised and brought to a considerable degree of perfection has directly and indirectly exerted upon the destinies of mankind, and especially upon those of the nations of Europe. In our quarter of the globe, at any rate, fishery has been the foster-mother of navigation and commerce, the disseminator of the germs of civilisation. Having glanced at the development of the industries connected with fishing, more especially by the Phœnicians, he continued:—These few remarks must suffice to indicate the wide field of interesting research which fisheries offer to the philosophical historian, and I pass on to speak of the fisheries from the point of view of our present practical interests. The supply of food is, in the long run, the chief of these interests. Every nation has its anxiety on this score, but the question presses most heavily on those who, like ourselves, are constantly and rapidly adding to the population of a limited area, and who require more food than that area can possibly supply. Under these circumstances, it is satisfactory to reflect that the sea which shuts us in at the same time opens up to us supplies of food of almost unlimited extent. In reference to the relation which the fisheries bore to the total supply of food of those who had easy access to the sea, he quoted the following paragraph from the Report of the Fisheries Commissioners, 1866:—"The produce of the sea around our coasts bears a far higher proportion to that of the land than is generally imagined. The most frequented fishing-grounds are much more prolific of food than the same extent of the richest land. Once in a year an acre of good land, carefully tilled, produces a ton of corn or two or three hundredweight of meat or cheese. The same area at the bottom of the sea in the best fishing-grounds yields a greater weight of food to the persevering fisherman every week in the year. Five vessels belonging to the same source in a single night's fishing brought in seventeen tons' weight of fish, an amount of wholesome food equal in weight to that of fifty cattle or 300 sheep. The ground which these vessels covered during the night's fishing could not have exceeded an area of fifty acres." My colleagues and I made this statement a good many years ago. I have recently tried to discover what yield may be expected, not from the best natural fishing-grounds, but from piscicultural operations. At Comacchio, close to the embouchure of the Po in the Adriatic, there is a great shallow lagoon which covers some 70,000 acres, and in which pisciculture has been practised in a very ingenious manner for many centuries. The fish cultivated are eels, gray mullet, atherines, and soles; and, according to the figures given by M. Coste, the average yield for the sixteen years from 1798 to 1813 amounted to 5 cwt. per acre—that is to say, double the weight of cheese or meat which could have been obtained from the same area of good pasture land in the same time. Thus the seas around us are not only important sources of food, but they may be made still more important by the artificial development of their resources. But this Exhibition has brought another possibility within the range of practically interesting questions. A short time ago a visitor to the market might have seen fresh trout from New Zealand lying side by side with fresh salmon from Scandinavia and from the lakes and rivers of North America. Steam and refrigerating apparatus combined have made it possible for us to draw upon the whole world for our supplies of fresh fish. In my boyhood "Newcastle" was the furthest source of the

salmon cried about the streets of London, and that was generally pickled. My son, or at any rate my grandson, whenever he goes to buy fish, may be offered his choice between a fresh salmon from Ontario and another from Tasmania. The fishing industry being thus important and thus ancient, it is singular that it can hardly be said to have kept pace with the rapid improvement of almost every other branch of industrial occupation in modern times. If we contrast the progress of fishery with that of agriculture, for example, the comparison is not favourable to fishery. Within the last quarter of a century, or somewhat more, agriculture has been completely revolutionised, partly by scientific investigations into the conditions under which domestic animals and cultivated plants thrive, and partly by the application of mechanical contrivances and of steam as a motive power to agricultural processes. The same causes have produced such changes as have taken place in fishery, but progress has been much slower. It is now somewhat more than twenty years since I was first called upon to interest myself especially in the sea fisheries. And my astonishment was great when I discovered that the practical fisherman, as a rule, knew nothing whatever about fish, except the way to catch them. In answer to questions relating to the habits, the food, and the mode of propagation of fish —points, be it observed, of fundamental importance in any attempt to regulate fishing rationally—I usually met with vague and often absurd guesses in the place of positive knowledge. The Royal Commission, of which I was a member in 1864 and 1865, was issued chiefly on account of the allegation by the line fishermen that the trawlers destroy the spawn of the white fish—cod, haddock, whiting, and the like. But, in point of fact, the spawn which was produced in support of this allegation consisted of all sorts of soft marine organisms except fish. And if the men of practice had then known what the men of science have since discovered, that the eggs of cod, haddock, and plaice float at the top of the sea, they would have spared themselves and their fellow-fishermen, the trawlers, a great deal of unnecessary trouble and irritation. Thanks to the labours of Sars in the Scandinavian seas, of the German Fishery Commission in the Baltic and North Seas, and of the United States Fishery Commission in American waters, we now possess a great deal of accurate information about several of the most important of the food fishes, and the foundations of a scientific knowledge of the fisheries have been laid. But we are still very far behind scientific agriculture, and, as to the application of machinery and of steam to fishery operations, in this country at any rate, a commencement has been made, but hardly more. The relative backwardness of the fishing industry made a great impression on my colleagues and myself in the course of the inquiries of the Royal Commission to which I have referred; and I beg permission to quote some remarks on this subject which are to be found in our Report issued in 1866:—"When we consider the amount of care which has been bestowed on the improvement of agriculture, the national societies which are established for promoting it, and the scientific knowledge and engineering skill which have been enlisted in its aid, it seems strange that the sea fisheries have hitherto attracted so little of the public attention. There are few means of enterprise that present better chances of profit than our sea fisheries, and no object of greater utility could be named than the development of enterprise, skill, and mechanical ingenuity which might be elicited by the periodical exhibitions and publications of an influential society specially devoted to the British fisheries." Taking this Exhibition, the third of its kind, as evidence that the public attention to fisheries for which they hoped had been attained, he remarked that the conference opened that day formed an entirely new feature of such exhibitions, and expressed a hope that there was in them a germ of that which, by due process of evolution, might become a great society, having for its object the welfare and the development of the fisheries of these islands. He presently turned to the question whether fisheries are exhaustible; and, if so, whether anything can be done to prevent their exhaustion. He did not think it possible to give a categorical answer. There were fisheries and fisheries; but he had no doubt that there were some fisheries which were exhaustible. Instancing the salmon rivers, he said it was quite clear that those who would protect the fish must address themselves to man, who was reachable by force of law; and that it not only might be possible, but it was actually practicable, to so regulate the action of man with regard to a salmon river that no such process of extirpation should take place. But if we turned to the great sea fisheries, such as cod and herring fisheries, the case was entirely altered. Those who have watched these fisheries off the Lofoden Isles on the coast of Norway, say that the coming in of the cod in January and February is one of the most wonderful sights in the world; that the cod form what is called a "cod mountain," which may occupy a vertical height of from 20 to 30 fathoms—that is to say, 120 to 130 feet, in the sea; and that these shoals of enormous extent keep on coming in in great numbers from the westward and southward for a period of something like two months. The number of these fish is so prodigious that Prof. Sars, the most admirable authority, from whom I quote these details, tells us that when the fishermen let down their loaded lines, they feel the weight knocking against the bodies of the codfish for a long time before it gets to the bottom. I have made a computation, with the details of which I will not trouble you, which leads to this result, that if you allow the fish each of them four feet in length, and let them be a yard apart, there will be in a square mile of such shoals something like 120 million fish. I believe I am greatly understating the actual number, for I believe the fish lie much closer; but I would beg your attention to the bearing of this underestimate, because I do not know that the Lofoden fishery has ever yielded more than 30 million fish in a good season; and so far as I am aware the whole of the Norwegian fisheries, great as they are, do not yield more than 70 millions. So you will observe that one of these multitudinous shoals would be sufficient to supply all the fisheries of Norway completely, and to leave a large balance behind. And that is not all. These facts about the cod apply also to the herring; for not only Prof. Sars, but all observers who are familiar with the life of the cod when it has attained a considerable size, tell us that the main food of the cod is the herring, so that these 120 million of cod in the square mile have to be fed with herring, and it is easy to see, if you allow them only one herring a day, that the quantity of herring which they will want in the course of a week will be something like 840 million. Now I believe the whole Norwegian herring fishery has never reached the figure of 400 million fish—that is to say, one half the fish which this great shoal of codfish eats in a week would supply the whole of the Norwegian fisheries. On these and other grounds it seemed to him that this class of fisheries—cod, herring, pilchard, mackerel, &c.—might be regarded as inexhaustible. But he should not venture to say this of the whole of the sea fisheries—of the oyster fisheries, for example. Here, again, the operations of man become exceedingly important. Regarding the regulation as to close time for oysters as alone absolutely futile for the purpose of protection, he urged that the more logical provisions of government supervision in Denmark, France, and elsewhere, were impracticable of application beyond the three-mile limit of this country. It was under this conviction that the Commission to which he referred recommended the abolition of all restrictive measures. In conclusion, he pointed out how heavily this question bore on the social condition of the fisherman. Every act of legislation with regard to the fisherman created a new offence. If the common welfare and the common interest, said Prof. Huxley, can be clearly shown to render such regulations desirable or necessary, then of course fishermen must put up with this as they put up with anything else—as we all put up with such restrictions. But supposing that no good case is made out, supposing that regulations of this kind are made on insufficient inquiry and based on insufficient understanding of the circumstances of the case, then I am free to confess that I think those who make such laws deserve very much severer penalties than those who break them.

THE SCIENTIFIC WORK OF THE "VEGA"[1]

THE volume we have before us—the first of a series—contains the results of the scientific observations made during the cruise of the *Vega*, and to say this is obviously to indicate that it contains a rich supply of most valuable information as to that part of the Arctic Ocean which extends along the coasts of Siberia, which appears in the shape of a series of elaborate papers on different departments of natural history of the Arctic regions. Several parts of this volume are already known. Such are the "Reports to Dr. Oscar Dickson" written by Baron Nordenskjöld during the expedition, and read throughout the civilised

[1] "*Vega*-Expeditionens Vetenskapliga Iakttagelser, bearbetade af deltagare i resan och andra forskare, utgifna af A. E. Nordenskjöld." Vol. i. Part 1. (Stockholm, 1882.)

world as soon as they reached Europe; or his paper on the possibility of navigation in the Siberian Arctic Sea; or, again, his paper on auroræ (recently summarised in the pages of NATURE). The well-known "Reports" appear as they were written on board the *Vega*, but with a map "of the northern coast of the old continent, from Norway to Bering Strait," and with several maps of separate islands and bays. The other papers are quite new, and we find in this volume a medical report, by E. Almquist, on the health of the crew; a paper by A. Lindhagen, on the determinations of latitudes and longitudes, which will put an end to the discussions as to the accuracy of the changes made in the map of the northern coast of Siberia by the astronomers of the *Vega*; a paper by H. H. Hildebrandson (in French), on the meteorological work of the expedition, being a comparison of the climate of Pitlekaj (the *Vega's* wintering station) with the climate of other Arctic stations; several papers on the Chukches; an elaborate paper by A. Stuxberg, on the invertebrate fauna of the Siberian Arctic Sea; and a series of papers by F. R. Kjellman, A. N. Lundström, and E. Almquist, on the vegetation of the region visited.

It is known that the expedition of the *Vega* was—as is, however, always the case with so experienced a traveller as Nordenskjöld, and as it was with Parry—one of the most successful Arctic expeditions with regard to the health of the crew. No death was to be regretted, and all the thirty men of the crew reached Naples in the best condition. It is true that at Pitlekai the sun did not disappear for months under the horizon, and that the crew were not worn out by long sledge journeys. But still the climatic conditions were not at all favourable, on account of the variability of the weather and strong winds which blew with a twenty miles' speed even during frosts —30° strong. The precautions taken for maintaining a temperature as equal as possible in the cabins and for eliminating moisture, as well as for much exercise in the fresh air and the maintenance of cheerfulness among the crew, are not to be underrated. A daily distribution of lime-juice and of jam surely had also their importance as preservatives against scurvy, and this the more as the crew of the *Vega* had no opportunities of having supplies of fresh meat during the winter. The daily baking of fresh bread was a very good innovation; as to the preserved meat, the crew very soon had enough of it, and even salted meat was preferred to corned beef; only the finer and more expensive preserved soups and steaks were appreciated throughout the cruise.

The Chukches were of course the subject of anthropological and ethnographical studies, as far as possible. Mr. Nordquist publishes a most valuable Chukch dictionary, and Mr. Almquist communicates interesting observations on colour-blindness among the Chukches, two hundred men and a hundred women having been submitted to experiments in accordance with Prof. Holmgren's hints as to this kind of research with people whose language is unknown ("Om färgblindheten," &c., Upsala, 1877). The supposition of Helmholtz and Young as to the blindness of the lower races with regard to violet rays has not been confirmed as far as the Chukches are concerned; they certainly distinguish it, but they merely call it red. The same is true with regard to pale green and bright blue; they confound both, but the organs for green are not missing with them. Like other lower races, they use much red colour for their skins, yellow being comparatively rarely used for ornaments. It results also from M. Kjellman's paper on the culinary plants of the Chukches, that, contrary to Wrangel's assertion, they do not despise vegetable food. Their provisions of plants for the winter are as large as their provisions of meat and fat. This feature so much distinguishes them from all other inhabitants of the Arctic regions that one is inclined to admit that the time is not far removed from that when they cultivated some better situated plots of soil on the coast of the Arctic Ocean, or were compelled to leave lower latitudes which had a happier climate than that of the north-eastern extremity of Siberia. Their provisions in vegetables are a very strange mixture of various plants, among which the following are the most common:—*Cineraria palustris*, L., *Petasites frigida*, Fr., *Pedicularis sudetica* and *P. lanata*, Willd., *Rhodiola rosea*, L., *Claytonia acutifolia*, Willd., *Halianthus peploides*, Fr., *Polygonum polymorphum*, L., and *Salix boganidensis*, Trautvetter.

The most important papers in this volume are those devoted to the vegetation of the region visited, that is, to the lichens and mosses of the coast, to the algæ of the Siberian Sea, and to the phanerogamic flora of Novaya Zemlya, of the coast region, and of the Asiatic coast of Bering Strait. The lichens are comparatively rare on the coast; whole stretches are quite devoid of them, and the lichen flora is altogether poor as to the number of species. The Calicieæ are represented by but three species, and the whole group of the Sclerolichens only by five or six species, none of them being spread over the whole of the coast. The Stictaceæ are represented by a couple of species of *Nephroma*; the Pannariaceæ by five to six species; *Pyrenopsis* has but one species which is widely spread, but not very common. The character of the flora is nearly the same on the whole of the coast, but towards the south, where the country becomes inhabited, the flora undergoes a notable change. The Phanerogams become also comparatively rich towards the east, in the land of the Chukches, where the grasses appear in the shape of whole sods, without a mixture of moss. The Algæ are few in the Siberian Sea, the whole number of observed species being but twelve, that is, only one-half of the number of species that are known on the Murman coast and in the Sea of Spitzbergen. The characteristic feature of all Arctic Algæ being their large size, the Siberian Algæ seem to be an exception to this rule. The largest of them was a *Laminaria Aghardii*, 210 centimetres long and 37 cm. wide.

The papers by MM. Kjellman and Lundström on the Phanerogams of the explored region will be read with great interest both by the botanist and the geographer. They are not bare enumerations of plants, but elaborate sketches of botanical geography taking into account former botanical work in neighbouring tracts, and describing the flora in its dependence upon local conditions of climate and soil. The coast-flora of Northern Siberia is altogether poor, as it numbers but 150 species of Phanerogams; this number slightly increases, however, towards the east, where it reaches 221, as well as towards the west, 185 species being known from Novaya Zemlya. The Obi—at least as far as the coast region proper is concerned—is not a separation-line between the Arctic European and the Arctic Asiatic floras, as was expected by Hooker. Only the *Salix rotundifolia* and *Wahlbergella affinis* do not appear to the east of the great West Siberian river. Of the 150 species noticed, only one-third are Monocotyledons. This proportion increases, however, at certain places, and there are monocotyledonous species extending over large areas. However poor as to the number of species, the Siberian coast-flora still affords a variety of forms, as it has representatives of 33 different families and 93 different genera. The families which are the most represented in the coast-flora are those of Gramineæ and Cruciferæ, which number respectively 23 and 20 species. They are followed by those of the Compositæ, Saxifragaceæ, Ranunculaceæ, Cyperaceæ, and Caryophyllaceæ (15 to 11 species). The family of Saxifragaceæ is that which maintains the greatest number of species towards the north, eight species out of fourteen having been found even at Cape Chelyuskin; the Caryophyllaceæ nearly keep pace with the former; whilst the family which spreads least towards the north is that of Compositæ

which was represented only by two species at the mouth of the Taimyr and none at Cape Chelyuskin. So also with the Cyperaceæ and Ranunculaceæ. *Saxifraga oppositifolia* is not the most widely-spread species on the Siberian northern coast, as is the case for other parts of the Arctic region, other species of Saxifraga being as much or more extensively spread than it. The most usual phanerogamous plants on the coast seem to be the *Luzula arcuata*, var. *hyperborea*, and *Stellaria longipes*.

We shall not analyse the valuable paper by M. Kjellman on the flora of Novaya Zemlya, which is a summary of all that is known on this subject, and we shall notice but a few facts concerning the vegetation of the Siberian coast of Bering Strait. It is represented on M. Kjellman's lists by 221 species belonging to 41 families and 109 genera. The Compositæ, Cyperaceæ, Saxifragaceæ, Caryophyllaceæ, and Gramineæ, numbering from 20 to 15 species each, are here also the richest as to the number of species. But we find on the Asiatic coast of the Bering Strait a good many plants belonging to the American flora, as also to the flora of the Altai and Baikal regions, which are not met with elsewhere on the northern coast. No less than 53 species out of 221 appear only to the east of the Kolyma, which appears thus to be, for the coast-region, a more important boundary line than the Obi. This notable increase cannot be accounted for only by the milder character of this region, but it could be explained, in our opinion, if we took notice of the orography of Eastern Siberia, which favours, by the extension of its chains of mountains from south-west to north-east, the spread of both animals and plants in the same direction.

Dredging was very diligently carried on during the whole of the cruise of the *Vega* in the Arctic Ocean; and Mr. Stuxberg's map of dredgings made during the Swedish expeditions of 1875, 1876, and 1878 to 1879, is dotted with 33 spots in the Kara Sea, and with 90 spots along the Siberian coast to Bering Strait. The temperature of water obviously was found to be very low; even at a few fathoms below the surface it was from $-0{\cdot}9^{\circ}$ to $-2{\cdot}3^{\circ}$ at a depth of 50 metres, and it had a normal specific gravity of 1·027. The most uniformly spread animals in the Siberian coast-region of the Arctic Ocean, and in the Kara Sea, are undoubtedly the Crustaceans; the Echinoderms are comparatively few, as also are the Mollusca, Bryozoa, and Hydroids. The Crustaceans *Idothea Sabinei*, *Idothea entomon*, *Diastylis Rathkei*, *Atylus carinatus*, and *Acanthostephia Malmgreni*, are the most usual. The first, as known, has been found nearly everywhere in the Arctic Ocean; whilst the second proved to be specific for the whole of the Arctic coast of the old continent, for a stretch of nearly 160 degrees of longitude; it has been found also in the lakes of Sweden and Northern Russia, even in the Caspian and Lake Aral—Lake Baikal being till now the sole explored great lake of this part of the old continent where it has not yet been found. As to the vertical distribution of the animal forms, no distinct regions can be established. It must be observed, however, that the littoral region—about 30 fathoms deep—on account of its ice and sweet water brought by rivers, is nearly quite devoid of animals; even the littoral forms go to take refuge in the sublittoral region. Not only is the Siberian Sea very rich in forms of animals (the number of described Amphipods being as much as 59 out of 114 Amphipods known in all Arctic seas together); it contains also such a number of individuals of certain species, that Mr. Stuxberg describes about 20 real "formations" (*djurformationen*), each consisting of very large quantities of individuals of one given species, with a comparatively small mixture of other species. Such are the *Diastylis Rathkei*, *Reticulipora intricaria*, *Alcyonidium mammillatum*, *Chiridota lævis*, *Echinus dröbachiensis*, *Asterias Lincki*, *Archaster tenuisbinus*, *Ctenodiscus crispatus*, *Ophiacantha bidentata*, *Ophiocten sericeum*, *Ophioglypha nodosa*, *Astrophyton eucnemis*, *Antedon Eschrichti*, *Yoldia arctica*, and *Idothea entomon*, as also Ascidiæ, Actiniæ, and Hydroids. As a whole, the Siberian basin differs very much in its fauna from the other parts of the Arctic basin, and it has no less than 16 species that are characteristic of it. Novaya Zemlya is the limit of the fauna of the Siberian Sea, being a separation-line for many species.

The foregoing notice will give a general idea of the valuable material contained in the first volume of the "*Vega's* Scientific Work," and the manner it is treated. We have but to express the wish to see, as soon as possible, the appearance of the following volumes of this series. They will surely give a new and powerful impulse to the study of Northern Siberia. P. K.

NOTES

WE are glad to learn that Mr. Spottiswoode continues to go on favourably.

STILL another well-deserved honour for Sir Joseph Hooker. The Society of Arts' Albert Medal for "distinguished merit for promoting arts, manufactures, or commerce," has been awarded to him for the present year, for the eminent services which, as a botanist and scientific traveller, and as Director of the National Botanic Department, he has rendered to the arts, manufactures, and commerce by promoting an accurate knowledge of the flora and economic vegetable products of the several colonies and dependencies of the Empire.

AMONG those to whom the Council of the Society of Arts have awarded their silver medals are Mr. Alex. Siemens and Dr. Hopkinson, for their papers on "The Transmission of Power by Electricity," and "The Portrush Railway," and to Capt. Douglas Galton for his paper on "The Economy of Sanitation." Thanks were voted to Mr. W. H. Preece, F.R.S., for his paper on Electrical Exhibitions.

A SPECIAL extra meeting of the Anthropological Institute was held at the Piccadilly Hall on Tuesday, when the Botocudo Indians and a large ethnological collection from Brazil were exhibited by the kindness of Mr. C. Ribeiro, and Prof. A. H. Keane read a paper descriptive of the Botocudos.

MR. MARK H. JUDGE has resigned his position as Secretary and Curator of the Parkes Museum.

M. DE LESSEPS has declared officially at the Academy of Sciences the intention of the Suez Company to open a new canal. During the works the maritime way will be lighted by electricity, and an appeal has been addressed to physicists to present their several systems. The work will begin as soon as possible.

WE have received a favourable report of the National Museum, Bloemfontein, Orange Free State. Considerable collections are being brought together, but the committee should not forget that the chief object of such a museum ought to be to make its collections mainly representative of the interesting country in which it is placed.

DR. KERR of Canton is publishing in Chinese a complete work on the theory and practice of medicine, compiled from European standard works upon that subject. The sections on fevers, and diseases of the stomach, have already been published, while those on affections of the heart and lungs have just been issued. Volumes on the kidneys and nervous system are in the press. The translator has omitted the discussion of all unsettled theories and disputed points. The volumes are printed from wooden blocks, clearly and evenly cut, and are sold at a price which brings them within the reach of all.

Mr. J. W. Taylor of Leeds, who is the editor of the *Journal of Conchology*, has issued a prospectus for a "Monograph of the Land and Freshwater Mollusca of the British Fauna," and he invites the assistance of conchologists towards his propo ed undertaking. According to the prospectus the work will be very comprehensive, and will include the subjects of variation, geographical and local distribution, synonomy and bibliography, "biological aspect and relation to environment." It would be desirable to add distribution in point of time or the palæontological aspect. Mr. Taylor has given specimens of the work in some of the lately published numbers of his *Journal of Conchology*, and they seem to be carefully and almost exhaustively done. We hope the cost of this work will place it within the means of the numerous and comparatively poor conchologists in the north of England, as so many manuals on the subject have already been published at very moderate prices. Great service would likewise be done to natural history by reducing the excessive number of so called species fabricated during the last twenty years by some Continental conchologists. The judicious remark made by Hooker and Thomson in the introduction to their "Flora Indica" ought always to be borne in mind, viz. that "the discovery of a form uniting two others previously thought distinct, is much more important than that of a totally new species, inasmuch as the correction of an error is a greater boon to science than a step in advance."

The *Union Médicale* of June 2 announces a discovery of the highest scientific interest, and which, if it turns out to be real, will show that prehistoric man is no longer a myth. On piercing a new gallery in a coal-mine at Bully-Grenay (Pas-de-Calais), a cavern was broken into containing six fossil human bodies intact—a man, two women, and three children—as well as the remains of arms and utensils in petrified wood and stone, and numerous fragments of mammals and fish. A second subterranean cave contained eleven bodies of large dimensions, several animals, and a great number of various objects, together with precious stones. The walls were decorated with designs of combats between men and animals of gigantic size. A third and still larger chamber appeared to be empty, but could not be entered in consequence of the carbonic acid it contained, which is being removed by ventilators. The fossil bodies have been brought up to the surface, and five of them will be exhibited at the *mairie* of Lens; the others are to be sent to Lille in order to undergo examination by the Faculté des Sciences. Representatives of the Académie des Sciences of Paris and of the British Museum having been telegraphed for, are expected to be present.

The Lords of the Committee of Council on Education have sanctioned the addition of hygiene to the list of sciences in which grants are made by the Department. A syllabus has been prepared, and will shortly be issued to science schools and clas es.

From the third Annual Report of the Hampstead Naturalists' Club we are glad to see that the society is in a prosperous condition, and is gradually getting together a useful museum.

G. P. Putnam's Sons of New York have published a nicely got up and profusely illustrated Guide to the Yellowstone National Park, by Mr. H. J. Winser, which those proceeding to the States for their holiday would do well to get.

Next month Messrs. Williams and Norgate will publish a new work, entitled "The Natural Genesis," in two volumes, by Mr. Gerald Massey, containing the Natural Genesis and Typology of Primitive Customs; Gesture-signs, Ideographs, and Primordial Onomatopœia; Time and Numbers; the Serpent, Dragon, and other Elementaries; the Tree, Cross, and Four Corners; the Great Mother, Twins, Triads, and Trinity; the Mythical Creations; the Fall in Heaven and on Earth; the Deluges and Ark; and Equinoctial Christology.

A writer in a recent number of the *North China Herald*, referring to fossils in China, remarks that the Chinese have never advanced a theory to explain their existence. In their books references are made to fossil shells, crabs, fish, trees, &c., but no attempt is made to account for their occurrence in solid rock. The little that is said is mostly of the marvellous sort. Ammonites are petrified snakes; fossil brachiopods (lamp-shells) are called "stone swallows," and are said to come to life and fly from their hiding places at the approach of wind and rain, changing again to stones on the return of fair weather. Fossil fish appear and disappear at pleasure, and their appearance prognosticates a plentiful harvest and prosperous times. One author supposes that the figures of birds, beasts, and plants, which he had seen on certain slabs, must be the work of gods or devils, for no human hand could chisel anything so minute and delicate.

Crackers play a large part in the superstitious observances of the ordinary Chinese. It is a popular belief that the evil spirits everywhere inhabiting the air are dispersed by crackling noises, attended by fire and smoke. Accordingly crackers are used on all special occasions to frighten away the demons who are tormenting a sick person, or who crowd around the people at the beginning of the New Year. Bamboo, which when burning emits a crackling sound, is also used for the same purpose.

We have received the Report of the West Kent Natural History, Microscopical, and Photographic Society for 1882-83. It appears to be more bulky than its predecessors, extending to 68 pp. 8vo. The President (Dr. F. T. Taylor) discourses on Bacteria and Vivisection; Mr. J. Glaisher, F.R.S., gives a very instructive paper on the extraordinary meteorological conditions between October 1881 and May 1882, illustrated by two diagrams indicating the mean daily barometric and thermometric readings, and their departure from the mean, as observed at Blackheath; Mr. J. Jenner Weir, F.L.S., discusses on the types of variation in Lepidoptera, in which is embodied much useful information; Mr. Stone alludes to certain points in the economy of wasps; Mr. Heisch's notes on "Adulteration" are of practical interest. In their next Report this old-established Society may perhaps think it advisable to give a tabular indication of the "contents"; the same remark would apply equally to the publications of other local societies.

Under the title of "Lantern Readings" Mr. Lant Carpenter has issued a pamphlet to be used (when necessary) with the first series of the biological lantern slides which we referred to in a recent number. These slides are now ready, and may be obtained from York and Son. The pamphlet and slides are intended to illustrate the results of the voyage of the *Challenger*. There are descriptions of forty-two slides in all, and "preliminary hints" show how the pamphlet is to be used.

The additions to the Zoological Society's Gardens during the past week include two Malbrouck Monkeys (*Cercopithecus cynosurus* ♂ ♀) from West Africa, presented respectively by Mr. L. Morris and Mr. A. M. Moore; a Macaque Monkey (*Macacus cynomolgus* ♀) from India, presented by Mrs. E. J. H. Sprague; a Rhesus Monkey (*Macacus erythræus* ♂) from India, presented by Mr. C. T. Pollock; a Bonnet Monkey (*Macacus radiatus* ♀) from India, presented by Mr. F. Nelson; two Mauge's Dasyures (*Dasyurus maugæi*) from Australia, presented by Sir Louis S. Jackson, F.Z.S.; two Earl's Weka Rails (*Ocydromus earlii*) from North Island, New Zealand, a Black-backed Porphyrio (*Porphyrio melanotis*) from Australia, presented by Capt. R. Todd; three Common Kingfishers (*Alcedo ispida*), British, presented by the Hon. and Rev. F. G. Dutton; a Common Night Heron (*Nycticorax griseus*), European, presented by Mr. H. H. Blacklock; a King Penguin (*Aptenodytes pennanti*), two Upland

Geese (*Bernicla magellanica* ♂ ♀), two Ruddy-headed Geese (*Bernicla rubidiceps*) from the Falkland Islands, presented by Mr. R. C. Packe; three Common Pheasants (*Phasianus colchicus* ♂ ♀ ♀), British, presented by Mr. H. T. Bowes; an Indian Python (*Python molurus*) from India, presented by Mr. G. E. Shute; a Sykes's Monkey (*Cercopithecus albigularis*), a Philantomba Antelope (*Cephalophus maxwelli* ♀), an Elate Hornbill (*Buceros elatus*), a Jardine's Parrot (*Pæocephalus gulielmi*) from West Africa, an Indian Civet (*Viverricula indica*), two Wandering Tree Pies (*Dendrocitta vagabunda*), from India, a Red-sided Eclectus (*Eclectus polychlorus*) from New Guinea, five Red-bellied Conures (*Conurus vittatus*), a Giant Toad (*Bufo agua*) from Brazil, a Horned Lizard (*Phrynosoma cornutum*) from Texas, four Cornish Choughs (*Fregilus graculus*), British, purchased; a Common Rhea (*Rhea americana*) from South America, received in exchange; two Indian Pythons (*Python molurus*) from India, received on approval; a Japanese Deer (*Cervus sika* ♂), born in the Gardens.

OUR ASTRONOMICAL COLUMN

The Paris General Catalogue of Stars.—In the last Annual Report issued by Admiral Mouchez we find particulars of the progress of formation of this extensive and important catalogue. It is intended to contain all the stars observed at Paris during the forty-five years 1837 to 1881 inclusive, about 40,000, but it is mainly the result of the revision of Lalande's stars in the *Histoire Céleste*; indeed, for several years past, the meridian instruments have been almost wholly occupied upon this work, and upwards of 27,000 observations were made during 1882, the year to which the Report refers. The entire number of observations upon which the Paris General Catalogue will be founded is about 350,000. The positions are referred to three principal epochs; 1845·0 for the years 1837-53, 1860·0 for the years 1854-67, and 1875·0 for the years 1868-82. A specimen of the form in which it is intended to print the catalogue is appended to the Report. The right ascensions and declinations are given for each principal epoch, with the number and mean year of the observations. The precessions are reckoned from the year 1875, with the term depending upon the square of the time. The magnitudes and the differences from the positions of the *Histoire Céleste* are annexed, and where a star has not been observed by Lalande a synonym in some other catalogue is given. In the first column we have the ordinal number, and in the second the star's number in the reduced catalogue of the *Histoire Céleste*. It is mentioned in the Report that M. Bossert had undertaken a new determination of the places of the stars in that work, making use of the reduction-tables of the late Doctor von Asten, which are more exact than the tables of Hansen and Nissen, employed for the catalogue published in 1847. M. Bossert has already effected the reduction of 2,300 stars, a voluntary labour which has occupied his leisure hours. It would add to the value of the columns showing the differences between the new Paris positions and those of Lalande, if the comparisons could be made with places resulting from the application of von Asten's tables, though it might be necessary to supplement M. Bossert's laudable efforts. In the last Greenwich Catalogue (1872) the precessions are given to four places of decimals in right ascension (time), and to three places in north polar distance; the Paris Catalogue gives these quantities with a figure less, which we are inclined to regard as a retrograde step.

This General Catalogue of the Observatory of Paris is to comprise two parts, which will be published simultaneously; the first part forming the catalogue proper, and the second containing details of the observations upon which the mean positions are founded. Each part will be composed of four volumes; the first volume of each is intended to appear during the year 1884.

Encke's Comet in the Years 1871–1881.—At the sitting of the Paris Academy of Sciences on June 11, M. Tisserand communicated a note by Dr. Backlund, of the Observatory of Pulkowa, relative to the motion of Encke's Comet in the interval 1871–1881. To complete the theory of this comet, it has been necessary to introduce an empirical to the mean motion of the form $\mu'\left(\frac{t}{1200}\right)$. The quantity μ', which was found to be nearly constant during the period 1819–1868, appears to have undergone a considerable variation about the latter epoch. Dr. Backlund bases his calculations upon osculating elements for October 27, 1874, which he considered exact enough for his purpose: they give—

$$\mu = 1079''\cdot33355 + \mu'\tau \left(\text{where } \tau = \frac{t}{1200}\right)$$
$$\mu' = + 0\cdot051731$$

After having carefully reviewed the computation of perturbations by Asten, and calculated by two different methods the perturbations during the revolution 1878–1881, Dr. Backlund compared the elements with the observations made in the years 1871, 1875, 1878, and 1881. By means of this comparison, he obtains corrections to the elements, and, observing that if there exists a tangential force, which varies the dimensions of the comet's orbit, its effect is not only secular, but also periodic, the periodic terms being always very small, except in the expression for the mean anomaly. This he takes into account, and finally deduces for the corrections of the two quantities above—

$$\Delta\mu = + 0''\cdot004745$$
$$\Delta\mu' = - 0\cdot0059867$$

Hence, he says, his investigation proves that the acceleration of the mean motion in the period 1871–1881 was less than half the value found by Encke and Asten for the period 1819–1865. Aten's value is + 0·104418.

CHEMICAL NOTES

Interesting experiments on the luminosity of gases are described by W. Siemens in *Ann. Phys. Chim.* [(ii.) 18, 311], and by E. Wiedemann [*ib.* 509]. Gases free from solid particles do not become luminous at high temperatures, nor is the luminosity of a flame due to incandescence of the products of combustion; if the gases are strongly heated before being burnt, the flame becomes hotter and shorter than it is when the preliminary heating is omitted, and the luminous flame is seen to be distinctly separated from the non-luminous products of combustion. Siemens seems inclined to regard the chemical action which proceeds as the cause of luminosity: if the existence of an envelope of ether around the molecules is assumed, then the reaction of one molecule on another may be regarded as starting vibrations in this envelope, which vibrations give rise to heat and light rays. Wiedemann especially considers the luminosity of gases under the influence of electric discharges: he thinks that in the process of charging the electrodes the ethereal envelopes of some of the gas molecules are distorted; when discharge occurs these envelopes are set into motion, and hence the luminous effects.

Some time ago Ostwald deduced the relative affinities of various acids in terms of nitric acid taken as 100; by relative affinity is meant the proportion in which two acids divide themselves between one base, all the reacting substances being in solution. Ostwald has recently investigated this subject by a method different from that formerly employed; he has studied the rates of action of various acids on acetamide, and from the results he has deduced the relative velocities of action, and hence the relative affinities. The following table contains the results. In column II. are placed figures representing the results of his former experiments—

		I.	II.
Hydrochloric	acid	100	98·0
Nitric	,,	98	100
Hydrobromic	,,	98	95
Trichloracetic	,,	80	80
Dichloracetic	,,	40·8	33
Monochloracetic	,,	13·0	7
Formic	,,	5·2	3·9
Lactic	,,	5·2	3·3
Acetic	,,	2·3	1·2
Sulphuric	,,	65·4	66·7
Oxalic	,,	22·6	—
Tartaric	,,	7·5	5·2
Malic	,,	4·7	2·9
Succinic	,,	2·5	1·5
Citric	,,	4·0	—
Phosphoric	,,	3·6	—
Arsenic	,,	3·5	—

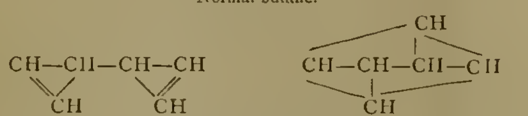
M. Spring continues his researches into the influence of great pressure upon chemical reactions: at a pressure of about

6500 atmospheres he finds that sulphur combines with magnesium, zinc, iron, cadmium, bismuth, lead, copper, silver, tin, and antimony. Sulphur and phosphorus do not combine when compressed together (*Berichte*, xvi. 999).

BENZENE is perhaps the most important body in the whole range of chemistry, not on account of any intrinsic interest in the substance itself, but because of the immense number of its derivatives. The constitution of these derivatives must depend upon the structure of the benzene molecule itself, and this problem is therefore one of the most interesting that presents itself to the chemist. Any idea that can throw light upon this subject is worthy of attention, and the more so as long as the least doubt exists as to whether benzene can yield more than three di, tri, or tetra derivatives, or more than one mono or penta derivative, the substituting groups being the same. Again, it is possible that benzene may exist in two or more isomeric modifications (disregarding dipropargyl), and the difference found by V. Meyer (*Ber.* xv. 2893) between two samples of benzene, both presumably pure, would seem to point in this direction. The mere fact, therefore, that one formula is good and useful is no condemnation of any other formula that may be proposed. M. Mendeléeff has suggested that benzene may be regarded as a normal butane, in which six hydrogen atoms are replaced by two triad groups, CH. If we allow that benzene is best represented as containing six CH groups, and there seems as yet no reason for departing from this supposition, then this replacement may take place in two ways, as shown by the following formulæ:—

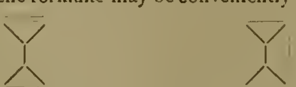
$$CH_3—CH_2—CH_2—CH_3$$

Normal butane.



Benzenes derived therefrom.

These two benzene formulæ may be conveniently written thus:—



and these expressions show at a glance the difference between them. The second is identical with Ladenburg's prism formula, the advantages of which do not need recounting. The first, so far as double and single linkings are concerned, is intermediate between the prism formula and Kekulé's. It lends itself in a particularly ready way to the expression of more complex formulæ, as of naphthaline, &c., but does not show the hexad nature of the benzene molecule. Moreover, it shows possible two mono or penta derivatives, and five each di, tri, and tetra derivatives, a capability that is not yet needed; and a formula should be a concise expression of facts, and should as far as possible show the limits of those facts. Thus, however valuable the suggestion of M. Mendeléeff may be as showing a possible method of synthesising benzene, it does not appear to be practically useful as indicating its constitution, though the future chemistry of benzene may require such a formula as the one referred to above.

PROF. MENDELÉEFF, to avoid the superheating which takes place during ordinary fractional distillation with a dephlegmator tube, has devised a modified method for the oils from Baku petroleums boiling between 15° and 150°, which consists in passing the vapours from the distilling flask by means of the dephlegmator, or delivery tube, to the bottom of a second similar flask, and from this to a third, and so on; the heated vapours from the one providing the necessary heat for the distillation of the next, &c. In this manner a great number of fractions at intervals of two degrees were obtained. By comparing boiling points and specific gravities of products the author concludes that Baku oils contain similar hydrocarbons to American petroleum, and in addition a hydrocarbon boiling at 55° and same specific gravity as hexan with the properties of an unsaturated compound. The great bulk of the Caspian petroleums appears to consist, in addition to derivatives of marsh gas, of C_nH_{2n} hydrocarbons, and also some members of the C_nH_n or acetylene series.

SOME interesting results have been obtained by Spring (*Ber. Ber.*) by washing precipitated sulphide of copper for several weeks until all traces of salts were removed. It was then found that the sulphide dissolved to a black liquid, with slight green fluorescence, in water. The solution might be boiled and evaporated without change; slight traces of salts caused precipitation. The author has also obtained sulphide of tin and oxides of antimony and manganese in a condition perfectly soluble in water. Sulphide of tin on evaporation of its solution in vacuo forms a transparent red glass.

GEOGRAPHICAL NOTES

ON June 6 Baron Nordenskjöld's Greenland expedition arrived at Reykjavik in the steamer *Sophia*. The *Sophia* lay at Reykjavik for a few days, and in the meantime Baron Nordenskjöld and the geologists of the party examined the coal deposits which occur in Bergarfiord. Dr. Arpi, a Swedish philologist, who has resided some time in Iceland and acquired a thorough knowledge of the language, accompanied the expedition thither, and will, along with two other men of science, remain in Iceland after the *Sophia* has left.

WE learn from *Science* that a party for the relief of the observers under Lieut. Greely at Lady Franklin Bay was to leave St. John's, Newfoundland, on one of the steam sealing-vessels belonging to that port, about June 15, probably accompanied by a naval vessel as tender. It will be commanded by Lieut. E. A. Garlington, U.S.A., and composed of twelve men, of whom ten are stated to be old sailors and accustomed to the use of boats. Twenty dogs, native drivers, and a supply of fur clothing, have been secured at Godhavn, Greenland. The party at Lady Franklin Bay will be reached and withdrawn if the state of the ice permits. If not, the relief party is to be landed on Littleton Island, and while part of them are engaged in preparing winter quarters, Lieut. Garlington will endeavour to open communication by sledges with Greely's people. In the failure of the first attempt, another will be made in the spring of 1884. It is to be hoped, if Greely is not reached, that an attempt will be made to leave at Cape Hawkes or Cape Sabine, if not the relief party as a whole, which would be best, at least a boat by which the open water to be anticipated between those points and Littleton Island next year (1884) may be passed by a retreating party, which might well find their own boat unseaworthy after dragging it over many miles of hummocky ice, if, indeed, they did not find themselves obliged to abandon it. Further, the schooner *Leo* is on the point of sailing for Point Barrow to withdraw the signal service observing party under Lieut. Ray, in compliance with the act passed by the last Congress. To utilise the opportunity, Mr. Marr, of the U.S. Coast-Survey, will accompany the vessel with the design of making absolute magnetic determinations, of fixing the astronomical position of the station, and of making pendulum observations.

IN a communication from the Russian Geographical Society we are informed that Col. Prejevalsky is about to start on his fourth journey to Central Asia, accompanied by two officers and seventeen men. The Emperor has granted to the Society 43,000 roubles for the purpose of Col. Prejevalsky's journey. The Society is also sending out a new expedition under M. Potanin, who is now completing the publication of the two last volumes on his journey of 1879–80. He will start in July for South-East Mongolia and the adjacent parts of China; he will be accompanied by a naturalist and M. Skassi, the companion of Severtzov in his exploration of the Pamir. The funds are being supplied partly by the Society and partly by M. Sookachef, a Siberian merchant.

IN the same communication we are informed that the average temperature of January and February at the Russian Polar station at Sagastyr, on the mouth of the Lena, was about −50° C. Thanks to the Governor-General of Eastern Siberia there has been organised a special postal service between Jakutsk and Sagastyr once a month. The observing party will most probably remain at the station up to the end of October, *i.e.* until the river is frozen.

THE last number of the *Zeitschrift der Gesellschaft fur Erdkunde zu Berlin* (Bd. 18, Heft 2) contains a paper by Herr van Langegg, entitled "Nara eine alte Kaiserstadt," describing the town of Nara, not far from Kioto, in Japan, at one time the capital of the country, and still much renowned for its temples. The celebrated colossal statue of Buddha there is fully described. The following figures give some notion of its dimensions:—Its weight is 500,000 kilog.; 3,000,000 kilog. of wood were con-

sumed in making the bronze, which consists of 250 kilog. of gold, 8413·5 of tin, 977 of mercury, and 493 of copper. The present image only dates from 1801.

We have received a German pamphlet by Herr Max Buch, on "Finland and its Nationality Question," being a reprint of papers which have appeared in recent issues of the *Ausland*. In the limited space of seventy-four pages the author gives a short but correct description of Finland, of the prehistoric Finns, according to Ahlquist's researches, of the history of the country, and of the present state of the "national question." He summarises the excellent researches by Retzius on the race-characters of the Finns—as far as can be done in a few pages—and dwells upon the recent efforts of Finnish writers towards the development of the Finnish language and literature as a reaction against the former supremacy of the Swedish language and influence. We notice the interesting fact that although only 7·5 per cent. of the Finns can now read and write, and 70 per cent. read, primary instruction has taken during the last few years a great extension. The number of State schools being too limited, they are supplemented by private instruction. Thus, of the 342,836 children from seven to sixteen years old of the Lutheran Finnish population, only 6983 had not received primary instruction in 1877 (1801 of them on account of disease). But only 26,900 went to the State schools, whilst 116,201 children received primary instruction in private ambulatory schools, and 177,925 at home.

The last number of the *Izvestia* of the Russian Geographical Society contains several interesting papers. M. Veselago gives a sketch of the life and work of the late Count Lütke. Prof. Fr. Schmidt discusses again the claim of Wrangel to the discovery of the land situated north of the Cape Yakan. He tries to prove, against Nordenskjöld, that Wrangel was right in denying the existence of a land which Andréeff said he saw from the fifth island of the Medvyejiy Archipelago; but he did not deny the existence of a land situated north of Cape Yakan. Prof. Schmidt admits, however, that even with regard to this land, Wrangel wrote "in different parts of his report with a varying degree of certainty as to the probability of its existence." M. Karzin, an official of the Verkhoyansk district, having been struck with the terrible fate of De Long, publishes a most valuable list of all settlements and places where human beings can be met with at different seasons on the coast of North-Eastern Siberia. M. Andréeff publishes a brief account of his hydrographical researches in the White Sea and on the Murman coast during the last three years. The flora of the coast becomes very poor north of Archangel. At the Svyatoy Noss lighthouse it consists only of lichens, mosses, and creeping brushes of *Betula nana*. It improves, however, west of Kildin and especially west of the Ribachiy peninsula, offering excellent forests and meadows at the new colony at Pechenga. The yearly average temperature, which is but $-0°·6$ Celsius at Archangel, and $-2°·4$ at the Svyatoy Noss lighthouse, reaches $-1°·1$ at Kola, and $+1°·4$ at Vardö. This increase of temperature is due, as is known, to the warm current which flows along the coast. Thus, at Svyatoy Noss, during the hottest days, the temperature of water does not exceed 6°·9; and during the autumn it reaches but 1°·9. To the west of 30° 6′ it suddenly becomes double that. In the spring the warm streamlets reach 4°·3, whilst the cold ones, flowing close by, reach but 1°·9; and during the summer the warm streamlets reach 12°·5, whilst the cold ones, close by, reach 6°·9 to 7°·5. It appears thus that one isolated measurement of temperature of water is of little value, the warm current being not so compact along the Murman coast as elsewhere. Under 33° 6′ E. long. it leaves the coast and flows towards the north-north-east. The positions taken by the warm current at the Murman coast vary with the seasons, and depend upon the prevailing winds. From April to August it touches the coast, but later on it is driven north by the southern winds; in October it already flows off Vardö. Its position varies also for different years, depending upon the prevailing winds. The richness of the fishing depends entirely upon the position taken by the warm current. In 1881, the Norwegians, owing to the current flowing in their waters, had the richest prey, whilst in 1882, the richest prey for a twenty years' series was given by the warm current to the Russian fishers. The same number of the *Izvestia* contains the first sheets of M. Polakoff's reports on his researches in Sakhalin, and M. Mezhoff's bibliographical index of the Russian geographical literature for the year 1880.

M. Thouar, the French traveller, has written a letter from Chili, in which he says that several members of the exploring party under Dr. Jules Crevaux, who was massacred with most of his followers in the early part of last year by Indians while making explorations along the Bolivian part of the Pilcomayo, are believed to be still alive, but prisoners in the hands of the Indians.

THE CAUSE OF EVIDENT MAGNETISM IN IRON, STEEL, AND OTHER MAGNETIC METALS [1]

Neutrality

The apparatus needed for researches upon evident external polarity requires no very great skill or thought, but simply an apparatus to measure correctly the force of the evident repulsion or attraction; in the case of neutrality, however, the external polarity disappears, and we consequently require special apparatus, together with the utmost care and reflection in its use.

From numerous researches previously made by means of the induction balance, the results of which I have already published, I felt convinced that in investigating the cause of magnetism and neutrality I should have in it the aid of the most powerful instrument of research ever brought to bear upon the molecular construction of iron, as indeed of all metals. It neglects all forces which do not produce a change in the molecular structure, and enables us to penetrate at once to the interior of a magnet or piece of iron, observing only its peculiar structure and the change which takes place during magnetisation or apparent neutrality.

The induction balance is affected by three distinct arrangements of molecular structure in iron and steel, by means of which we have apparent external neutrality.

Fig. 1 shows several polar directions of the molecules as indicated by the arrows. Poisson assumed, as a necessity of his theory, that a molecule is spherical, but Dr. Joule's experimental proof of the elongation of iron by 1/720,000th of its length when magnetised, proves at least that its form is not spherical; and as I am unable at present to demonstrate my own views as to its exact form, I have simply indicated its polar direction by arrows—the dotted oval lines merely indicating its limits of free elastic rotation.

In Fig. 1, at A, we have neutrality by the mutual attraction of each pair of molecules, being the shortest path in which they could satisfy their mutual attractions. At B we have the case of superposed magnetism of equal external value, rendering the wire or rod apparently neutral, although a lower series of molecules are rotated in the opposite direction to the upper series, giving to the rod opposite and equal polarities. At C we have the molecules arranged in a circular chain around the axis of a wire or rod through which an electric current has passed. At D we have the evident polarity induced by the earth's directive influence when a soft iron rod is held in the magnetic meridian. At E we have a longitudinal neutrality produced in the same rod when placed magnetic west, the polarity in the latter case being transversal.

In all these cases we have a perfectly symmetrical arrangement, and I have not yet found a single case in well-annealed soft iron in which I could detect a heterogeneous arrangement, as supposed by Ampère, De la Rive, Weber, Wiedermann, and Maxwell.

We can only study neutrality with perfectly soft Swedish iron. Hard iron and steel retain previous magnetisations, and an apparent external neutrality would in most cases be the superposition of one magnetism upon another of equal external force in the opposite direction, as shown in B, Fig. 1. Perfectly soft iron we can easily free, by vibrations, from the slightest trace of previous magnetism, and study the neutrality produced under varying conditions.

If we take a flat bar of soft iron, of 30 or more centimetres in length, and hold it vertically (giving while thus held a few torsions, vibrations, or, better still, a few slight blows with a wooden mallet, in order to allow its molecules to rotate with perfect freedom), we find its lower end to be of strong north polarity, and its upper end south. On reversing the rod and repeating the vibrations, we find that its lower end has pre-

[1] Paper read before the Society of Telegraph Engineers and of Electricians, on May 24, 1883, by Prof. D. E. Hughes, F.R.S., Vice-President Continued from p. 162.

cisely a similar north polarity. Thus the iron is homogeneous, and its polarity symmetrical. If we now magnetise this rod to produce a strong south pole at its lower portion, we can gradually reverse this polarity, by the influence of earth's magnetism, by slightly tapping the upper extremity with a small wooden mallet. If we observe this rod by means of a direction needle at all parts, and successively during its gradual passage from one polarity to the other, there will be no sudden break into a haphazard arrangement, but a gradual and perfectly symmetrical rotation from one direction to that of the opposite polarity.

If this rod is placed east and west, having first, say, a north polarity to the right, we can gradually discharge or rotate the molecules to zero, and as gradually reverse the polarity by simply inclining the rod so as to be slightly influenced by earth's magnetism; and at no portion of this passage from one polarity to neutrality, and to that of the opposite name, will there be found a break of continuity of rotation or haphazard arrangement. If we rotate this rod slowly, horizontally or vertically, taking observations at each few degrees of rotation of an entire revolution, we find still the same gradual symmetrical change of polarity, and that its symmetry is as complete at neutrality as in evident polarity.

In all these cases there is no complete neutrality, the longitudinal polarity simply becoming transversal when the rod is east and west. F, G, H, I, J, Fig. 1, show this gradual change, H being neutral longitudinally, but polarised transversely. If, in place of the rod, we take a small square soft iron plate and allow its molecules freedom under the sole influence of the earth's magnetism, then we invariably find the polarity in the direction of the magnetic dip, no matter in what position it be held, and a sphere of soft iron could only be polarised in a similar direction. Thus we can never obtain complete external neutrality whilst the molecules have freedom and do not form an internal closed circle of mutual attractions; and whatever theory we may adopt as to the cause of polarity in the molecule, such as Coulomb's, Poisson's, Ampère's, or Weber's, there can exist no haphazard arrangement in perfectly soft iron, as long as it is free from all external causes except the influence of the earth; consequently these theories are wrong in one of their most essential parts.

We can, however, produce a closed circle of mutual attraction in iron and steel, producing complete neutrality as long as the structure is not destroyed by some stronger external directing influence.

Oersted discovered that an external magnetic needle places itself perpendicular to an electric current; and we should expect that, if the molecules of an iron wire possessed inherent polarity and could rotate, a similar effect would take place in the interior of the wire to that observed by Oersted. Wiedermann first remarked this effect, and it has been known as circular magnetism. This circle, however, consists really in each molecule having placed itself perpendicular to the current, simply obeying Oersted's law, and thus forming a complete circle in which the mutual attractions of the molecules forming that circle are satisfied, as shown at C, Fig. 1. This wire becomes completely neutral, any previous symmetrical arrangement of polarity rotating to form its complete circle of attractions; and we can thus form in hard iron and steel a neutrality extremely difficult to break up or destroy. We have evident proof that this

neutrality consists of a closed chain, or circle, as by torsion we can partially deflect them on either side; thus, from a perfect externally neutral wire, producing either polarity, by simple mechanical angular displacement of the molecules, as by right- or left-handed torsion.

If we magnetise a wire placed east and west, it will retain this polarity until freed by vibrations, as already remarked. If we pass an electric current through this magnetised wire, we can notice the gradual rotation of the molecules, and the formation of the circular neutrality. If we commence with a weak current, gradually increasing its strength, we can rotate them as slowly as may be desired. There is no sudden break or haphazard moment of neutrality: the movements to perfect zero are accomplished with perfect symmetry throughout.

We can produce a more perfect and shorter circle of attractions by the superposition of magnetism, as at B, Fig. 1. If we magnetise a piece of steel or iron in a given direction with a strong magnetic directing power, the magnetism penetrates to a certain depth. If we slightly diminish the magnetising power, and magnetise the rod in a contrary direction, we may reduce it to zero by the superposition of an exterior magnetism upon one of a contrary name existing at a greater depth; and if we continue this operation, gradually diminishing the force at each reversal, we can easily superpose ten or more distinct symmetrical arrangements, and as their mutual attractions are satisfied in a shorter circle than that produced by electricity, it is extremely difficult to destroy this formation when once produced.

The induction balance affords also some reasons for believing that the molecules not only form a closed circle of attractions, as at B, but that they can mutually react upon each other, so as to close a circle of attractions as a double molecule, as shown at A. The experimental evidence, however, is not sufficient to dwell on this point, as the neutrality obtained by superposition is somewhat similar in its external effects.

We can produce a perfectly symmetrical closed circle of attractions of the nature of the neutrality of C, Fig. 1, by forming a steel wire into a closed circle, 10 centimetres in diameter, if this wire is well joined at its extremities by twisting and soldering. We can then magnetise this ring by slowly revolving it at the extremity of one pole of a strong permanent magnet; and, to avoid consequent poles at the part last touching the magnet, we should have a graduating wedge of wood, so that whilst revolving, it may be gradually removed to greater distance. This wire will then contain no consequent points or external magnetism: it will be found perfectly neutral in all parts of its closed circle. Its neutrality is similar to C, Fig. 1; for if we cut this wire at any point we find extremely strong magnetic polarity, being magnetised by this method to saturation, and having retained (which it will indefinitely) its circle of attractions complete.

I have already shown that soft iron, when its molecules are allowed perfect freedom by vibration, invariably takes the polarity of the external directing influence, such as that of the earth, and it does so even with greater freedom under the influence of heat. Manufacturers of electromagnets for telegraphic instruments are very careful to choose the softest iron and thoroughly anneal it; but very few recognise the importance as regards the position of the iron whilst annealing it under the earth's directing influence. The fact, however, has long since been observed.

Dr. Hooke (1684) remarked that steel or iron was magnetised when heated to redness and placed in the magnetic meridian. I have slightly varied this experiment by heating to redness three similar steel bars, two of which had been previously magnetised

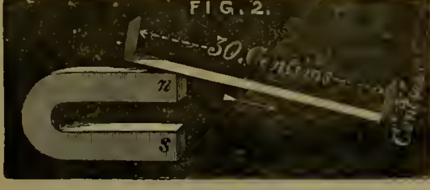
to saturation, and placed separately with contrary polarity as regards each other, the third being neutral. Upon cooling, these three bars were found to have identical and similar polarity. Thus the molecules of this most rigid material, cast steel, had become free at red heat, and rotated under the earth's magnetic influence, giving exactly the same force on each; consequently the previous magnetisation of two of these bars had neither augmented nor weakened the inherent polarity of their molecules. Soft iron gave under these conditions by far the greatest force, its inherent polarity being greater than that of steel.

I have made numerous other experiments bearing upon the question of neutrality, but they all confirm those I have cited, which I consider afford ample evidence of the symmetrical arrangement of neutrality.

Superposed Magnetism.—Knowing that by torsion we can rotate or diminish magnetism, I was anxious to obtain by its means a complete rotation from north polarity to neutrality, and from neutrality to south polarity, or to completely reverse magnetic polarity by a slight right or left torsion.

I have succeeded in doing this and in obtaining strong reversal of polarities by superposing one polarity given whilst the rod is under a right elastic torsion, with another of the opposite polarity given under a left elastic torsion, the neutral point then being reached when the rod is free from torsion. The rod should be very strongly magnetised under its first or right-hand torsion, so that its interior molecules are rotated, or, in other words, magnetised to saturation; the second magnetisation in the contrary sense and torsion should be feebler, so as only to magnetise the surface, or not more than one-half its depth: these can be easily adjusted to each other so as to form a complete polar balance of force, producing, when the rod is free from torsion, the neutrality as shown at B, Fig. 1.

The apparatus needed is simply a good compound horseshoe permanent magnet, 15 centimetres long, having six or more plates, giving it a total thickness of at least 3 centimetres. We need a sufficiently powerful magnet, as I find that I obtain a more equal distribution of magnetism upon a rod or strip of iron by drawing it lengthwise over a single pole in a direction from



that pole, as shown in Fig. 2; we can then obtain saturation by repeated drawings, keeping the same molecular symmetry in each experiment.

In order to apply a slight elastic torsion when magnetising rods or wires, I have found it convenient to attach two brass clamp keys to the extremities of the rods, or simply turn the ends at right angles, as shown in the following diagram, by which mean we can apply an elastic twist or torsion whilst drawing the rod over the pole of the permanent magnet. We can thus superpose several and opposite symmetrical structures, producing a polar north or south as desired, greatly in excess of that possible under a single or even double magnetisation, and by carefully adjusting the proportion of opposing magnetisms so that both polarities have the same external force, the rod will be at perfect external neutrality when free from torsion.

If we now hold one end of this rod at a few centimetres distance from a magnetic directive needle, we find it perfectly neutral when free of torsion, but the slightest torsion right or left at once produces violent repulsion or attraction, according to the direction of the torsion given to the rod, the iron rod or strips of hoop-iron which I use for this experiment being able, when at the distance of 5 centimetres from the needle, to turn it instantly 90° on either side of its zero.

The external neutrality that we can now produce at will is absolute, as it crosses the line of two contrary polarities, being similar to the zero of my electric sonometer, whose zero is obtained by the crossing of two opposing electric forces.

This rod of iron retains its peculiar powers of reversal in a remarkable degree, a condition quite different to that of ordinary magnetisation, for the same rod, when magnetised to saturation under a single ordinary magnetism, loses its evident magnetism by a few elastic torsions, as I have already shown; but when it is magnetised under the double torsion with its superposed magnetism, it is but slightly reduced by variations or numerous torsions, and I have found it impossible to render this rod again free from its double polar effects, except by strongly remagnetising it to saturation with a single polarity. The superposed magnetism then becomes a single directive force, and we can then by a few vibrations or torsions reduce the rod to its ordinary condition.

The effects of superposed magnetism and its double polarity I have produced in a variety of ways, such as by the electro-magnetic influence of coils, or in very soft iron simply by the directive influence of the earth's magnetism, reversing the rod and torsions when held in the magnetic meridian, these rods when placed magnetic west showing distinctly the double polar effects.

It is remarkable, also, that we are enabled to superpose and obtain the maximum effects on thin strips of iron from a quarter to half a millimetre in thickness, whilst in thicker rods we have far less effect, being masked by the comparatively neutral state of the interior, the exterior molecules then reacting upon those of the interior, allowing them to complete in the interior their circle of attractions.

I was anxious to obtain wires which would preserve this structure against the destructive influence of torsion and vibrations, so that I could constantly employ the same wires without the comparatively long and tedious process of preparation. Soft iron soon loses the structure or becomes enfeebled under the constant to and fro torsions requisite where we desire a constant change of polarity, as described later in the magnetic bells. Hard steel preserves its structure, but its molecular rigidity is so great that we obtain but mere traces of any change of polarity by torsion. I have found, however, that fine cast drill steel, untempered, of the kind employed by watchmakers, is most suitable: these are generally sold in straight lengths of 30 centimetres. Wires 1 millimetre in diameter should be used, and when it is desired to increase the force several of these wires, say nine or ten, should be formed into a single rod or bunch.

The wire as sold is too rigid to give its maximum of molecular rotation effect. We must therefore give it two entire turns or twists to the right, and strongly magnetise it on the north pole of the magnet whilst under torsion. We must again repeat this operation in the contrary direction, after restoring the wire to its previous position, giving now two entire turns to the left and magnetising it on the south pole. On restoring the wire to its original place it will be extremely flexible, and we may now superpose several contrary polarities under contrary torsions, as already described.

The power of these wires, if properly prepared, is most remarkable, being able to reverse their polarity under torsion, as if they were completely saturated; and they preserve this power indefinitely if not touched by a magnet. It would be extremely difficult to explain the action of the rotative effects obtained in these wires under any other theory than that which I have advanced; and the absolute external neutrality that we obtain in them when the polarities are changing, we know from their structure to be perfectly symmetrical.

I was anxious to show, upon the reading of this paper, some mechanical movement produced by molecular rotation, consequently I have arranged two bells that are struck alternately by a polarised armature put in motion by the double polarised rod I have already described, but whose position, at 3 centimetres distant from the axis of the armature, remains invariably the same. The magnetic armature consists of a horizontal light steel bar suspended by its central axle; the bells are thin wine glasses, giving a clear musical tone loud enough, by the force with which they are struck, to be clearly heard at some distance. The armature does not strike these alternately by a pendulous movement, as we may easily strike only one continuously, the friction and inertia of the armature causing its movements to be perfectly dead-beat when not driven by some external force, and it is kept in its zero position by a strong directive magnet placed beneath its axle.

The mechanical power obtained is extremely evident, and is sufficient to put the sluggish armature in rapid motion, striking the bells six times per second, and with a power sufficient to produce tones loud enough to be clearly heard in all parts of the hall of the Society. As this is the first direct transformation of molecular motion into mechanical movement, I am happy to show it on this occasion.

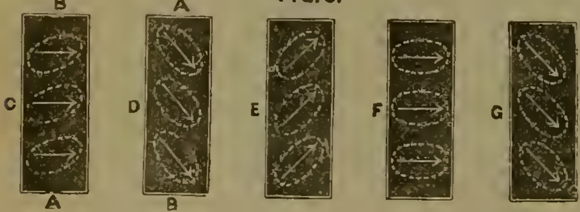
There is nothing remarkable in the bells themselves, as they evidently could be rung if the armature was surrounded by a coil, and worked by an electric current from a few cells. The marvel, however, is in the small steel superposed magnetic wire producing by slight elastic torsions from a single wire, 1 millimetre in diameter, sufficient force from mere molecular rotation to entirely replace the coil and electric current.

Elastic Nature of the Ether surrounding the Magnetic Molecules.—During these researches I have remarked a peculiar property of magnetism, viz., that not only can the molecules be rotated through any degree of arc to its maximum, or saturation, but that, whilst it requires a comparatively strong force to overcome its rigidity or resistance to rotation, it has a small field of its own through which it can move with excessive freedom, trembling, vibrating, or rotating through a small degree with infinitely less force than would be required to rotate it permanently on either side. This property is so marked and general that we can observe it without any special iron or apparatus.

Let us take a flat rod of ordinary hoop-iron, 30 or more centimetres in length. If, whilst holding this vertically, we give freedom to its molecules by torsions, vibrations, or, better still, by a few blows with a wooden mallet upon its upper extremity, we find, as is well known, that its lower portion is strongly north, and its upper south. If we reverse this rod, we now find it neutral at both extremities. We might here suppose that the earth's directing force had rotated the molecules to zero or transversely, which in reality it has done, but only to the limit of their comparatively free motion; for if we reverse the rod to its original position, its previous strong polarity reappears at both extremities, thus the central point of its free motion is inclined to the rod, giving by its free motion great symmetrical inclination and polarity in one direction, but when reversed the inclination is reduced to zero.

In Fig. 3 D shows the bar of iron when strongly polarised by earth's magnetic influence, under vibrations, with a sufficient

FIG. 3.



force to have rotated its elastic centre of action. C shows the same bar with its molecules at zero, or transversal, the directing force of earth being insufficient without the aid of mechanical vibration to allow them to change. The dotted lines of D suppose the molecule to be in the centre of its free motion, whilst at C the molecules have rotated to zero, as they are prevented from further rotation by being at the extreme end of its free motion

If, now, we hold the rod vertically, as at C, giving neutrality, and give a few slight blows with a wooden mallet to its upper extremity, we can give just the amount of freedom required for it to produce evident polarity, and we then have equal polarity no matter which end of the bar is below, the centre of its free rotation here being perfect, and the rod perfectly neutral longitudinally when held east and west. If, on the other hand, we have given too much freedom by repeated blows of the mallet, its centre of free motion becomes inclined with the molecules, and we arrive at its first condition, except that it is now neutral at D and polarised at C. From this it will be seen that we can adjust this centre of action, by vibrations or blows, to any point within the external directing influence.

We can perceive this effect of free rotation in a limited space in all classes of iron and steel, being far greater in soft Swedish iron than in hard iron or steel. A similar phenomenon takes place if we magnetise a rod held vertically in the direction of earth's magnetism. It then gives greater polarity than if magnetised east or west, and if magnetised in a contrary sense to earth's magnetism, it is very feebly magnetised, or, if the rod is perfectly soft, it becomes neutral after strong magnetisation. This property of comparative freedom, and the rotation of its centre of action, can be demonstrated in a variety of ways. One remarkable example of it consists in the telephone. All those who are thoroughly acquainted with electro-magnetism and know that it requires measurable time to charge an electro-magnet to saturation (about one-fifteenth of a second for those employed in telegraphy), were surprised that the telephone could follow the slightest change of timbre, requiring almost innumerable changes of force per second. I believe the free rotation I have spoken of through a limited range explains its remarkable sensitiveness and rapidity of action, and, according to this view, it would also explain why loud sounding telephones can never repeat all the delicacy of timbre that is easily done with those only requiring a force comprised in the critical limits of its free rotation. This property, I have found, has a distinct critical value for each class of iron. and I propose soon to publish researches upon the molecular construction of steel and iron, in which I have made use of this very property as a guide to the quality of the iron itself.

The elastic rotation (in a limited space) of a molecule differs entirely from that known as mechanical elasticity. In perfectly soft iron we have feeble *mechanical* elasticity, whilst in tempered steel we have that elasticity at its maximum. The contrary takes place as regards *molecular* elasticity. In tempered steel the molecules are extremely rigid, and in soft iron its molecular elasticity is at its maximum. Its free motion differs entirely from that given it by torsion or stress. We may assume that a molecule is surrounded by continuous ether, more of the nature of a jelly than of that of a gas: in such a medium a molecule might freely vibrate through small arcs, but a rotation extending beyond its critical limit would involve a much greater expenditure of force.

The discovery of this comparatively free rotation of molecules, by means of which, as I have shown, we can (without in any degree disturbing the external mechanical elasticity of the mass) change the axes of their free motion in any direction desired, has led me into a series of researches which have only indirectly any relation with the theory of magnetism. I was extremely desirous, however, of finding an experimental evidence which in itself should demonstrate all portions of the theory, and the following experiment, I believe, answers this purpose.

Let us take a square soft iron rod, 5 millimetres in diameter by 30 or more centimetres in length, and force the molecules, by aid of blows from a wooden mallet, as previously described, to have their centres of free motion in one direction, the rod will (as already shown) have polarity at both ends, when held vertically; but if reversed, both ends become completely neutral.

If now we turn the rod to its first position, in which it shows strong polarity, and magnetise it whilst held vertically, by drawing the north pole of a sufficiently powerful permanent magnet from its upper to its lower extremity, we find that this rod, instead of having south polarity at its lower portion, as we should expect from the direction of the magnetisation, is completely neutral at both extremities, but if we reverse the rod, its fullest free powers of magnetisation now appear in the position where it was previously neutral. Thus, by magnetisation, we have completely rotated its free path of action, and find that we can rotate this path as desired in any direction by the application of a sufficient directing power.

If we take a rod as described, with its polarities evident when held vertically, and its neutrality also evident when its ends are reversed in the same magnetic field, we find that its polarity is equal at both ends, and that it is in every way symmetrical with a perfect magnet. If we *gradually* reverse the ends and take observations of its condition through each degree of arc passed over, we find an equal symmetrical diminution of evident external polarity until we arrive at neutrality, when it has no external trace of inherent polarity, but its inherent polarity at once becomes evident by a simple return to its former position. Thus the rod has passed through all the changes from polarity to neutrality, and from neutrality to polarity, and these changes have taken place with complete symmetry.

The limits of this paper do not allow me to speak of the numerous theoretical evidences as shown by the use of my induction balance. I believe, however, that I have cited already experimental evidences to show that what has been attributed to coercive force is really due to molecular freedom or rigidity; that in inherent molecular polarity we have a fact admitted by Coulomb, Poisson, Ampère, De la Rive, Weber, Du Moncel, Wiedermann, and Maxwell; and that we have also experimental evidence of molecular rotation and of the symmetrical character of polarity and neutrality.

The experiments which I have brought forward in this paper,

in addition to those mentioned in my paper read before the Royal Society, will, I hope, justify me in having advanced a theory of magnetism which I believe in every portion allows at least experimental evidences of its probable truth.

THE REDE LECTURE

THE following abstract report of Prof. Huxley's Rede Lecture given on Tuesday week in the Cambridge Senate House, to a crowded audience, has been revised, to the extent of removing any errors of importance, by the author. We understand that a full report of the lecture will shortly be published in a separate form.

Professor Huxley said he had undertaken to treat in the course of such time as custom and the patience of his audience might permit, on a very great subject, no less a subject than the origin of all those forms of animal life which at present existed. It had behoved him to restrict what he might lay before them to those considerations which were absolutely essential for his purpose, and he should endeavour to lay before them facts of such an order as appeared to him to be of most importance in reference to his argument. Although he might fail to put those facts before them as clearly as they presented themselves to his own mind, the reasonings which might be based upon them were of so simple an order that he should consider his task performed if he gave them a tolerably clear conception of what those facts were, for he did not think it was the business of a man of science to use the arts of rhetoric or endeavour to procure persuasion. His sole business was to place the facts before those whom he wished to teach, and to leave it to their reason to form such judgment upon those facts as they might think fit. In the present case he should point out to them what judgments such facts had forced upon his mind, but he must leave it entirely to their responsibility to say what judgment they might constrain them to give in their case. They might assume this position at starting, that, whatever in such a matter was true for one animal, was true for the infinite series of the whole animal world; and as he was extremely anxious to avoid everything speculative, everything that could not be directly led back to the matters of fact upon which it was based, he proposed to select one animal particularly, and to put before them facts and arguments by the help of which they might form some probable conclusion as to the origin of that object. He took it for granted that, if the evidence inclined towards a particular conclusion in the case of that animal, they might assume that it would incline in the same direction with regard to all. He had no doubt that a great many of his audience were familiar at any rate with the shell of the animal about which he was going to speak, namely, that of the pearly nautilus, from which, or parts of which, very beautiful ornaments were fabricated. At the present time the nautilus inhabited the warmer parts of the Indian and Pacific Oceans, living at considerable depths and preying upon the hard shelled crustaceans and mollusks that crept along the bottom, and which it found in its way. For that end it was provided with a very curious beak, shaped like that of a parrot, but with each portion covered with a hard calcareous deposit, and which enabled it to be an efficient instrument for crushing its prey. If he were to touch upon the morphological problem which here presented itself, he could occupy far more time than they had at their disposal with the consideration of a multitude of interesting peculiarities which the nautilus presented, for it was one of those forms which at present stood almost isolated and alone in the animal world, separated by a wide gulf from its nearest allies, those animals which they knew as squids and cuttle-fishes. It held the middle place between sea-snails and the group of the cuttle-fishes. It would be, however, entirely out of place at present, and a purposeless waste of time if he were to touch upon any peculiarities except those which would be needed during his further argument. The only points to which he would direct their attention for that purpose were the facts which related to the structure of the shell. There was a diagram beside him showing a part of the nautilus shell in section, but he thought it possible that he could make the matter clearer by roughly sketching on the board the main points as he went on.—Prof. Huxley here described, with the aid of diagrams, preserved specimens, and models, the complicated structure of the shells of the pearly nautilus, or *Nautilus pompilius*. The animal itself was contained in the spacious chamber in the outer part of the shell, which was divided from the rest of the shell by a partition. The rest of the shell resembled a long cone closely coiled up, and divided by partitions at regular intervals into other chambers, which succeeded one another, and in the full-grown animal were full of air. From the hinder part of the animal's body a long tube, the siphuncle, was carried backwards through the whole of the shell, and as it completely filled up the openings in the partitions through which it passed there was no communication between one chamber and another. The first point to be considered was as to what was the origin of the particular nautilus in the bottle before him. Happily there was no dispute upon that point. The female nautilus contained eggs exactly as the hen did. These eggs were small masses of protoplasmic matter, each containing a nucleus in its centre, which was all that was essential. They knew that that pearly nautilus with all its complicated organism, and fitted with the complicated shell he had described, did, in some way or other, proceed from that relatively structureless body which they called the egg or the ovum. As fate would have it, up to the present they had known nothing from direct observation of the process by which that particular animal was produced from this microscopic particle. But they had so large a knowledge of the process in other animals of every description that there was no doubt whatever as to the nature of the process, which he would try to describe to them as briefly as possible, by reference to the process which took place in the case of the domestic hen. Neither by the highest powers of the microscope, nor by other means of investigation which they had at present, could they trace anything in the slightest degree resembling either the chick, which under certain circumstances proceeded from that egg, or the tissues of the chick. There was, however, one spot on the yolk of the egg, a little careful observation of which would show a clear space, which might be a fifth of an inch in diameter. It was very well known by the name of the cicatricula, or little scar. He would suppose that twenty-one eggs were placed together under the hen. If they took one egg day by day and examined it they would know what took place as if they had watched continuously, for what happened in any one egg happened also in the others. That was a process—the wonder of which he must confess never staled in his mind—by which the chick was gradually fashioned out of that transparent rudiment. They saw it make its appearance in the first place on the surface of the yolk, and to the naked eye it looked like a white streak. That white streak gradually assumed the appearance of a sort of elongated body, and that body shaped itself so that it could be seen that it was going to be an animal of some kind, it having a large head, and the rudiments of eyes and vertebræ. On the fifth day they could clearly see what they were going to have. Gradually, step by step, and moment by moment, new differences made their appearance from the original foundation, and until many days before hatching there was an unmistakable bird, and at the twenty-first day there emerged from the shell an animal endowed with all a bird's capacities and structures. That process was the process of development. If they inquired into the nature of the cicatricula, they would find that that was merely a double layer of minute nucleated cells. They would find that that resulted from the splitting up of a protoplasmic mass that had been there before. They could trace the process back into the body of the hen until they came down to a simple nucleated cell, so that it was a matter capable of demonstration that in that nucleated cell which formed a part of the egg organ of the hen—in that particle of, for morphological purposes, structureless jelly, were the same characteristics which were possessed by the very lowest forms of animal life which were known. They knew that in that particle resided a potentiality, capable of developing itself through the stages he had roughly indicated, until it became not only a machine of the highest order from a physiological point of view, but a very remarkable work of art. That particle of protoplasmic matter did that in virtue of the powers inherent in its material nature. That was the point he wished to put before them as clearly and definitely as he could, because it would be fundamental in all further discussion. For it was to the process he had briefly described that the great discoverers of the last two centuries applied the name of "evolution." Singularly enough the persons who first used that name did not use it in that sense in which it was universally used now, because they were under a mistake as to the exact nature of the process. But the whole conception of evolution was now based upon ascertained facts, showing the process of development of the most complicated animal out of a relatively structureless particle, which had no higher organisation than that of the

lowest animal they knew, a process which progressed step by step by means of the gradual addition of small differences, until the animal attained its perfect form. That was what was meant by the process of evolution. At this point he thought it might be desirable that he should deal with what he might speak of as the *a priori* objections to the doctrines of evolution. He had had opportunities of making extensive acquaintance with those objections during the past twenty years or so. He divided them into three categories: (1) That evolution was impossible; (2) that it was immoral; and (3) that it was opposed to the argument of design. Now that was a very heavy indictment, but he thought they must plead "not guilty" upon all three counts. It required no great amount of reasoning to convince one that that which happened could not be impossible; that that which happened thousands and millions of times every hour and every minute in this world as it now was, under certain conditions, could not be held without further evidence to be impossible under somewhat different conditions. Secondly, with regard to the question of morality. He had never understood that argument, and had always been disposed to reply that the morality which opposed itself to truth committed suicide. With regard to the argument of design he would not discuss that point himself, but would beg them to listen for a moment to words that would carry far more weight than any of his own could carry on that topic:—"The philosopher beholds with astonishment the production of things around him. Unconscious particles of matter take their stations and severally range themselves in an order so as to become collectively plants or animals, *i.e.* organised bodies with parts bearing strict and evident relation to one another and to the utility of the whole; and it should seem that these particles could not move in any other way than they do, for they testify not the smallest sign of choice, or liberty, or discretion. There may be particular intelligent beings guiding their motions in each case, or they may be the results of trains of mechanical dispositions fixed beforehand by intelligence or appointment and kept in action by a power at the centre." They might imagine, and not unreasonably, that those were the words of some ultra-evolutionist of the present day who desired to set himself right with the argument from design; but they were not so. They were more than eighty years old, and they were contained in the 23rd chapter of a book which was very much talked about, but, he was afraid, very little read, namely, the "Natural Theology" of Archdeacon Paley. When he was a boy that book was a very great favourite of his, partly for its own merits, and partly because it was one of the few books he was allowed to read on Sundays. He found it much more entertaining than most of the books included in that category. But from what had been since said of the Atheistic tendencies of the doctrine of evolution he began to think that he stood before them a miserable example of the manner in which a man's mind might be poisoned by early instruction, and that his incapacity to understand the force of the arguments against evolution arose from the circumstance that in his early childhood he was indoctrinated with the reasonings of a great divine of the Church.—Professor Huxley now proceeded to consider the next point, the coming into existence of the nautilus species in contradistinction from the origin of a particular nautilus as an individual. He showed that, according to all the evidence that could be gathered, there was every reason to believe the forms of animal life five thousand years ago were practically the same as they were now. If there were no other means of knowing anything about the history of animal life, undoubtedly this experience, resting upon a duration of five thousand years, would have furnished an apparently sufficient basis for a generalisation, tending to the conclusion that the forms of animal life had not changed during that period. Not only had that generalisation been made, but it had been concluded that the forms of animal life were unchangeable, a totally different proposition, the validity of which rested, among other things, on the proportion between our actual experience, supposing it to extend over that time, and our possible experience of the duration of life on the globe. It would, he thought, be absolutely impossible for any of them, however good their vision, to say from actual observation of the hour hand of a watch for four seconds that it had moved during that interval, and in point of fact the space over which it would move was so minute as to be indiscernible, even through a magnifying glass. Yet they knew very well that it had moved, and if they watched it for four or five minutes, the evidence of its movement would be perfectly obvious, even to the naked eye. They would observe, therefore, that a period of observation which extended over the nine-hundredth part of an hour, would give them no conception from which it would be possible to draw a conclusion as to what had happened during the total period. Now geologists told them that the whole depth and extent of the fossiliferous rocks, which composed a considerable portion of the earth's crust, represented a period of time at least one thousand times as great as the historical period. That was a point upon which there could be no room for hesitation. Hence it followed that when they acquainted themselves with the succession of animal forms which were embedded at different depths in the earth's crust, they did exactly what the observer of a watch did when he kept his eyes fixed on it, not for four seconds but for an hour, in which latter case the movement was not only conspicuous, but such as commonly served to indicate the lapse of time. If that analogy held good, the slow procession of events which might be absolutely indiscernible in the course of 5,000 years, would become obvious and plain when the period of observation was extended to a thousand times that period. And that was exactly what happened, for if they went back in the series of stratified rocks they found the genus nautilus, which in the present day was represented by one or two species, represented in the long period of its history by many other species. As far back as the Upper Silurian formation the genus nautilus was represented by an abundant number of shells fabricated by animals having all the essential peculiarities which he had described. In the geological specimens before him, and which were taken from the rich collection in the Woodwardian Museum, there were forms of nautili which no one doubted were to all intents and purposes the same in their general structure as the pearly nautilus of the present day, although they were at least 5,000,000 years old. Now came the main question: were those nautili whose history extended back through such a prodigious range of time identical in character with the modern species? So far as he knew there was nothing in the nature of things to show why a succession of generations which remained unchanged through 5,000 years should not remain so for 50,000 or 50,000,000 years. The facts, however, showed that there had been rather more than 100 distinct species of nautilus, each having as good a title to be called a species as *Nautilus pompilius* itself. No one of these species had endured for more than a portion of the duration of the whole genus, and many species had existed contemporaneously, those species, however, except perhaps two, were now extinct, so that now they were brought face to face with the heart of the question: by what hypothesis could they account for those phenomena? They were driven into hypothesis of some kind or other, because it was impossible to have any evidence of contemporary witnesses of facts which went so far back into the past. So far as he knew there were only two possible alternative hypotheses by which they could pretend to account for those facts. One of these hypotheses was what he ventured to call the hypothesis of construction. That hypothesis was that every one of those species was put together. It was making a needless difficulty to suppose that each species came out of nothing, because they knew that the body of the nautilus was made up of materials which were familiar to them in an inorganic state on the earth's surface; so that by the hypothesis of construction some agency had put together those materials a hundred times or so during the period that had elapsed from the formation of the Silurian rocks to the present day, as an artist constructed his work, or as a mechanician put together the parts of his machine. That was one hypothesis. For his part, he had not a word to say *a priori* against the possibility of that hypothesis. It was certainly conceivable and therefore, according to Hume's maxim, it was possible. But they must bring it, like all other hypotheses, to the test of facts and inquire how far it stood that test. He thought the hypothesis of construction presented two large and almost insuperable difficulties. The one was that it was absolutely opposed to everything that they had received traditionally concerning the origin of animal forms, and the second was that it was no less opposed to every doctrine which might reasonably be held upon grounds of sane science. It stood to reason and common sense that they could have recourse only to those causes for the assumption of which there was some ground of analogy. The business of science would be extremely easy if for every event one were permitted to invent special causes having no analogy in nature. The difficulty of science was in tracing every event to those causes which were in present operation. That difficulty was being so constantly overcome that it had become a canon of

physical science no less than it was a canon of historical science that speculation should confine itself to construing past events by the analogy of those of the present time. The hypothesis of construction seemed to him unacceptable, because it led them into contravention of tradition on the one side and into contravention of scientific logic on the other. The only other alternative hypothesis was that of evolution, which meant that the different forms of animal life had not arisen independently of each other in the great sweep of past time, but that the one had proceeded from the other; and that that which had happened in the course of past ages had been analogous to that which took place daily and hourly in the case of the individual. That was to say that just as at the present day in the course of individual development the lower and simple forms, in virtue of the properties which were inherent in them, passed step by step by the establishment of small successive differences into the higher and more complicated forms, so, in the case of past ages, that which constituted the stock of the whole ancestry had advanced grade by grade and step by step until it had attained the degree of complexity which was seen at the present day. No objection could be brought against this hypothesis on the ground of analogy, because in putting it forward they were not bringing in any kind of causation which was not abundantly operative at the present time. The question was whether the history of the globe in past time coincided with this hypothesis, and to that point he would next address himself. What did they find if they considered the whole series of these forms? Unquestionably, as he had said, nautili were found as far back as the Upper Silurian age. Before that time there were no nautili, but there were shells of the *Orthoceratidæ*—of which there were magnificent examples before him—which resembled those of the nautili in that they were chambered, siphoned, &c., with the last chamber of such a size that it obviously sheltered the body of the animal. He thought no one could doubt that the creatures which fabricated these still earlier shells were substantially similar to the nautili, although their shells were straight, just as a nautilus shell would be if it were pulled out from a helix into a cone. Then came the forms known as the *cyrtoceras*, which were slightly curved. Along with these they had the other forms which were on the table, and in which the shell began to grow spiral. The next that came were forms of nautilus, which differed from the nautilus of to-day in that the *septa* were like watch-glasses, and that the whorls did not overlap one another. In the next series, belonging to the later palæozoic strata, the shell was closely coiled and the *septa* began to be a little wavy, and the whorls began to overlap one another. And this process was continued in later forms, down to that of the present day. Looking broadly at the main changes which the nautilus stock underwent, changes parallel with those which were followed by the individual nautilus in the course of its development, he considered that there could be no doubt that they were justified in the hypothesis that the causes at work were the same in both cases, and that the inherent faculty, or power, or whatever else it might be called, which determined the successive changes of the nautilus after it had been hatched, had been operative throughout the whole continuous series of existence of the genus from its earliest appearances in the later Silurian rocks up to the present day. What the whole question, in whatever way it might be put, came to, was this: Successive generations of animals were so many cycles of evolution that succeeded one another. Within the historical period, there was no doubt that, speaking roughly, those succeeding cycles had been identical, that was to say, without discernible difference. But when the period of observation became proportional to the slow rate of change they found, so to speak, that the hour hand had moved; for, in the successive cycles of evolution which had occupied the whole period, successive cycles had differed from one another to a slight extent. If they might assume that, then the whole of the phenomena of palæontology would fall into order and intelligibility. If not, they had to adopt an hypothesis which, as he had pointed out, had no support in tradition, and which was absolutely contradicted by every sound canon of scientific research. This was his case for evolution, which he rested wholly upon arguments of the kind he had adduced. From the time when he first read Charles Darwin's "Origin of Species," now some twenty-four years ago, his mind had fixed itself upon the tenth chapter of that book, which treated of the succession of forms in geological times, for it appeared to him that that was the key of the position; that if the doctrine of evolution was correct, the facts of palæontology, as soon as they became sufficiently known, must bear it out and verify it in every particular. On the other hand, he believed that, if the facts of palæontology or the historical facts of life on the globe were against evolution, then all the rest of the argumentation in its favour would be vain and empty, because the difficulty of adopting it would be in that case absolutely insuperable. He would venture to repeat that the occurrence of evolution was a question of history. He did not know whether Sir Henry Maine was not more competent to speak on that point than he was. It was a question as to whether they would interpret the facts of animated nature scientifically, or whether they would open the door to every description of hypothetical vagary. He came to the conclusion that that was a point worth testing in every possible way, and for some twenty years he had given what leisure he had been able to beg, borrow, or sometimes steal, to the investigation of these questions. He had endeavoured to ascertain for himself how the doctrine of evolution fitted with the facts of palæontology, with regard to the higher vertebrated animals, and with regard to the chief varieties of invertebrate animals, and all he could tell them was that the farther his own investigations had gone, the more complete had appeared to be the coincidence between the facts of palæontology and the requirements of the doctrine of evolution. The conclusion he had come to was that at which every competent person who had undertaken a similar inquiry had arrived, and if they would pay attention to the writings of such men as Gaudry, Rütimeyer, Marsh, Cope, and others, who had added materials upon which to form a judgment such as were not dreamt of when Darwin first wrote, they would find that they all without hesitation attached themselves to the doctrine of evolution as the only key to the enigma. In deciding the issue between the two hypotheses, serious inquirers would not trouble themselves about any collateral points as to the how and the why, or as to any of the subordinate points at issue. He thought he was entitled to entreat those who by their calling or by their position in society, or by the fact that they possessed any influence, might be led to express an opinion upon this matter, to look into the arguments which formed the foundation of the case for evolution. Happily, he might address that recommendation to members of the University of Cambridge with a perfectly good conscience, for at this present time he knew not where in the world any one could find better means of passing through all those preliminary studies which were essential to a comprehension of this great question, or where any one could find more amply displayed the means of testing the arguments which he had laid before them. He ventured to say that the members of this University were without excuse if they gave opinions on this question of evolution without having prepared themselves, by as diligent study as they would for the purpose of approaching questions of literary or theological criticism, to express an opinion upon it. These were the considerations which he had wished to set before them that day. It would be understood that they would not suffice to enable any one to form a judgment upon the doctrine of evolution, but he hoped that they had sufficed, brief and insufficient as they were, to show that if judgment on this question was to be worth anything intellectually, if it was to be creditable to the moral sense of those who formed it, it would first be necessary that the facts should be clearly comprehended, and that the conclusion—whatever it might be—should be one which right reason would admit might be justly and perfectly connected with the facts.

UNIVERSITY AND EDUCATIONAL INTELLIGENCE

OXFORD.—The term that has just concluded has been chiefly noticeable for the interest drawn towards Oriental studies in the University by the building of the new Indian Institute. The visit of the Prince of Wales to the Chancellor of the University served to draw national attention to the work which Oxford, and especially Balliol College, has undertaken in respect to the training of the selected candidates for the Indian Civil Service. In spite of the failure of the late attempt to induce the University to relax its rule requiring three years' residence as a qualification for a B.A. degree in the case of the Indian Civil Servants, a considerable proportion of the selected candidates come into residence at the University; Balliol, by providing teachers and tutors in Oriental subjects, attracts by far the greatest number.

With the exception of two debates there has been little excitement during the term in the Convocation House. The two questions that roused general interest were, first, the proposal that

examiners in the Pass Schools should allow merit in one subject to compensate for a deficiency in another; and secondly, the decree to grant 10,000*l*. to Prof. Burdon Sanderson for the equipment of the new Physiological Laboratories. In the first debate the proposal was only carried by the casting vote of the Vice-Chancellor, and some doubt has since been raised on the qualification of one of the voters. In the second debate the opponents of vivisection, allied with those who oppose any large expenditure by the University on economic grounds, sought to throw out the decree and force the University to make special provision against the Professor experimenting on living animals. Prof. Burdon Sanderson in his speech disclaimed any intention of introducing vivisection into his courses or demonstrations, but declared that he would experiment on living animals in his own researches if he deemed it necessary for the discovery of truth. The decree was carried in a large house by 88 votes to 85.

The Commemoration in the Sheldonian Theatre passed off with less uproar this year. Among the recipients of honorary degrees Lord Rayleigh and Sir Frederick Abel represented Science.

In November next Balliol and Christ Church will hold examinations for electing to Natural Science Scholarships. The subjects at both colleges will be Chemistry, Physics, and Biology, with an essay and an optional paper in Mathematics.

CAMBRIDGE.—The Senior Wrangler in the Mathematical Tripos (Parts I. and II.) is Mr. Mathews, of St. John's College; Mr. Gallop is second; Mr. Lachlan, third; Messrs. Chevallier and A. N. Whitehead, bracketed fourth—all of Trinity College. One lady, Miss Perrin, of Girton, is placed between the 20th and 21st Wranglers. Three ladies are senior optimes; one is junior optime.

The Special Board for Mathematics have recommended that the Smith's Prize be awarded for the best essay on any subject in Mathematics or Natural Philosophy, to be sent in about a year and a half after the candidates are of standing for Parts I. and II. of the Mathematical Tripos; and that the adjudicators be the Vice-Chancellor, the Mathematical Professors, and the Cavendish Professor.

Prof. Foster has given notice of a revision class in Physiology during the Long Vacation, to be held at the Physiological Laboratory.

The proposed regulations for the Balfour Fund have been formally adopted by the senate.

The annual report of the Observatory states that, owing to the great progress made with the zone observations it has been possible to give much attention to the comets of recent years, and important contributions to the computations of their orbits have been made by the Observatory.

The General Board of Studies have published their recommendations with regard to new teachers, buildings, and appliances, and it is at once evident that several times the whole amount of the new income of the University could readily be spent in supplying the distinct wants of the several departments. They confine their recommendations as to Readerships within narrow limits owing to the extreme pressure upon University Funds, but they recommend the appointment of the present Readers in Indian Law, Classical Archæology, and Talmudic Literature as University Readers at 300*l*., not 400*l*. as proposed by the Statutes, a Reader in Comparative Philology at 300*l*. and a Reader in Botany at 100*l*. a year, tenable with a College Lectureship. As to University Lecturers, that is College Lecturers who throw open some of their lectures to the University and give advanced lectures, they recommend, as regards Medicine, four University Lecturers; Mathematics, five; Biology—one in Botany, one in Zoology; three in Physiology; two at least in Geology; and others in other departments.

More Demonstrators, the senior to be better paid than at present, are further recommended to be appointed in various departments of Natural Sciences.

The appointment of a Professor of Pathology is again pressed as urgent; provision to be made for a temporary laboratory.

As to buildings, the Board have placed among urgent requirements the extension of the buildings for Physiology and Comparative Anatomy, Chemistry, Botany, Mechanism, and for Geology, to be partly supplied by the Sedgwick Memorial Fund. As extra to the latter fund, 10,000*l*. is asked.

A special grant of 500*l*. for physiological apparatus is recommended. Further, the Museum and Lecture Rooms require at least 350*l*. more annually from the new funds, in addition to 500*l*. asked for from the ordinary income of the chest.

The cost of a chemical laboratory is provisionally estimated at 15,000*l*., and the purchase of Prof. Stuart's plant, with which he has at his own risk developed the flourishing school of engineering in the University, at 2,500*l*. Then permanent buildings for the school of Mechanism would cost 3,500*l*. more.

The recommendations of the General Board, after sifting and reducing the recommendations of the Special Boards, will entail annual charges of 4,360*l*., in addition to at least 2,500*l*. required by new professorships or new elections to existing professorships.

Capital expenditure will be required for buildings 31,200*l*., and for special grants other than building, 4,810*l*.; but it is proposed to borrow for these purposes. The Board have been informed that the special sum (500*l*.) asked for physiological apparatus will be provided by the liberality of a private donor who wishes to remain anonymous.

No voting can take place on these proposals till next term.

The special reports, on which the detailed Report of the General Board of Studies is founded, contain much interesting information about the present state of Natural Science studies in the University.

The Medical Board ask for provision for teaching in Medical Jurisprudence, Sanitary Science, Mental Diseases, and Elementary Medical and Surgical Methods. The number of students at present is about 200.

The report of the Classical Board contains an elaborate account of the present provision for studying Philology, Antiquities, Ancient Art, Topography, &c. which we cannot here reproduce.

The Board of Oriental Studies ask for University Lectures in Hebrew and Sanskrit, and for a Reader in Syriac, and that the Lord Almoner's Professor of Arabic be secured, if possible, as a resident professor by the augmentation of his stipend. They also urge the importance of establishing teaching in the departments of Egyptology and Assyriology.

The Mathematical Board estimate the resident students for Mathematical Honours as between 300 and 400. There are thirty-four College Lecturers in Mathematics, much of whose time is occupied in preparing candidates for the pass examinations. It will be impossible to secure adequate teaching of the subjects of Part III. of the Mathematical Tripos unless at least University Lecturers are appointed, and the Board ask for five at 50*l*. a year, two courses of advanced lectures being required from each lecturer every year.

The Board for Physics and Chemistry in addition to the laboratory claims press for additional means of catechetical teaching in Chemistry and instruction in Technical Electricity; in Mechanism a Superintendent of the Workshops, and in Mineralogy a Curator of the Museum. The number of students in Chemistry in the University is nearly 200; in Physics (Cavendish laboratory only), average for last two years, 54; Mechanism 42; Mineralogy 9.

The Board urge the advisability of Colleges permitting their lecturers in Chemistry and Physics to lecture in the University lecture rooms and laboratories, to allow more efficient organisation of teaching, as well as economy in expenses.

Lord Rayleigh asked for 600*l*. additional for Demonstrators in Physics, but the General Board of Studies have only recommended 220*l*. to be granted.

Professor Stuart's workshop has not more than half supported itself by fees of students as yet, but by employing the workmen in the manufacture of apparatus, &c., for other University departments after their hours of teaching, he has made a profit sufficient to pay the deficit, except the cost of demonstrators. A superintendent of the workshops is absolutely necessary if the University keeps up the school of Engineering, and a demonstrator in each department. Thus the Professor would not as now, be required to teach in the workshops and act as general manager as well as demonstrator. A considerably larger foundry is required, as the department has proved most useful.

The Board for Biology and Geology recommend the appointment of a Professor of Animal Morphology, and, failing this, three University Lecturers in this subject, one of whom shall direct the laboratory.

In relation to Physiology, Dr. Foster made an elaborate report, describing the organisation of, and work done in, his laboratory. He asked for a head demonstrator and four assistant demonstrators. As to advanced lectures, mention was made of the very largely unpaid work undertaken by Dr. Gaskell (entirely

unpaid), and Messrs. Langley and Lea, and University recognition of their work was asked for. Elementary Biology and Physiology of the Senses were also mentioned as needing a special lecturer.

With regard to Botany, teaching in Vegetable Morphology and in Physiology is urgently required, with lecture rooms and laboratories.

It is further asked that University teachers be eventually appointed in Agriculture, Anthropology, Geography, Metallurgy, and Mining.

In Geology it is pointed out that since Prof. Bonney left Cambridge, no College has given any assistance towards geological teaching, and that Dr. Roberts and the other demonstrators have received no University or College payments for the continued work they have done in lecturing and demonstrating.

The average number of students at present in the various departments of Biology and Geology is—Botany, 80; Geology, 40; Zoology, 75; Physiology, 120; Human Anatomy, 100 each term.

Donald MacAlister, M.D., M.B., Fellow and Medical Lecturer of St. John's College, Cambridge, was on Thursday, June 14, elected a member of the Council of the College.

The following awards have been made at St. John's College for proficiency in Natural Science:—Foundation Scholarships to Andrews, Kerr, Phillips (R.W.); Exhibitions to Goodman (already Scholar), Cooke, (E. H.), Fenton, Jones (H. R.), Watts; a Proper Sizarship to Gepp. Goodman obtains a Wright's Prize with augmentation of emoluments to 100*l.*, and a Hughes' Prize, as one of the two most distinguished third-year students in the College. The Open Exhibition was awarded to Rogers.

Mr. J. V. Jones, Principal of Firth College, Sheffield, has been elected by the Council to be the first Principal of the University College for South Wales and Monmouthshire.

SOCIETIES AND ACADEMIES

London

Royal Society, May 24.—"The Effects of Temperature on the Electromotive Force and Resistance of Batteries, II." By William Henry Preece, F.R.S.

In the discussion on the previous paper read on February 22, 1883, it was suggested that observations should be made on the influence of temperature to the case of secondary batteries. One of Mr. Tribe's cells was used.

The negative element of this cell consisted of pure peroxide of lead in the form of a plate 4 inches square carried in a grooved frame, from one end of which projected the necessary conductor. This element was placed between two plates of finely divided lead likewise 4 inches square. These were joined together, and formed the positive element of the cell. Each half of the positive plate was about a quarter of an inch distant from the negative, and all three plates were incased in a thin specially prepared fabric. The elements were contained in a leaden case, and the liquid was sulphuric acid of the strengths given in the various experiments. This cell was placed inside the cylindrical copper vessel used in the previous experiments, and precisely the same method of observation was adopted. The influence of heat on secondary cells was the same in kind as in the Daniell cell, but it differs very much in degree. The electromotive force practically remains constant for all degrees of temperature, but the internal resistance diminishes as the temperature increases at a very steady rate, increasing again as the temperature is lowered. The effect of varying the percentage of acid in solution is not very marked, though as might have been anticipated from Kohlrausch's observations, the 30 per cent. proportion gives the lowest resistance. The mean average reduction in resistance between 0° and 100° C., is 59·6 per cent.

Chemical Society, June 7.—Dr. Perkin, president, in the chair.—The following papers were read:—Laboratory notes by J. H. Gladstone and A. Tribe: (1) On the action of light and heat on cane and invert sugars; cane sugar solution, when heated, forms a small quantity of a substance which is not alcohol, but which gives the iodoform reaction. (2) On hydroxylamine; the copper zinc couple reduces this substance, ammonia being formed. (3) On the recovery of iodine from organic iodide residues; the residues are poured on to an excess of the couple, and the iodide of zinc formed, extracted with hot water; iodine is obtained in the free state by the action of hydrochloric acid and bleaching powder on the iodide. (4) A residual phenomenon of the electrolysis of oil of vitriol; the formation of Berthelot's persulphuric acid was noted. (5) On an alleged test for alcohol; Davy suggests that alcohol can be detected by the blue colour produced with a warm solution of molybdic anhydrid in oil of vitriol. The authors find that other reducing substances and sugar give the same reaction. (6) Reaction of the couple on nitric oxide; ammonia is formed, but no protoxide. (7) On the reducing action of spongy lead.—Note on a basic ammonio-copper sulphate, by S. U. Pickering.—Notes on Loew and Bokorny's researches on the probable aldehydic nature of albumin, by A. B. Griffiths.—Note on the action of sulphuric acid, sp. gr. 1·84, upon potassium iodide, by H. Jackson. The author has investigated this reaction quantitatively; he finds that two reactions occur, one with an excess of sulphuric acid when iodine and sulphur dioxide are formed; the second when just sufficient sulphuric acid is used to satisfy the potassium iodide; iodine and sulphuretted hydrogen are then liberated.—The action of nitrous anhydride on glycerin, by O. Masson. The author obtained the trinitrite of glyceryl; it is an amber-coloured liquid boiling at 150°, burns with a white flame, but does not explode under the hammer. It is decomposed by water, and cannot be preserved. In sealed tubes it generates sufficient gas to shatter the glass.

Linnean Society, June 7.—Sir John Lubbock, Bart., president, in the chair.—Mr. R. J. Clarke and Mr. Frank Matthews were elected Fellows of the Society.—Mr. W. T. Thiselton Dyer exhibited a series of Copals: some from Inhambane, near Mozambique, the product of *Copaifera Gorskiana* of various sorts, with a melting point from 310° to 360° Fahr.; others from Lagos (obtained by Capt. Moloney), used by the natives for burning, and powdered by the women as a body perfume. These last are supposed to be from a species of *Daniellia*, the native name being "Ogea."—Mr. Hiern drew attention to specimens of *Quercus Ilex*, var. *Fordii*, from Barnstaple, Devon, showing remarkable alteration in the leaves after pruning. There was exhibited for Mr. Stansfield R. Rake a burdock leaf with numerous excrescences, supposed to be the result of insect irritation.—Mr. G. Murray exhibited specimens of dace killed by the fungus disease (*Saprolegnia ferax*), the result of inoculation, and said to be the first recorded experimental proof of the communicability of the disease to those fish.—Dr. Cobbold showed shrimps sent by Dr. Burge of Shanghai. They contained immature flukes, which it was thought might prove to be the larval state of one or other of the three species of human fluke known to infest man in eastern countries. He proposed to call the parasite *Cercaria Burgei*.—A paper was read by Mr. H. N. Ridley, on new and rare monocotyledonous plants from Madagascar, among which may be mentioned species of *Drimia* hitherto only known from Africa, several curious orchids, one remarkable for possessing only one or two very large, handsome green, white, and purple flowers. Of Cyperaceæ one form well known as an Indian plant, another of the genus *Fintelmannia*, supposed to be confined to Brazil; he also describes a new genus, *Acriulus*, allied in some respects to *Cryptangium*.—A communication was read from Mr. George Lewis, on Japan Brenthidæ and notes of their habits. These beetles form part of the collection made by the author in his visit to Japan during the summers of 1880-81. He observes that there is no geographical barrier sufficient to exclude tropical forms from Japan, but their environment, when they reach it, prevents them from establishing themselves, to any great extent at least, in the northern parts. In the southern islands of the Japanese Archipelago the warmer climate enables a fair number of beetles of a truly tropical type to exist. The fact that each genus is only represented by one species nevertheless points to some physical check in their spread and numbers. A new genus, *Higonius*, is characterised, and several species of this and other genera described and illustrated.—Mr. T. H. Corry read a paper on the fertilisation of the Asclepiads, chiefly bearing out views noticed on a former occasion.—A short record of observations on the White Ants (Termites) of Rangoon, by Dr. Robert Romanis, was read by the Secretary. He details what he saw in what may be termed the swarming of a nest.

Edinburgh

Royal Society, June 4.—Mr. Thomas Gray, vice-president, in the chair.—Mr. Buchan read a second paper on the oscilla-

tions of the barometer, the conclusions of which were based largely on the *Challenger* observations. It appeared that the greatest diurnal oscillation occurred in regions over the sea where the air was very moist, being least indeed in those oceanic regions north and south of the equator where the average height of the barometer was greatest; whereas over land the contrary was the case, the greatest oscillations occurring where the air was driest. The explanation given was that over the ocean, whose surface changes very slightly in temperature throughout a whole day, the main effect results from the direct heating of the air and its contained moisture; while over the land the effect due to surface changes preponderates, being less, of course, the better the air acts as a screen to the solar rays, that is, the moister it is. Mr. Buchan then proceeded to account for the double maximum in the diurnal oscillations of the barometer. Beginning at six o'clock in the morning, an hour at which in general the barometer shows its daily mean, we find that the first effect of the sun is to heat the air, which tends to expand and rise. This tendency is of course somewhat resisted, so that the pressure is in the first instance increased; but by and by this resistance is overcome, the air flows freely upwards, the morning maximum is reached, and the pressure begins to fall. After noon this diminution in pressure is accelerated by the cooling of the air, for the same reason that the first effect of heating is to increase the pressure. Hence the barometer falls to its afternoon minimum. But as this is going on the region to the west is in its turn being heated, and an eastward movement of air overhead takes place towards the first locality, arresting the diminution of pressure, and then bringing it to a second maximum. This action, however, ceases as midnight comes on, the cooling of the air being then left to have its own effect, and the pressure falling to its second minimum till the approach of the sun on the east makes itself felt, and the same cycle of operations begins again. The modifications introduced by special conditions, such as the distribution of land and water, were also discussed, and explanations given of the retardation in certain places of the maxima and minima in time, and of the very slight, almost imperceptible, second minimum which in such cases frequently is found.—In presenting the last report of the Boulder Committee, Mr. Milne Home, the convener, intimated that the Committee purposed giving a general report in a form in which it could be readily compared with the British Association reports. —Mr. W. E. Hoyle read a paper on a new Entozoon from the mesentery of *Proteles cristatus* (Spairman). It is closely allied to *Pentastomum Diesingii* described by Van Beneden, belonging indeed to the same genus, but distinguished by its size, the number of its segments, and a slight difference in shape. The most curious point in its anatomy is that when the animal is encysted in the mesentery of its host the cirrus-sac is empty, and there is a stoppage in the vas deferens. The name proposed for the parasite is *P. Protelis*.

Mathematical Society, June 8.—Mr. J. S. Mackay, F.R.S.E., president, in the chair.—Mr. Thomas Muir, F.R.S.E., communicated some mathematical notes of interest to teachers and a new proof of Prof. Tait's problem of arrangement.—Mr. A. Y. Fraser read a paper on the fundamental notions of the differential calculus; and Dr. C. G. Knott, F.R.S.E., discussed the singularities of plane curves.

Berlin

Physiological Society, May 11.—Prof. Brieger reported on the further results of his study of the violent poisons formed by decomposition of animal bodies. In continuation of the communication he made a short time ago to the Society he described the process by which he had obtained from decomposing masses of flesh a substance which crystallised in acicular forms, and which he obtained by repeated crystallisation in such a degree of purity that he was able to analyse it. It afforded the empirical composition $C_5H_{14}N_2H_2Cl_2$, and consequently was a hydrochloric salt of a new base, which did not in its constitution resemble any known combination. This diamin-base had no longer the toxic properties of the extracts of the decomposition products. It was extremely easily decomposed, and could only be prepared from decomposing meat, and could neither be obtained in the later stages of putrescence nor from decomposing fibrin or other albuminous substances. Neither could it be demonstrated to be a constituent of meat. A second substance was obtained from the mother-liquor that remained after the crystallisation of the diamin-salt. This body, after purification by recrystallisation, showed the composition of $C_5H_{11}NCl$. This base proved to be a very virulent poison; 1 mg. in solution injected subcutaneously into a rabbit very soon produced the set of symptoms characteristic of fish-poison, *i.e.* salivation, quickened respiration, and diarrhœa, followed in a short time by death in convulsions. Even after the isolation of these exceedingly poisonous bases the mother-liquor contained other bases which have as yet not been more closely studied, and which belong to the group denominated by Prof. Brieger, "peptotoxine." They are more or less poisonous in their action upon the living organism, are decomposed with extraordinary facility, and are not only formed in the first stages of decomposition of masses of flesh, but are also contained in neurin in peptones.—Dr. George Hoppe-Seyler, who was present as a visitor, reported the results of the experiments that he made, starting on the basis of the chemical relation of nitrophenylpropiolic acid to indigo, which was studied by Herr Baeyer, in order to determine the physiological action of this acid, hoping thus to advance a step in the comprehension of the formation of indol and oxindol in the living organism. He found that oxindol appeared in the urine of rabbits into whose stomachs he had introduced solutions of nitrophenolpropiolate of soda, the animal in the meantime evincing no morbid symptom. When the solution was subcutaneously injected, blood appeared in the urine along with the oxindol, and when the treatment was continued for some time the constitution of the rabbits was injuriously affected so that they finally died, without, however, manifesting any characteristic symptoms. Dogs behaved quite otherwise when even a third part of that which the rabbits bore without inconvenience was introduced into their stomachs, increasing quantities of albumen and sugar appeared in their urine, and the animals succumbed to emaciation and loss of power. This very remarkable difference in the action of nitrophenylpropiolic acid on dogs and rabbits was not conditioned by the different diet, because when rabbits were driven to take to albuminous diet, by inanition or milk diet, until the reaction of the urine was acid, or when, on the other hand, the urine in dogs was made alkaline by giving them acetate of soda, the differences of the action remained unaltered, and their study promises a key to the comprehension of the origin of albuminuria and glycosuria.

CONTENTS

THURSDAY, JUNE 28, 1883

THE LINKS OF THE ANIMAL WORLD

Les Enchaînements du Monde Animal dans les Temps Géologiques. Fossiles Primaires. Par Albert Gaudry, Professeur de Paléontologie au Muséum d'Histoire Naturelle. (Paris : F. Savy, 1883.)

M. GAUDRY, a distinguished palæontologist, contributed five years ago a very interesting volume on the important and much debated question of the mammalia of the Tertiary epoch (see NATURE, vol. xviii. p. 537). The volume which he now publishes relates to the same *quæstio vexata*, but takes into consideration only the fossils that are to be found in primary strata. The author's proposed task is the same in both cases ; he undertakes to find the links and connections that may exist between the animals which have successively or simultaneously inhabited the lands and seas of past epochs.

A great deal has been written on the transformism-theory of Lamarck and Darwin, and it must be expected that much more will be written. One of the principal objections made to it is that if man is really the descendant of the ape, and the ape that of other mammalia, if, generally, there exist links between all animals, living and extinct, so that all animals trace their origin to a common ancestor, how is it that no link really exists between man and ape, or between fish and frog, or between vertebrate and invertebrate? Embryological considerations, it is said, show a real connection between very different animals : a frog for instance is a fish for some time during its youth, and amphioxus looks very much like an ascidian.

But, notwithstanding numerous arguments to support Lamarck's theory, no transformist can show any species gradually losing its peculiar characters to acquire new ones belonging to another species, and thus transforming itself. However similar the dog may be to the wolf, no one has found any dead nor living animal or skeleton which might as well be ascribed to wolf as to dog, and therefore be considered as being the link between the two. One may say exactly as much concerning the extinct species; there is no gradual and imperceptible passage from one to another. Moreover, the first animals that lived on this earth are not, by any means, those that one may consider as inferior and degraded.

M. Gaudry in the first pages of his work states very clearly that he prefers the theory according to which links do exist between the extinct animals of different groups, but he does not show that facts support it yet very strongly.

The opinion one may entertain as to this question being entirely dependent upon facts and the manner of understanding them, let us now turn over the leaves of M. Gaudry's book and see whether we can find in it some firm support to Lamarck's and Darwin's theories.

According to J. Barrande's numerations the number of the species contained in the Silurian strata, comprising the Cambrian, is the following :—

	Species.
Sponges and Protozoa	153
Corals	718
Echinodermata	588
Worms	185
Trilobites	1579
Other Crustaceans	348
Bryozoans	478
Brachiopods	1567
Lamellibranchs	1086
Heteropods and Pteropods	390
Gasteropods...	1316
Cephalopods	1622
Fishes	40
Of uncertain relations	4
	10,074

Thus, in the first of the primary strata nearly all the invertebrates are to be found—excepting insects—and the first vertebrate animals appear.

Generally speaking, and leaving *Eozoon canadense* and *Archæosphærina* out of the question, worms are the oldest fossils to be found in the Silurian strata. It is so in England (Caerfay), in Scandinavia, in France, in Bohemia (Przibram), in America (St. John). In Russia, Crustaceans (Obolus) are the first fossils found. It must be therefore acknowledged that life has not begun on earth with the lowest forms. M. Gaudry, it must also be said, does not believe that such has been the case. M. Gaudry's principal aim is to show that, for instance, all Polyzoa somewhat resemble each other, and that the species are so very similar that the links between them are evident ; but he does not pretend to show the relations between Worms and Crustaceans for instance, and try to find links between them.

M. Gaudry shows easily enough that, according to Nicholson's, Verril's, and Moseley's views, the arrangement and classification of Polyp corals, Zoantharia, Tubipora, Tabulata, and Rugosa is a very difficult thing inasmuch as by some points of their anatomy the species under consideration should come in one group, and by others come in another group. The same thing may be said of Echinoderms.

Between Starfishes and Crinoids there are many links ; others exist between Sea-urchins and Starfish, and Holothurians. As concerns Brachiopods, M. Gaudry remarks that the oldest species of animal on earth is the Lingula, so abundantly found in the Lingula Flags of the English Silurian. It exists to this day, being the best example of "fine old age" yet recorded. Links between the various genera of Brachiopods are not yet very firmly established. Mr. Davidson is opposed to the idea, but Mr. N. Glass's (of Manchester) researches give on the contrary some support to it, by showing how much the arms of the Brachiopods vary and differ in different animals of the same species. Mr. Davidson says that it is often very difficult to ascertain exactly the species of the individuals. Though not having seen, generally speaking, a great number of individuals of the same species, I may say however that of about twenty *Atrypa reticularis* often seen and handled by myself in one of the Parisian geological collections, not two were strictly alike. I have no doubt that the same is true of all, or at least most, species.

Of Mollusks and Crustaceans, M. Gaudry says what he has already said of Brachiopods and other animals. As Dr. Woodward remarks, although Trilobites are always easily distinguishable from other Crustaceans (extinct, of course), one wonders at the astonishing variety afforded by this group of animals.

Let us pass over the Mollusks to see what M. Gaudry says of fishes and other Vertebrates.

Fishes begin in the Upper Silurian strata; they are abundant enough in the Ludlow beds (Pteraspis), and in the Downton grit (bone-bed—Pteraspis, Thelodus, Plectrodus, and Ctenacanthus). The fishes of the first ages of our globe were very singular animals; for instance, *Didymaspis grindrodi* wore on the back a *scutum* very much like that of a queer little Crustacean—at present very abundant in some places around Paris—named *Apus productus*. Pterichthys wore a yet stranger *scutum* which has been ascribed to insects, to Crustaceans, and to turtles before one could understand its meaning; it was incased in a bony helmet, and its fore-fins were also incased in a similar envelope, somewhat like the limbs of a crayfish or a lobster. Those primary fishes were sometimes devoid of a vertebral column, and nothing similar to these animals can be found among the living species. It must be therefore conceded that some links are missing, or that they have not existed.

As to Reptiles, they begin after fishes, in the Carboniferous and Permian strata, at the same time that Batrachia appear. The first of these, *Protriton petrolei*, has been discovered by M. Gaudry in the Permian strata of Autun, in France. *Pleuronoura pellatii*, *Branchiosaurus*, *Apateon*, and many others resemble somewhat the Protriton, and M. Gaudry remarks that these little animals are generally abundant in the same strata where Labyrinthodonts and similar animals are to be found. It may be that some of them are young Labyrinthodonts. Among Reptilia M. Gaudry seems inclined to consider Archegosaurus and Actinodon as the primitive type. They had no real vertebral column, the brain was imperfectly developed, and the limbs were rather imperfect. It is easy to perceive, by careful study of other Reptilia, that they differ from these only very slightly in some cases.

M. Gaudry comes to the following general conclusions:—

There are certainly links between the Silurian, Devonian, Carboniferous, and Permian species, and links exist between these and the actually living species of the same groups. Primary Foraminifera, for instance, are very similar to the actual species of our seas and oceans. This is true also of Brachiopods, Polyps, Mollusks, and Trilobites, but less so of Echinodermata. Brachiopods perhaps llustrate this general theory best, since they are, of all animal groups, the only one that has lasted from the beginning of animal life (Lower Silurian) to the present day.

As d'Omalius d'Halloy says, "It is scarcely credible that the Almighty Being whom I consider as the Author of Nature has, at different times, killed all living animals, to give himself the pleasure of creating new animals, which, very similar to the preceding ones, present successive differences, and display a marked tendency to blend with the actually living forms."

HENRY DE VARIGNY

COLIN CLOUT'S CALENDAR

Colin Clout's Calendar; The Record of a Summer, April-October. By Grant Allen. (London: Chatto and Windus, 1883.)

OF all the writers in this country who seek to render the facts and the theories of modern science attractive to the general public, Mr. Grant Allen is in our opinion among the most successful. We know that he does not profess to be in any serious manner an original investigator of these facts, and we are far from being always ready to accept his theories; but in most of his writings we meet with a characteristic ingenuity of thought, and perhaps a still more characteristic grace of style, which together render his essays the most entertaining in the kind of literature to which they belong.

It has recently been said in these columns, with express reference to Mr. Allen, that this kind of literature does more harm than good to the cause of science and to the advancement of the theory of evolution. But here, we think, the most that can be fairly said is that his zeal may sometimes be in danger of outrunning his discretion, so inducing him to trespass upon the domain of scientific questions which a more technical biologist would feel to be precarious ground. We should remember, however, that the function of a popular writer is to make his material attractive to the general reader, and if he succeeds in doing this for science, we think that he deserves to be encouraged by scientific men, even if they find that in running somewhat too fast over the grounds of theory he occasionally trips over matters of fact. Now, as we have said, Mr. Allen, considered as a literary man, is certainly a man of unusual ability, and he devotes his ability to diffusing an interest in biology among readers of periodical literature, who certainly could not be reached by any less attractive means. Moreover, he is a man of originality, both as regards thinking and observing, and if he were to devote less time to spreading out the sweets of science for popular consumption, there can be little doubt that he might do good work in collecting them.

But, be this as it may, we think that there should be no difference of opinion touching the service which Mr. Allen has rendered in his own province, even if we do not all go so far as to say with Mr. Wallace that he "certainly stands at the head of living writers as a popular exponent of the evolution theory." The book which we have now to notice is restricted to this province, and in its main features resembles those previous volumes which from time to time have been favourably reviewed in these pages. It consists of thirty-nine short papers republished from the *St. James's Gazette*, the greater number of which are devoted to botanical subjects. As the title of the collection suggests, these papers embody a number of observations and reflections on the natural history of plants and animals commonly met with in English country life; and as the essays are written in the least technical and most graphic language, they might be read with profit by all who take any intelligent interest in these things.

We may now give a few quotations, which will serve to show the general nature of the book:—

"But what is most interesting of all about the butterwort is the fact that it is peculiarly adapted for attracting

insects from two distinct points of view—for food, and as fertilisers. While it lays itself out to catch and eat miscellaneous small flies with its gummy leaves, it also lays itself out to allure bees with its comparatively large and handsome mask-shaped flowers. . . . Why should these totally distinct plants [butterwort and sundew], living in precisely similar circumstances, have acquired this curious and uncanny habit of catching and devouring live flies? Clearly, there must be some good reason for the practice: the more so as all other insect-eating plants —Venus's fly-traps, side-saddle flowers, pitcher-plants, bladderworts, and so forth—are invariably denizens of damp watery places, rooting as a rule in moist moss or decaying loose vegetation. Now, in such situations it is difficult or impossible for them to obtain those materials from the soil which are usually supplied by constant relays of animal manure; and under such circumstances, where the roots have no access to decaying animal matter, those plants would flourish best which most utilised every scrap of such matter that happened to fall upon their open leaves."

"The bird which came northward at the close of the glacial period, to inhabit the now thawed plains of northern Europe, much as the American partridge might take possession of Greenland if all its glaciers were to clear away in a more genial era, was doubtless a more or less southern and temperate type of grouse-kind. Coming into Britain, it would soon be entirely isolated from all its allies elsewhere; for it is of course a poor flyer for distance, and it inhabits only the northerly or westerly parts of our island which lie furthest from the Continent, separated from Holland and Scandinavia by a wide sea. Here it could not fail to be subjected to special conditions, differing greatly from those of the European mainland, partly in the equable insular climate, partly in the nature of the vegetation, and partly in the absence of many mammalian foes or competitors. These conditions would be likely first to affect the colouring and marking of the feathers, the spots on the bill, the naked scarlet patch about the eye, and so forth: for we know that even freer-flying birds in the south, which cross often with Continental varieties, tend slightly to vary in such ornamental points; and a very isolated group like the red grouse would be far more likely to vary in similar directions. Meanwhile, the main branch of the family, separated on the great continents from this slightly divergent group, would probably acquire the habit of changing its plumage in winter among the snows of the north, by stress of natural selection, just as the Arctic fox and so many other northern animals have done; for in a uniform white surface any variation of colour is far more certain to be spotted and cut off than in a many-coloured and diversified environment. Thus it would seem probable that the Scotch grouse has slowly become accommodated to the heather, among which it is so hard to discover; while the willow-grouse has grown to resemble the snow in winter, and the barer grounds of its northern feeding-places in the short Scandinavian and Icelandic summer.

"If this be so, we must regard both birds as slightly divergent descendants of a common ancestor, from which, however, our grouse has varied less than its Continental congener. Of course, it is just possible that the common ancestor had already acquired the habit of changing its coat in winter before the divergence took place; and if so, then it is the Scotch grouse which has altered most: but this is less probable, because the usefulness of the change would certainly be felt even in a Scotch winter, and the white suit is not, therefore, likely ever to have been lost when once acquired. Though the winter is not severe enough in Scotland to make such a change of coat inevitable where it does not already exist, it is yet quite severe enough to preserve the habit in animals which have once acquired it, as we see in the case of the varying hare, a creature which in colder ages spread over the whole of northern Europe, and which still holds its own among the chillier portions of the Scotch Highlands. Hence we may reasonably infer that if our grouse had ever possessed a winter coat it would have always retained it for an alternative dress, as the ptarmigan still does in the selfsame latitudes. Accordingly, analogy seems to point to the conclusion that the Scotch grouse is a truly native breed, slightly altered by the conditions of its insular habitat from a closely allied Continental species, whose representatives elsewhere have now all assumed the guise of Scandinavian willow-grouse. In other words, the two isolated groups into which the species has split up have altered each in its own way, but the Continental variety has moved faster away from the primitive type than its British congener."

But in thus recommending Mr. Allen's latest work, we do not wish to appear unduly tolerant of inaccuracy. All we should wish to say is that, assuming Mr. Allen or any other expositor of science to be an amateur not thoroughly versed in technical matters, and therefore liable to fall into technical errors, we do not feel on this account that he need be precluded from publishing his observations and his theories for whatever they may be worth. Sooner or later these are sure to be duly winnowed, and even though they may contain more chaff than Mr. Allen has been in the habit of presenting, they may also contain some seeds of germinative value.

GEORGE J. ROMANES

AGRICULTURE IN INDIA

Field and Garden Crops of the North-Western Provinces and Oudh. Part I. With illustrations. By J. F. Duthie, B.A., Superintendent of the Saharanpur Botanical Gardens, and J. B. Fuller, Assistant Director of Agriculture and Commerce, North-West Provinces and Oudh. (Printed at the Thomason Civil Engineering College Press, 1882.)

THIS *brochure* is the first of a short series in which it is proposed to describe the cultivated products of the North-West Provinces of India. With the exception of an introduction of considerable length, treating generally of the physical, social, and agricultural peculiarities of the North-Western Provinces, the volume is chiefly devoted to a description of farm crops. Many of these, such as wheat, barley, oats, maize, hemp, tobacco, millet, and poppy, are as familiar to European cultivators as to Asiatics. Others, such as opium, rice, sugar-cane, and cotton, betoken the tropical nature of at least a portion of the season. The botanical descriptions of the various crops are contributed by Mr. Duthie in the usual language of the text-books, affording little room for original remark of any kind. By far the greater portion of the work has been compiled from the reports of Settlement Officers and other Government records, or contributed by Mr. Fuller. The agricultural information is of a highly interesting character, and the illustrations are particularly excellent. The work is, however, in a manner disfigured and rendered obscure by the peculiar views of the authors as to the first rule of arithmetic. Sixty-seven millions, &c., are expressed as 6,79,06,496, and six millions, &c., as 64,96,567. Ten millions, &c., are written in figures as 1,09,57,837. This principle of notation renders the statistical portion of the work difficult to follow, and it is not easy to see why it has been adopted.

The text is not free from remarks betokening a want of knowledge as to the progress of research on certain points. When, for example, treating of the enemies which affect the wheat crop, the author (presumably Mr. Fuller) writes as follows:—"But by far the most extraordinary disease to which wheat is liable is *schwan*, in which the young wheat-grains are found to be filled with minute worms . . . The most extraordinary fact connected with this disease is, however, that the worms can retain their vitality for a long time," &c. A footnote is then added as follows:—"Since the above was written, the worms have been identified as belonging to the order *Nematoidea*, and are apparently of the genus *Tylenchus*"! This is really too gross and wilful ignorance. The well-known and often-described "pepper brand" or "ear-cockle" attributable to the *Vibrio tritici*, now known as the *Tylenchus tritici*, is paraded as a "most extraordinary" disease, the precise nature of which has been ascertained "since the above was written." If such is the fact, the figures 1882 should be withdrawn from the title-page, and 1828 be substituted in their place. Neither do the authors appear to be at home in treating of the varieties of the cultivated plants. The varieties of rice, we are told, are more numerous and more strongly marked than those of any other crop. Forty-seven distinct varieties are announced, in support of this statement, as existing in Bareilly, although the writer proceeds somewhat naïvely to add, "Probably in the Provinces their number considerably exceeds 100." Now, as 300 varieties of wheat have been propagated by one naturalist, the forty-seven varieties of rice do not strike us as bearing out the statement as to the extraordinary variability of the plant.

Another point we cannot forbear to notice is the evident carelessness on the part of the authors as to whether their work should be understood by Englishmen in England. What is a "lakh," a "maund," or a "seer"? In vain we look for an English equivalent, and yet it is "maunds" *per acre* which are constantly spoken of, and which we long to translate into bushels or some intelligible unit of measure or weight. Viewing the volume as a whole, we cannot but pronounce it interesting and readable. The introduction is especially rich in information regarding the climate, irrigation, and cultivation of the North-West Provinces, a vast district comprising, in the quaint method of enumerating employed by the authors, 6,79,06,496 acres.

An alluvial soil and a climate by which the year is divided into two complete seasons certainly are conditions highly favourable to vegetation and to agriculture. In the colder season, wheat, barley, and oats are brought to perfection, while in the kharif, or hot season, rice, cotton, sugar-cane, and maize thrive. Not only do these highly-favoured provinces enjoy a temperate and a tropical climate, but each half of the year is again divided into two definite sub-seasons fitted for producing crops peculiar to it. We cannot but wonder whether the strange climatal vagaries to which the western world has latterly been exposed have disturbed the pleasant division of the year into kharif and rati in the North-West Provinces of India; but on this point our authors are silent.

JOHN WRIGHTSON

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications.*

[*The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to insure the appearance even of communications containing interesting and novel facts.*]

Auroræ of October 2 and November 17, 1882

THE article of Mr. Rand Capron in the *Philosophical Magazine* on the aurora of November 17, 1882, has proved to me that my observation of that phenomenon has not been without value, but it appears that Mr. Capron has been obliged to consult a not quite accurate abstract or translation of my description which I had inserted in the *Utrecht Journal*; it was from lack of time that I did not immediately send translations of it to foreign periodicals.

Perhaps you will still be so kind as to admit into NATURE a translation of both my articles on the auroræ of October 2 and of November 17; they were written immediately after observing the phenomenon. J. A. C. OUDEMANS

Utrecht, June 9

Translation of the Article in the "Utrecht Journal" of October 3, 1882

Yesterday evening, between seven and eight o'clock, there appeared here a brilliant aurora. Before seven there was visible a white arc running from west to east. This arc grew clearer and clearer, and rose slowly, till at 7h. 5m. (mean time) there emanated brilliant beams from its whole circumference.

At 7h. 10m. there rose also a bright column from the east-north-east, which phenomenon was repeated now and then also from the west.

In the southern part of the sky a magnificent sight was the large white spots, looking like brightly illuminated white clouds, but proving by their variable brilliancy to be no clouds, but to be connected with the auroral light. At 7h. 15m. such a large white spot was just before Aquila, *i.e.* north of the equator, but the spots showed a movement to the south, so that at 7h. 30m. the zone formed by them was some degrees south of the equator.

At 7h. 42m. there arose a large beam through the quadrangle of Ursa Major to β Ursa Minoris; this beam moved to the west, and was dissolved a few minutes afterwards.

At 7h. 50m. there appeared again a cloudy patch in Aquila.

At 7h. 54m. a brilliant shooting star descended almost vertically from the space between Lyra and the Dragon through Hercules; this observation was, however, not made with accuracy.

At 8h. the last cloudy patches in the south vanished; in the north also I saw no more upward radiating beams.

Long afterwards the auroral light remained visible in the north. After 8h. 30m. I did not longer look for the phenomenon; it seemed to me to have come to an end.

Utrecht, October 3, 1882 J. A. C. O.

Translation of the Article in the "Utrecht Journal" of November 18, 1882

Yesterday we had again a brilliant aurora, differing entirely in its details from that of October 2.

In the east as well as in the west there appeared at 6h. mean time a red gleam like that of a distant fire. Both these extended red patches were united through the north by an arc much resembling that which appeared in the aurora of October 2. The splendid vertical beams remarked on that occasion were now fewer in number and not so intense; they were of the same reddish hue as the above-mentioned patches visible in the east and in the west.

At 6h. 23m. there appeared suddenly in the east a bright featherlike[1] appearance, which in the beginning showed some resemblance to a comet; the end of it was exactly above Aldebaran. In no more than two minutes this feather had lengthened over an arc that passed above Saturn, through the quadrangle of Pegasus, and south of the Aquila stars, and as the front or western end proceeded, the eastern end followed. With the aid of a star-map we see that the arc, covered successively by this featherlike appearance, was elevated 20° above the

[1] By this word I did not mean that the borders or edges were not fairly defined.

celestial equator, and that its intersections with that equator had R.A. 110° and 290°. The horizon was intersected by this arc about 18° N. of E. and 18° S. of W., *i.e.* in two points situated nearly 90° from the magnetic meridian (Mr. R. Capron cites 20° erroneously; a repeated calculation gives me 18° 14′ N. of E).

When the arc was visible over more than 90° (which lasted no longer than a few seconds), its middle part, having a breadth of about 3°, was separated by a dark rift, 10° long and ½° broad, sharp at both ends.

At 6h. 25m the whole arc had vanished.

At 6h. 27m. a bundle of red rays appeared between Ursa Major and Ursa Minor; it extended from Polaris to γ Ursæ Minoris, *i.e.* over a width of almost 20 degrees; this bundle moved steadily to the west, just as had been the case with several vertical beams of the former aurora.

At 6h. 30m. clouds began to appear in different parts of the sky: first in Auriga, in the E.N.E.; a moment afterwards another in Aries. In the beginning it was not evident if they were clouds or auroral phenomena, but they proved soon to be clouds floating off southwards by the north wind.

The aurora had now lost much of its splendour; I noted still a few beams, viz.:—

At 6h. 32·5m. a beam about 10° east of Polaris; at 6h. 33m. the red beam, discovered at 6h. 27m., had so far shifted to the west that it went through the head of the Dragon; at 6h. 37m. there was still a faint beam extending from α Coronæ (visible in the twilight) to Vega, which stood higher and more to the south.

At 6h. 38m. there was a white band between α Aurigæ and α Persei; I was not sure if it was auroral or a cloud.

At 6h. 40m. there appeared two new very broad beams in the north-west.

Now the east becomes more and more covered; from the north to the west there extends itself a whitish glow to a height of 40 degrees; in the north-east this glow is much feebler and lower; therefore I estimate it as not higher than from 20 to 30 degrees. It flows off very indistinctly. The polar light seemed now to have passed its greatest intensity; the observations were no longer continued.

A part of the night a white auroral light remained still visible over the northern sky; at 9h. the sky was entirely overcast.

At midnight it was clear again, and the whole northern sky was always covered by an auroral light; I am told that it was still there at 5h. in the morning.

It is said to have been then in the north-west, of a purple colour, in the north-east more pale yellow. J. A. C. O.

Utrecht, November 18, 1882

Effects of Lightning

PERHAPS your readers will be interested in the following remarkable incident of the action of lightning which has just occurred at the village of Great Lumley, near Chester-le-Street, in Durham.

A severe thunderstorm passed over the valley of the Wear on Saturday, June 9. Great Lumley is situated on the top of an elevated plateau on the east side of this valley, and its houses are conspicuously exposed to the weather. At about 11 a.m., during the progress of the storm, an old stone house with steep pantiled roof, known as the "Old Hall," was struck by a thunderbolt. The house is only of moderate height, having three low stories; but it is on some of the highest ground in the village, and is also one of the loftiest houses therein. It is on the south side of the principal street forming the village, and it has two gable ends, which are, however, partially concealed at their lower portions by lower houses joining on. There is a yard in rear of the "Old Hall," separating it by a few yards from some smaller houses mostly inhabited by pitmen. The coal lies about seventeen fathoms below the surface, and there are several pits in the neighbourhood.

The principal damage done to the house is in the internal woodwork; but a hole has also been made in the tiled roof, the upper courses of a brick chimney-stack built over the centre of a brick partition wall which divides the building into two separate dwelling-houses have been thrown down, and some broken bricks and tiles were hurled as far as sixty feet distant to the southward. The building is about 220 years old, and is externally in a rather dilapidated condition. The masonry is rough sandstone rubble, the surface of which has in places been much disintegrated by age and weather. The building is Lord Durham's property, and is let in tenements. At the time of the accident the whole of its eastern dwelling-house was unfurnished and uninhabited. This was where the principal injuries were effected. The western compartment is occupied; and the inmates were knocked down by the concussion, various pieces of furniture being also damaged by the same cause; but nothing serious occurred. The building, being so old, has also experienced considerable damage externally from the tremendous concussion which appears to have accompanied the explosion of the thunderbolt. Loose tiles and stones have been dislodged; an old wooden frame in a window-opening in the loft at the east gable has been thrown to the ground below, and a considerable patch of surface masonry immediately above the window-opening has been shaken down. Falling bricks and stones have also made some holes in adjoining roofs; and the signs of injury shown by all these causes led the people in the adjacent houses to believe that these damages constituted the extent of the harm done by the explosion. I inspected the scene just three days after the event. The eastern and unused portion of the "Old Hall" had not then been examined internally, and the doors were still locked.

On visiting the interior I found that great injury had been done to a wooden rafter of the roof, close to the hole already mentioned; to a wooden upright post resting on the second floor and supporting this rafter; and to a wooden girder and joist sustaining the foot of the post. The plaster on the walls adjacent to the post and girder was also torn off; but below the first floor no certain trace of the explosion could be found. The damaged rafter is on the south side of the roof, and the nearest one on the east of the chimney. Its scantling is 6 inches broad by 3 inches deep; and at about one-third of its length from the eaves it is supported by a longitudinal purlin (on a level with the floor of the loft), and also (just inside the purlin) by the upright post already mentioned. From the level of the upper side of the purlin, for a length of about 6 feet upwards, the rafter is completely shivered, and two large pieces are torn out of it. One, the lower piece, is 3 feet 6 inches long, 3 inches wide, and about 1½ inch deep. This has been cut very neatly out of the west side of the under face of the rafter, leaving the new under face almost smooth; and the lower end of the scar thus formed is scooped out in a *dovetailed* form flush with the horizontal top of the adjacent purlin. No broken pieces corresponding to this scar could be found anywhere about the small loft; but some very small splinters of wood were discovered on the floor to the north-east of the rent rafter and in the direction of the window-opening already mentioned. This latter was quite open to the outer air when the accident happened, and immediately after it occurred numerous small splinters of wood were found in the yard to the eastward of the opening. These had evidently been blown out of it with the old window frame. Some were found against a low wall horizontally distant 63 feet from the opening. These splinters were not visible when I arrived, as they had at once been secured for firewood by the spectators; but I brought away one of the fragments found on the loft floor. Its fibres are quite disevered from each other, as if the wood had been completely *permeated* along their direction by the force of the explosion. The other scar in the rafter is cut out of the under face on the east side, partly alongside the first scar, but extending higher up and ending just under the hole in the roof tiles already mentioned. This second scar is 3 feet long by 4 inches by about 2 inches, and two pieces of wood were found lying on the loft floor about 8 feet distant to the north-east, which exactly fitted this scar. The hole through the roof is made at a point where a small iron nail fixed a lath (for the tiles) to the rafter.

The upright post underneath the rafter has a scantling of 8 inches by 3 inches; it is split and torn right up from its foot at the second floor (the next floor below the loft) to its junction with the rafter, to which it was fixed by several 2½-inch iron nails. The exact joint between the principal splits in the post and rafter seemed to be at the position of one of these nails, which was almost laid bare. I brought this nail away. Its upper two-thirds is quite rusty, whilst its lowest third (nearest the point) is clean and, in minute places, nearly bright. A heavy piece measuring 7 feet 6 inches by 6 inches by about 3 inches was torn clean off the north-west angle of this upright, and was found lying on the second floor about 3 feet off to the northward. The upright forms an angle post on the east side of a sort of dormer or porch projecting out of the south side of the roof, and formerly giving access from the outside to the second floor by a step-ladder. The latter, however, had disappeared shortly

before the accident occurred, having (as I understood) fallen down from age. The upright is split vertically in two places, one (where the piece was rent off) following exactly the line of the small iron nails which fixed the laths (for the wall plaster) to the upright, and the other about 2 inches to the east of this line. At this east split the outer portion of the upright is forced outwards about 4 inches from the centre portion, and all but separated from it. Along the line of the first split the lath nails are forced out of the upright, and the lath ends pushed outwards, and some broken off. A great patch of the plaster that covered them (the centre of it being about 4 feet above the floor) is torn off and thrown violently against the north wall of the building on the opposite side of the room 21 feet distant. The wall is dotted (high and low) with white powdery marks, and the floor at the foot of it is covered with broken and powdered plaster, as also is (more or less) the space of intervening floor. The splits in the upright unite upwards, and pass through the loft floor at a comparatively small orifice, and the piece rent out tapers considerably from the bottom upwards. The rent surface of this piece is minutely *fretted* in a curious manner. In the bottom of the rent off piece (which comprised the greater part of the sectional area of the post at this end) were three or four $2\frac{1}{2}$-inch iron nails, probably used for fixing the post to the floor. These appeared to be driven and bent into the wood; there was no sign of fusing on them, and the surface of the floor immediately underneath the bottom of the post was not in the slightest degree damaged, so far as could be detected. On each side of the slight brickwork forming the east wall of the dormer or porch, a small patch of plaster (about 6 inches square or so) was broken off close to the floor, and about 3 feet southward of the post, but no other marks attributable to the explosion (and these small patches might have been due only to the concussion) could be discovered at the second floor.

On entering the room below (on the first floor), which has no ceiling, it was found that the wooden girder ($9\frac{1}{2} \times 4\frac{1}{2}$), on which the post rested, had a splinter 16 inches long and about 1 inch square (on an average) torn horizontally off the east side of it, 3 or 4 feet to the southward of the position of the bottom of the post, and about 6 feet from the south wall. The girder extends across the room, and rests on the north and south walls. Below the splinter, on the same girder, there was a horizontal crack, extending through the breadth of the girder, and proceeding about 8 feet along it northward, but stopping short of the position of the post above. It seemed, however, *possible* that there might be yet another horizontal split in this girder *close* to the under side of the floor above, and extending right up to the position of the post. Along the west side of the girder is a short joist (7 inches by 3 inches) passing through the south wall, extending about 4 feet into the room, and fixed to the girder by three large iron trenails; and just below it there was some more slight woodwork bedded in the wall, and apparently rotten. This joist was forced out from the other about 1 inch, and had a horizontal split passing exactly along the line of the heads of the three trenails, and completely separating it into two layers. A small piece of the rotten wood underneath (about 6 inches by 2 inches by $\frac{1}{2}$ inch) was broken off, and thrown about 4 feet into the room on the floor to the north. An irregular patch of damp plaster about 4 feet by 18 inches, which had formerly concealed this rotten woodwork, was torn off, and most of it was (as in the room above) thrown hard against the opposite north wall, to which portions were still adhering. These portions are chiefly high up the wall, and near the floor above. Some larger pieces were also spread over the intervening space. The wall where this plaster was torn off was almost saturated with moisture, and the plaster round the rent piece was quite wet and discoloured.

There are no certain traces of the thunderbolt visible on the outside of the south wall, where, however, it most assuredly must have been present. The ground at the foot showed no signs of rending. There was a small lean-to outhouse nearly below, the roof of which was damaged; but I was led to understand that this had been done before; and as the place was locked, and my time was limited, I did not go inside it. At the angle made by one of the side walls of this outhouse with the main building, and not far from a point vertically below the position of the end of the girder, was a wet piece of ground habitually used for emptying slops at; and this seemed by no means an unlikely place for the thunderbolt to have originated. Here and there on the face of the dilapidated masonry some rather new looking abrasions were to be seen; but not even just abreast of the end of the girder could I detect for certain any traces of the explosion; and no metal of any kind was visible. In this connection it may be mentioned that there were no eaves-gutters, rain-water pipes, or metals of any sort on the outside of the house or on the roof.

Perhaps the most noteworthy feature of this accident was the *complete absence of any sign of burning or charring* at the rents in the girder, joist, post, and rafter. The nails struck also showed no symptoms of fusing; and, for all the *traces* that were left by the stroke, it might have been quite unaccompanied by heat. The work of the explosion seems to have taken altogether the form of mechanical violence. The wood of the post, rafter, and girder is sound, dry, old fir, and this would seem peculiarly liable to be set on fire.

The almost perpendicular bend that the course of the stroke seems to have taken from the girder to the post is also very curious. That the direction and force of the stroke was *upward* appears to me a conclusion quite irresistible. I have but little doubt in my own mind, from the traces left by the thunderbolt, that it sprang from the ground outside the building, at or near the wet south wall; passed up its outer face, entered the building through the wall at the rotten wood, and passed through or close to the joist and girder; then, attracted by the nails in the bottom of the post, it took a sudden turn upward (for there were no other marks of its course in the first floor room than those described), cleft right through the heart of the post, altered its course obliquely to gouge out the lower part of the rafter as far as the small nail, broke through the tiles, knocked off the chimney-top, and thence rushed to join the complementary force that had already started from the thundercloud to meet it. A. PARNELL

13, Windsor Terrace, Newcastle-on-Tyne, June 14

The Soaring of Birds

IN NATURE, vol. xxvii. p. 535, Lord Rayleigh gives what he suggests as a possible explanation of the soaring of "pelicans and other large birds in Assam" mentioned by Mr. S. E. Peal. My own observations correspond so exactly with the theory advanced that I venture to give them for whatever they may be worth.

I have never indeed observed the flight of pelicans, but the Indian kite, the turkey buzzard, and perhaps all vultures, have the same habit of soaring in great circles. The *sandhill crane*, as it is commonly called in the United States, a large migratory crane, possesses this characteristic in a most remarkable degree. These birds will go soaring about for hours at an immense height, never seeming to move a pinion except once in a great while to steady themselves a little. They always move in irregular circles at such times, and there is always a drifting with the wind; but at such a great distance above one it would be impossible by mere ordinary observation to detect the obliquity of the circles if it existed.

A short time since, however, I had a fine opportunity of witnessing the soaring of some kites; the advantageous circumstances being that they were not far away, and that I saw them commence when they were so low that there was little chance of being mistaken in what I saw. I was sitting before an open window one day about eleven o'clock. There was a gentle breeze blowing from the south-east at the time. Presently my attention was attracted by several kites over the village to the north-west. The motions of two in particular I followed for some time. After moving their wings to attain an elevation above the houses and trees they began soaring, and continued upward in this manner to a height of perhaps two thousand feet, apparently making no exertion with their wings excep to steady themselves a little occasionally. The method of accomplishing this was evidently to circle away to leeward in a great curve which inclined downward a little, thus acquiring considerable momentum; then turning toward the wind and adjusting the surfaces of the wings to the proper angle, they would shoot upward to a point considerably higher than the one from which the circle began. By the time the momentum was exhausted the bird was circling around again for another sweep to leeward.

There was considerable drifting with the wind, so that in attaining an elevation of some two or three thousand feet the birds had moved away nearly a quarter of a mile. Their consequent upward motion was in an irregular spiral, the highest parts of the curves being on the windward side.

Ongole, India, May 21 W. R. MANLEY

Geology of Cephalonia

IN answer to the inquiry of your correspondent in the last number of NATURE (p. 173) I beg to inform him that the shells of the Pliocene formation in the Morea have been long since investigated, as is shown by the great and well-known work of Hörnes. And Dr. Fischer has published a list of the fossil shells from the same formation at Rhodes. These subapennine beds extend over the whole of the south of Europe. For many of those species which are still living I have given the localities of the Morea and Rhodes as fossil in the *Proceedings of the Zoological Society*.

J. GWYN JEFFREYS

June 25

On the Chemical Characters of the Venom of Serpents

DR. WEIR MITCHELL calls my attention to an error in the brief notice which I wrote in NATURE recently (vol. xxviii. p. 114), on the researches into the chemical characters of snake poison conducted by him and Dr. Reichart. It is that instead of "They are unable to confirm the statement of Gautier of Paris that an alkaloid resembling a ptomaine exists in cobra poison; *or that of Prof. Wolcott Gibbs, that the poison of Crotalus yields an alkaloid*," it should be, "Prof. Wolcott Gibbs was unable to find an alkaloid."

J. FAYRER

53, Wimpole Street, W., June 26

Earthquake in South-West England

I HAVE just felt and heard the shock of an earthquake. The trembling of the earth was very great and the accompanying noise very loud, comparing it with one or two other slight shocks which I have before experienced in this district. I found the time to be 1.38 p.m. The time it lasted was several seconds. It was longer and louder than an ordinary clap of thunder when the lightning is not far off. A man reports that the slates of the cow-house were made to rattle.

As I now write (2.7 p.m.) a second shock has been felt, a little less severe. The weather is very calm, sky cloudy. This place is close to Dartmoor, on the westward side, about 500 feet above the sea-level.

W. F. COLLIER

Woodtown, Horrabridge, S. Devon, June 25

I BEG to inform you of the occurrence of two slight earthquake shocks here to-day, one shortly before 2 p.m., the other near half an hour later. The direction of progress seemed to be from north-west to south-east—that is along the line of the deep and narrow valley. The tremor was sufficient to cause jangling of glass and earthenware, and of the slates covering the house. The usual rumbling noise accompanied the shocks.

SAMUEL DREW

Penalla Terrace, Boscastle, Cornwall, June 25

ON WHALES, PAST AND PRESENT, AND THEIR PROBABLE ORIGIN[1]

FEW natural groups present so many remarkable, very obvious, and easily appreciated illustrations of several of the most important general laws which appear to have determined the structure of animal bodies, as those selected for my lecture this evening. We shall find the effects of the two opposing forces—that of heredity or conformation to ancestral characters, and that of adaptation to changed environment, whether brought about by the method of natural selection or otherwise—distinctly written in almost every part of their structure. Scarcely anywhere in the animal kingdom do we see so many cases of the persistence of rudimentary and apparently useless organs, those marvellous and suggestive phenomena which at one time seemed hopeless enigmas, causing despair to those who tried to unravel their meaning, looked upon as mere will-of-the-wisps, but now eagerly welcomed as beacons of true light, casting illuminating beams upon the dark and otherwise impenetrable paths through which the organism has travelled on its way to reach the goal of its present condition of existence.

[1] Lecture delivered at the Royal Institution on the evening of Friday, May 25, 1883, by Prof. Flower, LL.D., F.R.S., P.Z.S., &c.

It is chiefly to these rudimentary organs of the Cetacea and to what we may learn from them that I propose to call your attention. In each case the question may well be asked, granted that they are, as they appear to be, useless, or nearly so, to their present possessors, insignificant, imperfect, in fact *rudimentary*, as compared with the corresponding or homologous parts of other animals, are they survivals, remnants of a past condition, become useless owing to change of circumstances and environment, and undergoing the process of gradual degeneration, preparatory to their final removal from an organism to which they are only, in however small a degree, an incumbrance, or are they incipient structures, beginnings of what may in future become functional and important parts of the economy? These questions will call for an attempt at least at solution in each case as we proceed.

Before entering upon details, it will be necessary to give some general idea of the position, limits, and principal modifications of the group of animals from which the special illustrations will be drawn. The term "whale" is commonly but vaguely applied to all the larger and middle-sized Cetacea, and though such smaller species as the dolphins and porpoises are not usually spoken of as whales, they may to all intents and purposes of zoological science be included in the term, and will come within the range of the present subject. Taken all together the *Cetacea* constitute a perfectly distinct and natural order of mammals, characterised by their purely aquatic mode of life and external fishlike form. The body is fusiform, passing anteriorly into the head without any distinct constriction or neck, and posteriorly tapering off gradually towards the extremity of the tail, which is provided with a pair of lateral pointed expansions of skin supported by dense fibrous tissue, called "flukes," forming together a horizontally-placed, triangular propelling organ. The forelimbs are reduced to the condition of flattened ovoid paddles, incased in a continuous integument, showing no external sign of division into arm, forearm, and hand, or of separate digits, and without any trace of nails. There are no vestiges of hind-limbs visible externally. The general surface of the body is smooth and glistening, and devoid of hair. In nearly all species a compressed median dorsal fin is present. The nostrils open separately or by a single crescentic valvular aperture, not at the extremity of the snout, but near the vertex.

Animals of the order *Cetacea* abound in all known seas, and some species are inhabitants of the larger rivers of South America and Asia. Their organisation necessitates their life being passed entirely in the water, as on the land they are absolutely helpless; but they have to rise very frequently to the surface for the purpose of respiration. They are all predaceous, subsisting on living animal food of some kind. One genus alone (*Orca*) eats other warm-blooded animals, as seals and even members of its own order, both large and small. Some feed on fish, others on small floating crustacea, pteropods, and medusæ, while the staple food of many is constituted of the various species of Cephalopods, chiefly *Loligo* and other *Teuthidæ*, which must abound in some seas in vast numbers, as they form almost the entire support of some of the largest members of the order. With some exceptions the Cetacea generally are timid, inoffensive animals, active in their movements, sociable and gregarious in their habits.

Among the existing members of the order there are two very distinct types—the Toothed Whales, or *Odontoceti*, and the Baleen Whales, or *Mystacoceti*, which present throughout their organisation most markedly distinct structural characters, and have in the existing state of nature no transitional forms. The extinct *Zeuglodon*, so far as its characters are known, does not fall into either of these groups as now constituted, but is in some respects intermediate, and in others more resembles the generalised mammalian type.

The important and interesting problem of the origin of the Cetacea and their relations to other forms of life is at present involved in the greatest obscurity. They present no more signs of affinity with any of the lower classes of vertebrated animals than do many of the members of their own class. Indeed in all that essentially distinguishes a mammal from one of the oviparous vertebrates, whether in the osseous, nervous, vascular, or reproductive systems, they are as truly mammalian as any, even the highest, members of the class. Any supposed signs of inferiority are, as we shall see, simply modifications in adaptation to their peculiar mode of life. Similar modifications are met with in another quite distinct group of mammalia, the *Sirenia*, and also, though in a less complete degree, in the aquatic Carnivora or seals. But these do not indicate any community of origin between these groups and the Cetacea. In fact, in the present state of our knowledge, the Cetacea are absolutely isolated, and little satisfactory reason has ever been given for deriving them from any one of the existing divisions of the class more than from any other. The question has indeed often been mooted whether they have been derived from land mammals at all, or whether they may not be the survivors of a primitive aquatic form which was the ancestor not only of the whales, but of all the other members of the class. The materials for—I will not say solving—but for throwing some light upon this problem, must be sought for in two regions—in the structure of the existing members of the order, and in its past history, as revealed by the discovery of fossil remains In the present state of science it is chiefly on the former that we have to rely, and this therefore will first occupy our attention.

One of the most obvious external characteristics by which the mammalia are distinguished from other classes of vertebrates is the more or less complete clothing of the surface by the peculiar modification of epidermic tissue called hair. The Cetacea alone appear to be exceptions to this generalisation. Their smooth, glistening exterior is, in the greater number of species, at all events in adult life, absolutely bare, though the want of a hairy covering is compensated for functionally by peculiar modifications of the structure of the skin itself, the epidermis being greatly thickened, and a remarkable layer of dense fat closely incorporated with the tissue of the derm or true skin; modifications admirably adapted for retaining the warmth of the body, without any roughness of surface which might occasion friction and so interfere with perfect facility of gliding through the water. Close examination, however, shows that the mammalian character of hairiness is not entirely wanting in the Cetacea, although it is reduced to a most rudimentary and apparently functionless condition. Scattered, small, and generally delicate hairs have been detected in many species, both of the toothed and of the whalebone whales, but never in any situation but on the face, either in a row along the upper lip, around the blowholes or on the chin, apparently representing the large, stiff "vibrissæ" or "whiskers" found in corresponding situations in many land mammals. In some cases these seem to persist throughout the life of the animal; more often they are only found in the young or even the fœtal state. In some species they have not been detected at any age.

Eschricht and Reinhardt counted in a new born Greenland Right Whale (*Balæna mysticetus*) sixty-six hairs near the extremity of the upper jaw, and about fifty on each side of the lower lip, as well as a few around the blowholes, where they have also been seen in *Megaptera longimana* and *Balænoptera rostrata*. In a large Rorqual (*Balænoptera musculus*), quite adult and sixty-seven feet in length, stranded in Pevensey Bay in 1865, there were twenty-five white, straight, stiff hairs about half an inch in length, scattered somewhat irregularly on each side of the vertical ridge in which the chin terminated, extending over a space of nine inches in height and two and a half inches in breadth. The existence of these rudimentary hairs must have some significance beyond any possible utility they may be to the animal Perhaps some better explanation may ultimately be found for them, but it must be admitted that they are extremely suggestive that we have here a case of heredity or conformation to a type of ancestor with a full hairy clothing, just on the point of yielding to complete adaptation to the conditions in which whales now dwell.

In the organs of the senses the Cetacea exhibit some remarkable adaptive modifications of structures essentially formed on the Mammalian type, and not on that characteristic of the truly aquatic Vertebrates, the fishes, which, if function were the only factor in the production of structure, they might be supposed to resemble.

The modifications of the organs of sight do not so much affect the eyeball as the accessory apparatus. To an animal whose surface is always bathed with fluid, the complex arrangement which mammals generally possess for keeping the surface of the transparent cornea moist and protected, the movable lids, the nictitating membrane, the lacrymal gland, and the arrangements for collecting and removing the superfluous tears when they have served their function cannot be needed, and hence we find these parts in a most rudimentary condition or altogether absent. In the same way the organ of hearing in its essential structure is entirely mammalian, having not only the sacculi and semicircular canals common to all but the lowest vertebrates, but the cochlea, and tympanic cavity with its ossicles and membrane, all, however, buried deep in the solid substance of the head; while the parts specially belonging to terrestrial mammals, those which collect the vibrations of the sound travelling through air, the pinna and the tube which conveys it to the sentient structures within are entirely or practically wanting. Of the pinna or external ear there is no trace. The meatus auditorius is certainly there, reduced to a minute aperture in the skin like a hole made by the prick of a pin, and leading to a tube so fine and long that it cannot be a passage for either air or water, and therefore can have no appreciable function in connection with the organ of hearing, and must be classed with the other numerous rudimentary structures that whales exhibit.

The organ of smell, when it exists, offers still more remarkable evidence of the origin of the Cetacea. In fishes this organ is specially adapted for the perception of odorous substances permeating the water; the terminations of the olfactory nerves are spread over a cavity near the front part of the nose, to which the fluid in which the animals swim has free access, although it is quite unconnected with the respiratory passages. Mammals, on the other hand, smell substances with which the atmosphere they breathe is impregnated; their olfactory nerve is distributed over the more or less complex foldings of the lining of a cavity placed in the head, in immediate relation to the passages through which air is continually driven to and fro on its way to the lungs in respiration, and therefore in a most favourable position for receiving impressions from substances floating in that air. The whalebone whales have an organ of smell exactly on the mammalian type, but in a rudimentray condition. The perception of odorous substances diffused in the air, upon which many land mammals depend so much for obtaining their food, or for protection from danger, can be of little importance to them. In the more completely modified Odontocetes the olfactory apparatus, as well as that part of the brain specially related to the function of smell is entirely wanting, but in both groups there is not the slightest trace of the specially aquatic olfactory organ of fishes. Its complete absence and the vestiges of the aërial organ of land mammals found in the Mystacocetes are the clearest possible indications of the origin of the Cetacea from air-breathing and air-smelling terrestrial

mammalia. With their adaptation to an aquatic mode of existence, organs fitted only for smelling in air became useless, and so have dwindled or completely disappeared. Time and circumstances have not permitted the acquisition of anything analogous to the special aquatic smelling apparatus of fishes, the result being that whales are practically deprived of whatever advantage this sense may be to other animals.

It is characteristic of the greater number of mammalia to have their jaws furnished with teeth having a definite structure and mode of development. In all the most typical forms these teeth are limited in number, not exceeding eleven on each side of each jaw, or forty-four in all, and are differentiated in shape in different parts of the series, being more simple in front, broader and more complex behind. Such a dentition is described as "heterodont." In most cases also there are two distinct sets of teeth during the lifetime of the animal, constituting a condition technically called "diphyodont."

All the Cetacea present some traces of teeth, which in structure and mode of development resemble those of mammals, and not those of the lower vertebrated classes, but they are always found in a more or less imperfect state. In the first place, at all events in existing species, they are never truly heterodont, all the teeth of the series resembling each other more or less or belonging to the condition called "homodont," and not obeying the usual numerical rule, often falling short of, but in many cases greatly exceeding it. The most typical Odontocetes, or toothed whales, have a large number of similar, simple, conical, recurved, pointed teeth, alike on both sides and in the upper and under jaws, admirably adapted for catching slippery, living prey, such as fish, which are swallowed whole without mastication. In one genus (*Pontoporia*) there may be as many as sixty of such teeth on each side of each jaw, making 240 in all. The more usual number is from twenty to thirty. These teeth are never changed, being "monophyodont" and they are, moreover, less firmly implanted in the jaws than in land mammals, having never more than one root, which is set in an alveolar socket which is generally wide and loosely fitting, though perfectly sufficient for the simple purpose which the teeth have to serve.

Most singular modifications of this condition of dentition are met with in different genera of toothed whales, chiefly the result of suppression, sometimes of suppression of the greater number, combined with excessive development of a single pair. In one large group, the Ziphioids, although minute rudimentary teeth are occasionally found in young individuals, and sometimes throughout life, in both jaws, in the adults the upper teeth are usually entirely absent, and those of the lower jaw reduced to two, which may be very large and projecting like tusks from the mouth, as in *Mesoplodon*, or minute and entirely concealed beneath the gums, as in *Hyperoodon*,—an animal which is for all practical purposes toothless, yet in which a pair of perfectly formed though buried teeth remain throughout life, wonderful examples of the persistence of rudimentary and to all appearance absolutely useless organs. Among the *Delphinidæ* similar cases are met with. In the genus *Grampus* the teeth are entirely absent in the upper, and few and early deciduous in the lower jaw. But the Narwhal exceeds all other Cetaceans, perhaps all other vertebrated animals, in the specialisation of its dentition. Besides some irregular rudimentary teeth found in the young state, the entire dentition is reduced to a single pair, which lie horizontally in the upper jaw, and both of which in the female remain permanently concealed within the bone, so that this sex is practically toothless, while in the male the right tooth usually remains similarly concealed and abortive, and the left is immensely developed, attaining a length equal to more than half that of the entire animal, projecting horizontally from the head in the form of a cylindrical or slightly tapering pointed tusk, with the surface marked by spiral grooves or ridges.

The meaning and utility of some of these strange modifications it is impossible, in the imperfect state of our knowledge of the habits of the Cetacea, to explain, but the fact that in almost every case a more full number of rudimentary teeth is present in early stages of existence, which either disappear, or remain as concealed and functionless organs, points to the present condition in the aberrant and specialised forms as being one derived from the more generalised type, in which the teeth were numerous and equal.

The Mystacocetes, or Whalebone Whales, are distinguished by entire absence of teeth, at all events after birth. But it is a remarkable fact, first demonstrated by Geoffrey St. Hilaire, and since amply confirmed by Cuvier, Eschricht, Julin, and others, that in the fœtal state they have numerous minute calcified teeth lying in the dental groove of both upper and lower jaws. These attain their fullest development about the middle of fœtal life, after which period they are absorbed, no trace of them remaining at the time of birth. Their structure and mode of development has been shown to be exactly that characteristic of ordinary mammalian teeth, and it has also been observed that those at the posterior part of the series are larger, and have a bilobed form of crown, while those in front are simple and conical, a fact of considerable interest in connection with speculations as to the history of the group.

It is not until after the disappearance of these teeth that the baleen, or whalebone, makes its appearance. This remarkable structure, though, as will be presently shown, only a modification of a part existing in all mammals, is, in its specially developed condition as baleen, peculiar to one group of whales. It is therefore perfectly in accord with what might have been expected, that it is comparatively late in making its appearance. Characters that are common to a large number of species appear early, those that are special to a few, at a late period; alike both in the history of the race and of the individual.

Baleen consists of a series of flattened, horny plates, several hundred in number, on each side of the palate, separated by a bare interval along the middle line. They are placed transversely to the long axis of the palate, with very short spaces between them. Each plate or blade is somewhat triangular in form, with the base attached to the palate, and the apex hanging downwards. The outer edge of the blade is hard and smooth, but the inner edge and apex fray out into long, bristly fibres, so that the roof of the whale's mouth looks as if covered with hair, as described by Aristotle. The blades are longer near the middle of the series, and gradually diminish near the front and back of the mouth. The horny plates grow from a dense fibrous and highly vascular matrix, which covers the palatal surface of the maxillæ, and which sends out lamellar processes, one of which penetrates the base of each blade. Moreover, the free edge of these processes is covered with very long vascular thread-like papillæ, one of which forms the central axis of each of the hair-like epidermic fibres of which the blade is mainly composed. A transverse section of fresh whalebone shows that it is made up of numbers of these soft vascular papillæ, circular in outline, each surrounded by concentrically arranged epidermic cells, the whole bound together by other epidermic cells, which constitute the smooth cortical (so-called "enamel") surface of the blade, and which, disintegrating at the free edge, allows the individual fibres to become loose and to assume the hair-like appearance spoken of before These fibres differ from hairs in not being formed in depressed follicles in the enderon, but rather resemble those of which the horn of the rhinoceros is composed. The blades are supported and bound together for a certain

distance from their base, by a mass of less hardened epithelium, secreted by the surface of the palatal membrane or matrix of the whalebone in the intervals of the lamellar processes. This is the "intermediate substance" of Hunter, the "gum" of the whalers.

The function of the whalebone is to strain the water from the small marine mollusks, crustaceans, or fish upon which the whales subsist. In feeding they fill the immense mouth with water containing shoals of these small creatures, and then, on their closing the jaws and raising the tongue, so as to diminish the cavity of the mouth, the water streams out through the narrow intervals between the hairy fringe of the whalebone blades, and escapes through the lips, leaving the living prey to be swallowed. Almost all the other structures to which I am specially directing your attention, are, as I have mentioned, in a more or less rudimentary state in the Cetacea; the baleen, on the other hand, is an example of an exactly contrary condition, but an equally instructive one, as illustrating the mode in which nature works in producing the infinite variety we see in animal structures. Although appearing at first sight an entirely distinct and special formation, it evidently consists of nothing more than the highly modified papillæ of the lining membrane of the mouth, with an excessive and cornified epithelial development.

The bony palate of all mammals is covered with a closely-adhering layer of fibrovascular tissue, the surface of which is protected by a coating of non-vascular epithelium, the former exactly corresponding to the derm or true skin, and the latter to the epiderm of the external surface of the body. Sometimes this membrane is perfectly smooth, but it is more often raised into ridges, which run in a direction transverse to the axis of the head, and are curved with the concavity backwards; the ridges moreover do not extend across the middle line, being interrupted by a median depression or *raphé*. Indications of these ridges are clearly seen in the human palate, but they attain their greatest development in the Ungulata. In oxen, and especially in the giraffe, they form distinct laminæ, and their free edges develope a row of papillæ, giving them a pectinated appearance. Their epithelium is thick, hard, and white, though not horny. Although the interval between the structure of the ridges in the giraffe's palate and the most rudimentary form of baleen at present known is great, there is no difficulty in seeing that the latter is essentially a modification of the former, just as the hoof of the horse, with its basis of highly developed vascular laminæ and papillæ, and the resultant complex arrangement of the epidermic cells, is a modification of the simple nail or claw of other mammals, or as the horn of the rhinoceros is only a modification of the ordinary derm and epiderm covering the animal's body differentiated by a local exuberance of growth.

(*To be continued.*)

THE PERAK TIN-MINES[1]

THIS interesting memoir, which forms part of the *Archives des Missions scientifiques et littéraires*, série iii. vol. ix., gives the result of a seven months' exploration of the Malay State of Perak, made by the author, who was sent by the French Government upon a mission of scientific inquiry into the Malay Archipelago in 1881. Perak, although an insignificant unit among even the smallest States of the world, its extreme dimensions being only 95×50 miles, or an area of less than 5000 square miles, has long been known as a tin-producing country, being mentioned in the narratives of Tavernier, and the Dutch and Portuguese navigators of the seventeenth century; but it is only since the large influx of Chinese miners, consequent upon the suppression of the Taeping rebellion, that it has become of first-rate importance.

[1] "Les Mines d'Étain de Perak." Par J. Errington de la Croix. 8vo. Paris, 1882.)

The success attained by the first-comers led to a rapid increase of the Chinese population, who arrived in such numbers as to be soon beyond the control of the feeble Malay Government, and the mining being carried on without any regulations as to boundaries, the miners became divided into two parties, who made war upon each other with varying success, the Sultan looking on impartially during the contest, but siding with the winners. The defeated party in 1872 having taken to piracy at sea, was suppressed by English gunboats, and a resident was appointed for the purpose of keeping order; but the Malays having revolted in 1875, when the resident was murdered, the country has since been placed under a British protectorate, with a native rajah, under the title of Regent. This has been attended with the happiest results, the country having made great progress during the last six years, under the vigorous and enlightened management of the resident, Hugh Low, Esq., C.M.G., and now bids fair, according to the author, to become the most considerable producer of tin in the world.

The mines worked up to the present are entirely alluvial or stream works, the watercourses being filled with sand and gravel deposits to a depth of 20 or 30 feet, resting upon a floor of pure china clay, apparently derived from the decomposition of the granitic rocks forming the numerous parallel ridges which traverse the country from north to south. The geological description is necessarily imperfect owing to the dense tropical vegetation which covers the entire county; but the author has been able to establish the presence of numerous quartz veins traversing the granites which are coarsely porphyritic in the centre and largely charged with tourmaline at the edges of the masses, in fact reproducing the phenomena observed in the north-western tin districts of Cornwall. No mines have as yet been opened in any of these veins, but the author speaks of blocks of tin ore weighing more than 1 cwt. as having been found in the immediate vicinity of the hills, which are evidently not far removed from their original position. The bulk of the production is, however, derived from smaller rounded crystalline masses and grains contained in the lower part of the alluvial gravel, the workable thickness ranging from 7 to 10 feet, and the proportion of clean ore or "black tin" from about 1 to 4½ per cent. by weight. This is remarkable for its purity, being almost entirely free from wolfram, arsenic, and other foreign substances, which are so troublesome to the Cornish tin-miner. The methods of working, mechanical preparations, and smelting of the ore are of the simplest possible kind, the work, with the exception of a few centrifugal steam-pumps, and of Chinese chain-pumps driven by water-wheels, being entirely carried out by manual labour, with furnaces and other appliances of the most primitive types. This simplicity adds considerably to the interest of the author's detailed and carefully illustrated description, which enables the reader to realise in imagination the conditions prevailing in our western districts in the days when the Phœnicians traded with the Cornish miners for tin at St. Michael's Mount. Under the new British rule, the country has made rapid progress, the output of tin having risen from 2059 tons in 1876 to 5994 tons in 1881, the whole of which is exported through Penang. As at the latter date the cost of production, including revenue charges of about 17*l*., was estimated at about 61*l*. per ton, while the local selling price was 88*l*., showing a profit of 45 per cent., the popularity of the business is sufficiently explained. It is not probable, however, that such large profits will continue to be realised after the more productive deposits have been exhausted. It does not appear from the narrative that European labour of any kind is employed, the workpeople belonging to three races, namely, Malay aborigines, Klings or coolies from Madras and the Malabar coast, and Chinese, the latter supplying the whole of the miners, smelters

and other artisans directly employed in producing the metal. The author has a very high opinion of the Chinese miners, who are described as sober, regular in work, and accustomed to cooperative enterprises, against which, however, must be set the defects of being addicted to excess in opium and gambling, besides being very quarrelsome and exceedingly superstitious. The latter failing is, however, of interest as reproducing the old European legends of guardian genii of the mine, the "Kobads" of Germany and "Knockers" of Cornwall, who require to be propitiated by sacrifices and kept in good humour by orderly behaviour on the part of the miners. Infractions of the last rules are punished by the withdrawal of the guardian gnome, who takes all the unwrought ore in the mine away with him.

The execution of the work, both as regards illustration and typography, are exceedingly good, and reflect great credit upon the French National Printing Office.

H. B.

THE SIZE OF ATOMS[1]

FOUR lines of argument founded on observation have led to the conclusion that atoms or molecules are not inconceivably, not immeasurably small. I use the words "inconceivably" and "immeasurably" advisedly. That which is measurable is not inconceivable, and therefore the two words put together constitute a tautology. We leave inconceivableness in fact to metaphysicians. Nothing that we can measure is inconceivably large or inconceivably small in physical science. It may be difficult to understand the numbers expressing the magnitude, but whether it be very large or very small there is nothing inconceivable in the nature of the thing because of its greatness or smallness, or in our views and appreciation and numerical expression of the magnitude. The general result of the four lines of reasoning to which I have referred, founded respectively on the undulatory theory of light, on the phenomena of contact electricity, on capillary attraction, and on the kinetic theory of gases, agrees in showing that the atoms or molecules of ordinary matter must be something like the 1/10,000,000, or from the 1/10,000,000 to the 1/100,000,000 of a centimetre in diameter. I speak somewhat vaguely, and I do so, not inadvertently, when I speak of atoms and molecules. I must ask the chemists to forgive me if I even abuse the words and apply a misnomer occasionally. The chemists do not know what is to be the atom; for instance, whether hydrogen gas is to consist of two pieces of matter in union constituting one molecule, and these molecules flying about; or whether single molecules each indivisible, or at all events undivided in chemical action, constitute the structure. I shall not go into any such questions at all, but merely take the broad view that matter, although we may conceive it to be infinitely divisible, is not infinitely divisible without decomposition. Just as a building of brick may be divided into parts, into a part containing 1000 bricks, and another part containing 2500 bricks, and those parts viewed largely may be said to be similar or homogeneous; but if you divide the matter of a brick building into spaces of nine inches thick, and then think of subdividing it farther, you find you have come to something which is atomic, that is, indivisible without destroying the elements of the structure. The question of the molecular structure of a building does not necessarily involve the question, Can a brick be divided into parts, and can those parts be divided into much smaller parts? and so on. It used to be a favourite subject for metaphysical argument amongst the schoolmen whether matter is infinitely divisible, or whether *space* is infinitely divisible, which some maintained, whilst others maintained only that *matter* is not infinitely divisible, and demonstrated that there is nothing inconceivable in the infinite subdivision of space. Why, even time was divided into moments (time-atoms!), and the idea of continuity of time was involved in a halo of argument, and metaphysical—I will not say absurdity—but metaphysical word-fencing, which was no doubt very amusing for want of a more instructive subject of study. There is in sober earnest this very important thing to be attended to, however, that in chronometry as in geometry, we have absolute continuity, and it is simply an inconceivable absurdity to suppose a limit to smallness whether of time or of space. But on the other hand, whether we can divide a piece of glass into pieces smaller than the 1/100,000 of a centimetre in diameter, and so on without breaking it up, and making it cease to have the properties of glass, just as a brick has not the property of a brick wall, is a very practical question, and a question which we are quite disposed to enter upon.

I wish in the beginning to beg you not to run away from the subject by thinking of the exceeding smallness of atoms. Atoms are not so exceedingly small after all. The four lines of argument I have referred to make it perfectly certain that the molecules which constitute the air we breathe are not very much smaller, if smaller at all, than 1/10,000,000 of a centimetre in diameter. I was told by a friend just five minutes ago that if I give you results in centimetres you will not understand me. I do not admit this calumny on the Royal Institution of Great Britain; no doubt many of you as Englishmen are more familiar with the unhappy British inch; but you all surely understand the centimetre, at all events it was taught till a few years ago in the primary national schools. Look at that diagram (Fig. 1), as I want you all to understand an

One centimetre. One millimetre.

Fig. 1.

inch, a centimetre, a millimetre, the 1/10 of a millimetre, and the 1/100 of a millimetre, the 1/1,000 of a millimetre, and the 1/1,000,000 of a millimetre. The diagram on the wall represents the metre; below that the yard; next the decimetre, and a circle of a decimetre diameter, the centimetre and a circle of a centimetre, and the millimetre, which is 1/10 of a centimetre, or in round numbers 1/40 of an inch. We will adhere however to one simple system, for it is only because we are in England that the yard and inch are put before you at all, among the metres and centimetres. You see on the diagram then the metre, the centimetre, the millimetre, with circles of the same diameter. Somebody tells me the millimetre is not there; I cannot see it, but it certainly is there, and a circle whose diameter is a millimetre, both accurately painted in black. I say there is a millimetre and you cannot see it. And now imagine *there* is 1/10 of a millimetre, and *there* 1/100 of a millimetre and 1/1000 of a millimetre, and *there* is a round atom of oxygen 1/1,000,000 of a millimetre in diameter. You see them all.

Now we must have a practical means of measuring, and optics supply us with it for thousandths of a millimetre. One of our temporary standards of measurement shall be the wave-length of light; but the wave-length is a very indefinite measurement, because there are wave-lengths for different colours of light, visible and invisible, in the ratio of 1 to 16. We have, as it were—borrowing an analogy from sound—four octaves of light that we know of. How far the range in reality extends above and below the range hitherto measured, we cannot even guess in the present state of science. The table before you (Table I.) gives you an idea of magnitudes of length,

[1] A lecture delivered by Sir William Thomson at the Royal Institution, on Friday, February 2. Revised by the Author.

TABLE I.—*Data for Visible Light.*

Line of Spectrum.		Wave-length in Centimetres.		Wave Frequency, or number of periods per second.
A	...	$7 \cdot 604 \times 10^{-5}$	...	$395 \cdot 0 \times 10^{12}$
B	...	6·867 ,,	...	437·3 ,,
C	...	6·562 ,,	...	457·7 ,,
D_1	...	5·895 ,,	...	509·7 ,,
D_2	...	5·889 ,,		
E	...	5·269 ,,	...	570·0 ,,
b	...	5·183 ,,	...	
F	...	4·861 ,,	...	617·9 ,,
G	...	4·307 ,,	...	697·3 ,,
H_1	...	3·968 ,,	...	756·9 ,,
H_2	...	3·933 ,,	...	763·6 ,,

and again of small intervals of time. In the column on the left you have the wave-length of light in fractions of a centimetre; the unit in which these numbers to the left is measured is the 1/100,000 (or 10^{-5}) of a centimetre. We have then, of visible light, wave-lengths from $7\frac{1}{2}$ to 4 nearly, or 3·9. You may say then roundly, that for the wave-lengths of visible light, which alone is what is represented on that table, we have wave-lengths of from 4 to 8 on our scale of 1/100,000 of a centimetre. The 8 is invisible radiation a little below the red end of the spectrum. The lowest, marked by Fraunhofer with the letter *A*, has for wave-length $7\frac{1}{2}$/100,000 of a centimetre. On the model before you I will now show you what is meant by a "wave-length;" it is not length along the crest, such as we sometimes see well marked in a breaking wave of the sea, on a long straight beach; it is distance from crest to crest of the waves. [This was illustrated by a large number of horizontal rods of wood connected together and suspended bifilarly by two threads in the centre hanging from the ceiling;[1] on moving the lowermost rod, a wave was propagated up the series.] Imagine the ends of those rods to represent particles. The rods themselves let us suppose to be invisible, and merely their ends visible, to represent the particles acting upon one another mutually with elastic force, as if of india-rubber bands, or steel spiral springs, or jelly, or elastic material of some kind. They do act on one another in this model through the central mounting. Here again is another model illustrating waves (Fig. 2).[2] The white circles on the wooden rods represent pieces of matter—I will not say molecules at present, though we shall deal with them as molecules afterwards. Light consists of vibrations transverse to the line of propagation, just as in the models before you.

[1] The details of this bifilar suspension need not be minutely described, as the new form, with a single steel pianoforte wire to give the required mutual forces, described below and represented in Fig. 2, is better and more easily made.

[2] This apparatus, which is represented in the woodcut, Fig. 2, is of the following dimensions and description. The series of equal and similar bars (B) of which the ends represent molecules of the medium, and the pendulum bar (P), which performs the part of exciter of vibrations, or of kinetic store of vibrational energy, are pieces of wood each 50 centimetres long, 3 centimetres broad, and 1·5 centimetres thick. The suspending wire is steel pianoforte wire No. 22 B W.G. (·07 of a cm. diameter), and the bars are secured to it in the following manner. Three brass pins of about ·4 of a centimetre diameter are fitted loosely in each bar in the position as indicated; *i.e.* forming the corners of an isosceles triangular figure, with its base parallel to the line of the suspending wire, and about 1 mm. to one side of it. The suspending wire, which is laid in grooves cut in the pins, is passed under the upper pin, outside the pin at the apex of the triangle, over the upper side of the lower pin, and thence down to the next bar. The upper end of this wire is secured by being taken through a hole in the supporting beam and several turns of it put round a pin placed on one side of the hole, as indicated in the diagram. To each end of the pendulum bar is made fast a steel spiral spring as shown; the upper ends of these springs being secured to short cords which pass up through holes in the supporting beam, and are fastened by two or three turns taken round the pins. These steel springs serve as potential stores of vibrational energy alternating in each vibration with the kinetic store constituted by the pendulum bar. The ends of the vibrating bars (B) are loaded with masses of lead attached to them. The much larger masses of lead seen on the pendulum bar, which are adjustable to different positions on the bar, are, in the diagram, shown at the smallest distance apart. The lowermost bar carries two vanes of tin projecting downwards, which dip into viscous liquid (treacle diluted with water) contained in the vessel (*c*). A heavy weight resting on the bottom of this vessel, and connected to the lower end of the suspending wire by a stretched indiarubber band, serves to keep the lower end of the apparatus in position. The period of vibration of the pendulum bar is adjustable to any desired magnitude by shifting in or out the attached weights, or by tightening or relaxing the cords which pull the upper ends of the spiral springs.

Now in that beautiful experiment well known as Newton's rings we have at once a measure of length in

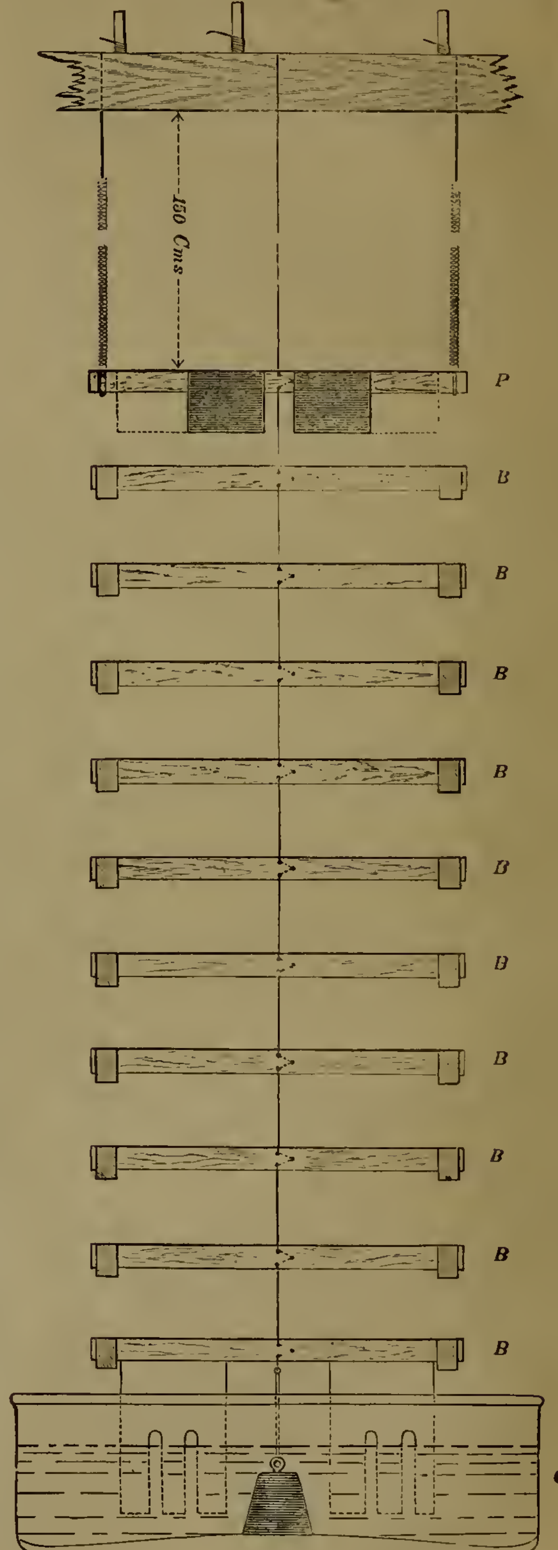


FIG. 2.

the distance between two pieces of glass to give any par-

ticular tint of colour. The wave-length you see, in the distance from crest to crest of the waves travelling up the long model when I commence giving a simple harmonic oscillation to the lowest bar. I have here a convex lens of very long focus, and a piece of plate glass with its back blackened. When I press the piece of glass against the glass blackened behind, I see coloured rings; the phenomenon will be shown to you on the screen by means of the electric light reflected from the space of air between the two pieces of glass. This phenomenon was first observed by Sir Isaac Newton, and was first explained by the undulatory theory of light. [Newton's rings are now shown on the screen before you by reflected electric light.] If I press the glasses together, you see a dark spot in the centre; the rings appear round it, and there is a dark centre with irregularities. Pressure is required to produce that spot. Why? The answer generally given is, because glass repels glass at a distance of two or three wave-lengths of light; say at a distance of 1/5,000 of a centimetre. I do not believe that for a moment. The seeming repulsion comes from shreds or particles of dust between them. The black spot in the centre is a place where the distance between them is less than a quarter of a wave-length. Now the wave-length for yellow light is about 1/17,000 of a centimetre. The quarter of 1/17,000 is about 1/70,000. The place where you see the middle of that black circle corresponds to air at a distance of less than 1/70,000 of a centimetre. Passing from this black spot to the first ring of maximum light, add half a wave-length to the distance, and we can tell what the distance between the two pieces of glass is at this place; add another half wave-length, and we come to the next maximum of light again; but the colour prevents us speaking very definitely because we have a number of different wave-lengths concerned. I will simplify that by reducing it all to one colour, red, by interposing a red glass. You have now one colour, but much less light altogether, because this glass only lets through homogeneous red light, or not much besides. Now look at what you see on the screen, and you have unmistakable evidence of fulcrums of dust between the glass surfaces. When I put on the screw, I whiten the central black spot by causing the elastic glass to pivot, as it were, round the innumerable little fulcrums constituted by the molecules of dust; and the pieces of glass are pressed not against one another, but against these fulcrums. There are innumerable—say thousands—of little particles of dust jammed between the glass, some of them of perhaps 1/3,000 of a centimetre in diameter, say 5 or 6 wave-lengths. If you lay one piece of glass on another, you think you are pressing glass on glass, but it is nothing of the kind; it is glass on dust. This is a very beautiful phenomenon, and my first object in showing this experiment was simply because it gives us a linear measure bringing us down at once to 1/100,000 of a centimetre.

Now I am just going to enter a very little into detail regarding the reasons that those four lines of argument give us for assigning a limit to the smallness of the molecules of matter. I shall take contact electricity first, and very briefly. If I take these two pieces of zinc and copper and touch them together at the two corners, they become electrified, and attract one another with a perfectly definite force, of which the magnitude is ascertained from absolute measurements in connection with the well-established doctrine of contact electricity. I do not feel it, because the force is very small. You may do the thing in a measured way; you may place a little metallic knob or projection on one of them of 1/100,000 of a centimetre, and lean the other against it. Let there be three such little metal feet put on the copper; let me touch the zinc plate with one of them, and turn it gradually down till it comes to touch the other two. In this position, with an air-space of 1/100,000 of a centimetre between them, there will be positive and negative electricity on the zinc and copper surfaces respectively, of such quantities as to cause a mutual attraction amounting to 2 grammes weight per square centimetre. The amount of work done by the electric attraction upon the plates while they are being allowed to approach one another with metallic connection between them at the corner first touched, till they come to the distance of 1/100,000 of a centimetre, is 2/100,000 of a centimetre-gramme, supposing the area of each plate to be one square centimetre.

(*To be continued.*)

DEATH OF THE PRESIDENT OF THE ROYAL SOCIETY

It is with the profoundest regret that we announce the death of Mr. Spottiswoode, the President of the Royal Society, at 11.15 yesterday morning. The bulletin issued on Tuesday to the effect that although there was no hemorrhage, still that there was no improvement in Mr. Spottiswoode's condition, boded ill because those who knew him best feared that a reserve of strength, which might perhaps have made way against the further progress of the fever through its later stages, was wanting.

As the sad news reaches us just as we are going to press, and as indeed we so recently entered at some considerable length into the lifework of him who is now no more, there is no necessity for us on the present occasion to do more than make the above announcement. This, however, must be said: that there is hardly a man of science in this country, and there are very many in other countries, who will not feel that they have lost a true friend, and one of whose friendship any man might have been proud. There is little doubt too that if he had been more sparing of himself in the various duties which were incumbent upon him as President of the Royal Society, if he had not so freely given all his thoughts and all his exertions to any scientific question which was going on, there might have been more time for relaxation, and there might have been strength to have tided over the illness which has now laid him low.

NOTES

We regret to have to announce the death of General Sir Edward Sabine, K.C.B., which occurred on the 26th inst. at Richmond, where he had been residing for the last twelve months. He was in his ninety-fifth year, having been born October 14, 1788.

At the meeting of the Paris Academy of Sciences on Monday last week the following message concerning the eclipse observations from M. Janssen, dated San Francisco, was read:—"*Janssen:* discovery of the Fraunhofer spectrum and the dark lines of the solar spectrum in the corona, showing cosmical matter around the sun. Large photographs of the corona and the circumsolar regions to a distance of 15°, in search for intra-Mercurial planets. *Palisa* and *Trouvelot:* Exploration of the circumsolar regions; no intra-Mercurial planets found. *Trouvelot:* Sketch of the corona. *Tacchini:* Polarisation of the corona and streamers; spectrum of the streamers, showing analogy

with the spectrum of comets; continuous spectrum corona; spectrum of protuberances; plates and drawings of protuberances."

THE American Association for the Advancement of Science will hold its thirty-second annual meeting at Minneapolis, Minn., August 15 and following days. The president-elect is Prof. C. A. Young of Princeton, and the following is the list of the sectional vice-presidents of the meeting:—Section A (Mathematics and Astronomy), W. A. Rogers of Cambridge; B (Physics), H. A. Rowland of Baltimore; C (Chemistry), E. W. Morley of Cleveland; D (Mechanical Science), De Volsen Wood of Hoboken; E (Geology and Geography), C. H. Hitchcock of Hanover; F (Biology), W. J. Beal of Lansing; G (Histology and Microscopy), J. D. Cox of Cincinnati; H (Anthropology), O. T. Mason of Washington; I (Economic Science and Statistics), F. B. Hough of Lowville. The permanent secretary is F. W. Putnam of Cambridge; the general secretary (of the meeting), J. R. Eastman of Washington.

THE annual meeting of the American Academy of Arts and Sciences was held in Boston, Tuesday, May 29. The following officers, we learn from *Science*, were elected for the ensuing year:—President, Prof. Joseph Lovering; vice-president, Dr. Oliver Wendell Holmes; corresponding secretary, Prof. Josiah P. Cooke; recording secretary, Prof. John Trowbridge; treasurer, H. P. Kidder; librarian, S. H. Scudder. M. Adolph Wurtz of Paris was elected a foreign honorary member. The list of members of the Academy now includes 192 resident Fellows, 92 associate Fellows, and 72 foreign honorary members. The Academy voted unanimously to confer the Rumford gold medal upon Prof. Henry A. Rowland of Baltimore for his researches in light and heat.

A NEW mode of measuring light was proposed at the last meeting of the Royal Society by Mr. Preece, F.R.S. The standard of reference is a small surface illuminated to a given intensity, and the mode of comparison is the light given by a small glow lamp whose state of incandescence is raised or lowered by increasing or diminishing an electric current. The amount of illumination is measured by the amount of current flowing, so that the number of amperes gives the degree of illumination. The standard surface is that illuminated by a British "candle" at 12·7 inches, and this is the same as that produced by the French "bec" at 1 metre distance. In this way sunlight, moonlight, twilight, fog, and the amount of illumination in any part of a room or building, or that distributed over a street or area at any time of day or night can be measured without any reference to the source of light or its distance from the point lighted. We have in fact a standard of illumination very easily and simply measured.

PROF. BUREAU has been appointed Director of the Jardin des Plantes in place of the late M. Decaisne.

ON Saturday, June 16, a joint meeting of the Essex Field Club and the Geologists' Association was held at Grays for the purpose of visiting the "Deneholes" in Hangman's Wood. From fifty to sixty members and visitors, including many members of the Anthropological Institute, were present, and nearly all had an opportunity of descending one or both of the two holes which were exhibited for the occasion. The meeting was under the conductorship of Mr. T. V. Holmes, F.G.S., who has written a paper giving an account of last year's preliminary exploration, which will shortly appear in the *Transactions of the Essex Field Club*. Photographs of the interior of one of the holes were successfully taken by Mr. A. J. Spiller by means of magnesium burning in oxygen. The party assembled for tea at the "King's Arms" Hotel, when the president of the Club, Prof. G. S. Boulger, and Mr. Holmes announced that it was the intention of the Club to undertake the systematic investigation of these interesting prehistoric remains, both at Grays and elsewhere along the Essex shore of the Thames. A large fund will be required for this work, and a committee has been formed for the purpose of organising the explorations, which will be carried on under the personal superintendence of Mr. T. V. Holmes and Mr. F. C. J. Spurrell. After some remarks by Dr. Hicks, the president of the Geologists' Association, the meeting broke up. A public appeal for assistance will shortly be made, and in the meantime subscriptions will be gladly received by the treasurer of the Essex Field Club, Mr. Andrew Johnston, J.P., The Firs, Woodford, or by the Hon. Secretary, Mr. William Cole, Laurel Cottage, Buckhurst Hill, to be paid to the account of the "Denehole Exploration Fund."

IT will be seen from our Correspondence Columns that an earthquake was felt in the south-west of England on Monday. The shock seems to have spread very widely over Devonshire and East Cornwall. At Holsworthy, Devonshire, a very perceptible shock was felt at seventeen minutes to two o'clock that afternoon. Floors shook, and doors and windows rattled as from a passing train. No damage is reported. A severe shock was felt at Hartland and Clovelly at 1.30 p.m., and a second shock at Clovelly at 1.40. Houses shook considerably, and the bottles on counters in shops were knocked against each other. A similar statement is sent from Bude. The inhabitants of Princetown and the vicinity of Dartmoor, about two o'clock were startled by two smart shocks, followed by a subterranean rumbling like the passing of a very heavy waggon, or the echo of distant thunder. The first trembling was of sufficient intensity to be perceptibly felt by those who happened to be occupying a chair, and the like effect was produced on small movable objects, but it resulted in no mischief. The disturbances apparently travelled from north east to west. At Launceston at twenty minutes to two a shock was felt, accompanied by a rumbling noise, which lasted at intervals during about thirty minutes. The houses shook, and china and earthenware rattled on the shelves. About an hour afterwards another shock was felt, but not so severe. Similar information comes from Lostwithiel, Liskeard, Lydford, Tavistock, Okehampton, and Bideford.

THE Fine Art Society (148, New Bond Street) have sent us an artist's proof of M. Leopold Flameng's very fine etching after Mr. John Collier's picture of the late Mr. Darwin. The original is admittedly faithful and characteristic, and of high rank as a work of art, and M. Flameng has been perfectly successful in reproducing the artist's intention. The result of both these labours is a portrait of the greatest man of science of this century, which all other men of science should be glad to possess. We believe the number printed is limited. M. Flameng will also undertake a similar etching of Mr. Collier's picture of Prof. Huxley in this year's Exhibition.

WE have received the Report of the Royal Victoria Coffee Hall, where, as may be known to many of our readers, much good work is being done at the present time in the way of providing cheap amusement every night, free from the temptation to drink and other evils common to ordinary music halls. Among other experiments being tried are short lectures of the simplest and most popular kind, generally on some scientific subject illustrated by the oxyhydrogen lantern. We are told that a really good lecturer who understands his audience as well as his subject meets with a most encouraging reception, but that very few men of science give their assistance in this good work. We regret this; but we believe it is largely due to the fact that very little is known of the work in question, and that if a general appeal were made to those men of science who occasionally give an account either of their own work or the work of others, many would be found willing to join in the

effort which is now being made to interest the working classes in science in what was formerly the Royal Victoria Theatre.

OUR Paris Correspondent writes:—An interesting experiment took place on June 24, at the early hour of 3.45 in Paris, for the purpose of testing the capacity of accumulators of the French Storage Company to move tramcars. We travelled up to La Muette and back, a distance of 30 kilometres, in about three hours and twenty minutes, including stoppages and loss of time incurred by several incidents. The road has many steep inclines, which were ascended without difficulty. The mean velocity exceeded 10 kilometres per hour. The electricity was supplied by seventy accumulators, weighing 30 kilograms each, which were placed under the seats. At starting the potential was 140 volts, and having completed its task the current was as high as 126, so that at least 10 kilometres more could have been run if deemed necessary. This run is the longest on record made by electricity. M. Philippart was directing the operations.

THE Balloon Exhibition was closed at the Trocadéro on the 24th inst. It was visited by two officers of the British army, sent by the Government to report. Among the notable objects we may mention the original valve used by Gay-Lussac in his ascent, a new valve used by French aëronauts, the car and net of Lhoste as rescued from the North Sea, a panoramic apparatus for photographing a bird's-eye view of scenery as seen from a balloon at an altitude of 200 metres, several photographs taken from the cars of captive or free balloons in Paris, Boston, and Rouen, a refrigerator by Mignon and Bouard for instantly condensing vapour from clouds, bichromate elements constructed by Trouvé for Tissandier's intended aërial experiments.

THE following are the details of the method by which the fairy-like illuminations at Moscow at the coronation were produced:—The Tower of Ivan the Great and its side galleries were lit up by 3,500 small Edison lamps, fed by eighteen portable engines, which moved a number of dynamo-electric machines of every existing system. The portable engines and machines were kept at the other bank of the Moskwa. The sheds communicated with the tower by seventy aërial electric wires. On the ramparts of the Kremlin towards the river eight large and ten smaller electric suns threw their light over the river. The rest of the illuminations consisted of 200,000 lamps and 30,000 coloured glass globes, 50,000 lanterns of Venetian glass, 600,000 tapers, and 10,800 lb. of fireworks.

THE National Museum at Washington is one of the best examples in the United States of the practical application of electricity. In so large a building it was found advisable to take advantage of the best means of communication, first being its system of telephones and call-bells, by which those in any room can communicate with every room in the building. Twenty-six telephones are connected by a local telephone exchange, which in turn is connected with the main telephone office of the city. The result is that but three messengers are needed in this vast establishment. The photographic laboratory is independent of the sun, owing to the electric light there used. If one of the 850 windows or 230 doors is opened, a bell rings, and an electric annunciator shows to an attendant at the main office which window or door it is. This system is soon to be applied to every case of specimens. The watchmen at night, also, are kept to their posts by hourly releasing an electric current at certain stations, which pierces a dial and records their visit. The sixteen clock dials are likewise run by electric currents.

A MONUMENT to the memory of the celebrated naturalist, Lorenz Oken, will be unveiled at Offenburg on August 1 next. It will be in the shape of a fountain crowned with a marble bust of Oken.

MESSRS. GRIFFITH AND FARRAN have issued a new and cheaper edition of Mrs. Lankester's "Talks about Plants; or Early Lessons in Botany," first published in 1878.

PRINCE LUDWIG FERDINAND of Bavaria, an indefatigable worker in the domain of comparative anatomy, is about to publish a monograph on the tongue. Riedel (Munich) will be the publisher.

A MAGNIFICENT meteor was observed at Giesshübl, near Mödling (Vienna), on June 3, at 9.44 p.m. It seemed to consist of two fireballs, an emerald green one followed by a red one. They both moved apparently at a not very great altitude in the direction south-east to north west. The phenomenon lasted for three seconds. It is remarkable that the meteor seen at the same place on the evening of March 13, moved in almost exactly the same direction. Also at Gau-Algesheim (near Mainz) a fine meteor was seen in the northern sky on the evening of June 3; it left a most vivid trail behind, which shone for some time along the whole extent of its path.

A WRITER in the *North China Herald* gives some curious information respecting the foot-measure in China. At present it varies largely in different parts of the country and according to different trades; thus the foot of the carpenter's rule at Ningpo is less than ten, while that of the junk-builders at Shanghai is nearly sixteen, inches. But a medium value of twelve inches is not uncommon. The standard foot of the Imperial Board of Works at Peking is twelve and a half inches. A copper foot-measure, dated A.D. 81, is still preserved, and is nine and a half inches in length. The width is one inch. The small copper coins, commonly called *cash*, were made of such a size, sometimes, as just to cover an inch on the foot-rule. In the course of two centuries it was found that the foot had increased half an inch, and a difference in the dimensions of musical instruments resulted. Want of harmony was the consequence, and accordingly in A.D. 274 a new measure, exactly nine inches in length, was made the standard. Among the means employed for comparing the old and new foot are mentioned the gnomon of official sundials, and the length of certain jade tubes used according to old regulations as standards. One of these latter was so adjusted that an inch in breadth was equal to the breadth of ten millet seeds. A hundred millet seeds, or ten inches, was the foot. The Chinese foot is really based on the human hand, as is the European foot upon the foot. It strikes the Chinese as very incongruous when they hear that we measure cloth, woodwork, masonry, &c., which they regard as especially matters for the hand, by the foot. Of the jade tubes above mentioned there were twelve, and these formed the basis for the measurement of liquids and solids four thousand years ago. They are mentioned in the oldest Chinese documents with the astrolabe, the cycle of sixty years, and several of the oldest constellations. It is likely that they will be found to be an importation from Babylon, and in that case the Chinese foot is based on a Babylonian measure of a span, and should be nine inches in length.

MR. CHAS. G. LELAND, the writer of No. 4 (1882) of the "Circulars of Information" of the United States Bureau of Education, on the subject of Industrial Art in Schools, after premising that ornamental art is innate in man, and indeed is developed in a race before it attains proficiency in the useful, and remarking that the brains of the Parisians of the thirteenth century, when Gothic art adorned every object, were much smaller than they are now, draws the conclusion that children are more open to art education than to technical training. He finds the sexes equal in ability; urges outline drawing and monochrome as the foundation of further work; recommends the use of various mechanical helps, as of compasses and stencilling as actual incentives to freehand drawing; urges the practice of freehand

from the shoulder, even in the variety of drawing called writing; and gives a wonderful list of artistic effects which pupils who have had only short instruction in these arts are competent to produce. Education of this sort is valuable as simply affording healthy occupation of body and mind to some classes; it opens the eyes of the mind, which will tend to make work popular instead of idleness. He teaches that nothing made by machinery can be artistic; physical comforts may be supplied by it, but works of taste and refinement must be hand-made, and among the poorer classes should be the produce of home art, like the carved oak of Ann Hathaway's cottage. Mr. Leland was one of the first to point out that the decay of the apprentice system must soon necessitate industrial education, and he has prepared a series of cheap art-work manuals on decorative design, ceramic or porcelain painting, tapestry or dye-painting, outline and filled-in embroidery, decorative oil-painting, wood-carving, *repoussé* or sheet-brass work, leather work, papier mâché, modelling in clay, with underglaze faïence decoration, and stencilling.—No. 5 of the "Circulars" is on the subject of Maternal Schools in France, which answer to our Infant Schools. The value of them as laying the foundations of education is urged by the Commissioners. Excellent suggestions for object lessons, whose subjects are supplied by the season of the year, and also for the arrangement of school buildings are given. The result of such schools should be a slight training of the senses by object-lessons; the beginnings of habits and dispositions favourable to future education; a taste for gymnastics, for singing, and for drawing; an eagerness to listen, observe, question, and answer; the power of attention; a generally quickened intelligence, and a mind open to receive good moral influences. In other words, education is a "bringing forth" of the powers of the mind, and not a making it a live cyclopædia. No. 6 is a full copy, with a few useful notes for comparison, of the English Report of the Royal Commission on Technical Education in France, presented by Mr. Samuelson and his coadjutors in February, 1882.

WITH the May number the *Journal of Forestry* changed both its title and the colour and design of its cover, and it now appears under the simple name of *Forestry*. It is an acknowledged fact that changes of this character are generally inadvisable in a journal of long-established reputation, but under the editorship of Mr. Francis George Heath we have no doubt that *Forestry* will at least maintain the reputation and circulation it had attained under its old management, if it does not increase them, which indeed it is most likely to do. The May number opens with an editorial note entitled "A May Note," in which the glories of spring and summer in woodland glades and forest are set forth. Then Mr. R. D. Blackmore gives us "A Cuckoo Song." Amongst other readable articles may be mentioned "Lord Somerville; a forgotten President of Agriculture," by Mr. R. A. Kinglake; Mr. Boulger's "Beauties of British Trees," and Mr. Guillemard's "Forest Ramble in New South Wales." In the June number the same amount of interest and variety is maintained. Mr. Guillemard gives "A Forest Ramble in Norway." The article on "Epping Forest and its Future Management" will however, we have no doubt, be read by most readers, as any one having the slightest inclination towards any branch of natural history cannot fail to be interested in maintaining the Forest in all its native beauty, and if *Forestry* is able by its advocacy, backed up by the opinions of those who are now taking a lead in the matter, to stem the tide of improvements so-called in Epping Forest, it will have fulfilled a work for which thousands will be thankful.

THE additions to the Zoological Society's Gardens during the past week include a Crab-eating Raccoon (*Procyon cancrivorus* ♂) from Brazil, presented by Mr. Theo. Walsh; a Ring-tailed Coati (*Nasua rufa* ♂) from Brazil, presented by Mr. R. G. Hamilton; two Common Hedgehogs (*Erinaceus europæus*), British, presented by Mr. S. Mummery; four Restless Cavies (*Cavia caprera*) from Brazil, presented by Mr. E. H. Draper; a Ring-necked Parrakeet (*Palæornis torquatus*) from India, presented by Mr. W. Quail; two Common Kingfishers (*Alcedo ispida*), British, presented by Mr. T. E. Gunn; three Common Vipers (*Vipera berus*), British, presented by Mr. C. Taylor; two Common Snakes (*Tropidonotus natrix*), European, presented by Lord Arthur Russell, F.Z.S.; a Puma (*Felis concolor* ♀) from South America, a Goffin's Cockatoo (*Cacatua goffini*) from Queensland, deposited; two West African Love Birds (*Agapornis pullaria*) from West Africa, an Indian Python (*Python molurus*) from India, purchased; two Vulpine Phalangers (*Phalangista vulpina*), eight Gold Pheasants (*Thaumalea picta*), six Prairie Grouse (*Tetrao cupido*), a Herring Gull (*Larus argentatus*), bred in the Gardens.

OUR ASTRONOMICAL COLUMN

THE NEXT TOTAL SOLAR ECLIPSE.—In NATURE, vol. xiv. p. 450, we gave some results of an approximate calculation of the total eclipse of the sun on September 8–9, 1885, wherein the central line traverses New Zealand, but does not encounter land in any other part of its course. The correction required to the moon's place there employed is sufficiently important to render a new calculation of interest, and we shall accordingly present here some of the circumstances of the eclipse, resulting from the substitution of the lunar places in the *Nautical Almanac*, which are founded upon Hansen's Tables, with Prof. Newcomb's corrections. The elements of the eclipse as given in the ephemeris are employed, excepting that in place of Hansen's semidiameter of the moon, we infer the semidiameter from the ratio, 0.2725 of the horizontal parallax.

At a point in longitude 11h. 40m. 0s. E. of Greenwich, with 40° 49'·4 south latitude (nearly on the central line) the total eclipse begins, September 9, at 7h. 44m. 9s. local mean time, and continues 1m. 51s., and this will be about the longest duration of totality available for observation upon land in this eclipse. For any place near the above point, the Greenwich mean times of beginning and ending of totality may be obtained from the following formulæ:—

$$\text{Cos. } w = -116{\cdot}3108 - [2{\cdot}10869] \sin. l + [1{\cdot}64777] \cos. l \cos. (L - 161°\ 16'{\cdot}5)$$
$$t = 8\text{h. } 58\text{m. } 21{\cdot}1\text{s} \mp [1{\cdot}74578] \sin. w - [3{\cdot}37987] \sin. l$$
$$- [3{\cdot}85619] \cos. l \cos. (L - 145°\ 49'{\cdot}6).$$

Here L is the longitude from Greenwich, reckoned *positive*, and *l* the geocentric latitude, which may be deduced from Mr. Stone's valuable table in the *Monthly Notices of the Royal Astronomical Society* for January last, a table it might have been worth while to publish separately. The quantities in square brackets are logarithms.

As one result of the introduction of the more accurate place of the moon, it is found that the central line approaches much nearer to Wellington; a direct calculation for that place shows that the total eclipse begins there at 7h. 44m. 23s. a.m., and ends at 7h. 45m. 46s. local mean time, thus continuing 1m. 23s., and the same figures are given by the above equations. At Nelson totality commences at 7h. 37m. 16s. a.m. local mean time, and continues 1m. 3s.

It may be noted that during the totality of this eclipse the planet Jupiter will be situated only 45' from the sun's limb, on an angle of about 26° with the circle of declination at his centre.

THE ANNULAR SOLAR ECLIPSE OF OCTOBER 31, 1883.—In May last we had a case where the track of a total eclipse of the sun was almost wholly an ocean-track, and where it was consequently necessary to send expeditions to the Mid-Pacific, to obtain observations. The annular eclipse in October next is similarly circumstanced; excepting possibly one or two mere rocks in the Pacific, it will not be observable on land, elsewhere than on the island of Niphon, Japan. If we calculate from the *Nautical Almanac* elements for longitude 9h. 20m. 48s. E. and latitude 38° 11' N., we find the annular phase commences at 7h. 28m. 2s. a.m., and ends at 7h. 35m. 23s., a duration of 7m. 21s., and the sun will be at an altitude of about 12°. At the capital, Tokio, the eclipse will not be annular; the greatest phase is at 7h. 28m. a.m., magnitude 0·88 (the sun's diameter being taken as unity).

THE GREAT COMET OF 1882.—In No. 2521 of the *Astronomische Nachrichten* is an elliptical orbit of this comet by Mr. John Tatlock, jun., of Williamstown, Mass., with a period of 1376 years, which, as Prof. Krueger remarks in a note, differs materially from the results of Kreutz, Frisby, and Fabritius. It may be added that the new calculation can have little weight, being founded upon normals for October 8, November 24, and January 29, so that at the date of the first normal the comet was already far past the perihelion, and in fact during the whole interval only described a heliocentric arc of about 5° 10'. Dr. Kreutz has shown the possibility of closely representing by the same orbit the anteperihelion observations and those made subsequently to perihelion passage, though there may be need of much more minute discussion before it can be safely assumed that there was absolutely no appreciable effect from the comet's passage through the solar coronal region.

GEOGRAPHICAL NOTES

Science announces that Lieut. Schwatka, accompanied by Assistant-Surgeon Wilson, C. A. Homan, U.S. Engineer Corps, and three private soldiers, left for Chilkat, Alaska, May 22, from Portland, Or., on the steamer *Victoria*. They are provisioned for a six months' cruise, will employ Indians for packers, &c., and intend to ascend the Chilkat River to its head, make the passage to the head waters of the Lewis River, and descend the same to its junction with the Yukon, and descend the Yukon River to its mouth. It is said to be their intention to survey the course of these rivers; and there is no doubt that a properly qualified and equipped party would find abundance of useful work ready to their hands. The whole route has been travelled before, but not by persons in search of and qualified to obtain geographical information, except in very small part. The explorations of the Krause brothers on the Chilkat and vicinity have been alluded to before. The Yukon has been superficially examined by McMurray, Ketchum, Zagoskin, Dall, Whymper, Raymond, Nelson, and others, and a few points have been astronomically determined; but nothing like an exact map has been attempted, nor do the data for it exist. Astronomical and magnetic observations anywhere along its banks, and especially any data for a map of the Lewis River and its feeders (which are only known from the reports of prospectors and natives), would be of the highest interest.

AT last news has again been received by Dr. Schweinfurth from the well-known African explorer, Dr. W. Junker. He was still in the Nyam Nyam country, and his last news was dated October 16, 1882, from the residence of a chief named Semio some days' journey south of the Mosio district of the present maps. Dr. Junker, who has travelled through vast districts hitherto unexplored, will now soon return home. The last time he had spent principally in various excursions, during which he repeatedly crossed the Uëlle River to the south, and also the third degree north latitude, leaving his provisions in the care of his companion, Herr Bohndorff. On September 27 he again joined the latter after an absence of eighteen months, but found him so poorly that he had to send him home with the collections made up to that time. Bohndorff started with thirty-two porters, who carried the natural history and ethnographical collections. Of special interest for geographers was an excursion of Dr. Junker's, which he made south of the former Munsa district of the Monbuttu. Some seven days' journey (about 60 kilometres) south of this place he reached a large river named the Nepoko, which the traveller identified with Stanley's Aruwimi, one of the main northern tributaries of the Congo in the middle course of the latter.

DR. POGGE has sent a report from the Mukenge station on the Lulua regarding his return journey from Nyangwe, showing that this was not quite as peaceable as the journey to Nyangwe, and that he had frequently to defend himself seriously against the enmity of the natives. From the Lualaba to the Lomani, Dr. Pogge travelled by the same route as he had previously come with Lieut. Wissmann

ONE of the most recent additions to the "Bibliothèque d'Aventures et de Voyages" published by Dreyfous of Paris is a volume containing the letters and journals of La Perouse during his famous voyage round the world in 1785–88, which ended in the disappearance of the circumnavigator among the islands of the South Pacific. The volume is annotated by M. George Mantoux, who also supplies a prefatory memoir of the great sailor.

"IM Reiche des Æolus" is the title of a little book by Adolf von Pereira, published by Hartleben of Vienna, and containing reminiscences of a tour the author undertook to the Lipari Isles. It is profusely illustrated and contains a map.

AUSTRIAN papers report that a mountain in the neighbourhood of Czernowitz, in the Bukovina, is manifesting singular symptoms of disturbance. The ground around its base, to the extent of over 1000 fathoms, has opened out in wide and deep chasms. Most of the houses of a village on the spot (Kuczumare) have fallen down.

THE Thuringian Geographical Society met at Jena on the 17th inst., when Prof. Hæckel read a paper on the flora of Ceylon, and Herr G. Kurze one on the outposts of European civilisation on the way from Zanzibar to Lake Tanganyika.

THE SPECTRUM OF THE AURORA

IN view of the increased frequency of auroras, an inquiry into the present position of our knowledge as to their spectra has seemed to me desirable.

The accompanying table gives in wave-lengths all the observations I could find of the position of the bright lines of the auroral spectrum. J. R. Capron's "Auroræ and their Spectra," goes more fully into the subject than any other work I know, and therefore many of the positions are taken from it, being found on the page or plate indicated in the column headed "Page, &c." The authorities for other observations are given in the notes, but in other cases again I cannot now state whence I obtained them.

They are arranged approximately in order of accuracy,[1] but this is manifestly a very difficult matter to decide: if, as is very likely, I have made mistakes in this respect, I hope I shall be excused. I have gone very carefully into the matter, judging of the accuracy of the observations partly by their internal evidence, and partly by the weights which are in some cases attached to them by the observers themselves. The observers' probable errors are given in the table after the positions of the lines. I consider J. R. Capron has attributed too much accuracy to most of the observations of the auroral spectrum that have hitherto been made; certainly he has to mine. Nearly all the observers have measured the principal line; and, as its position is very well known, the measurements of it are to a considerable extent a guide to the amount of dependence that may be placed on the rest. Of course it may happen to be measured correctly by accident, while the rest are incorrect; but, on the other hand, if it is incorrectly measured, it is not likely that the rest will be correct. It is, therefore, very desirable that observers should measure this line at the same time as they measure any of the others; not necessarily in order to a certain its position, but as an indication of the correctness of the rest; although it does not always happen that all the lines are by any means equally accurate.

The most probable positions of the lines, given at the foot of the table, are derived from the most accurate of the observations of each. Below are indicated the observations which have been used in the calculation in each case, with the weight given to each; for I have not taken the simple average of those used, but have given higher weights to those that seemed the best. The "Probable Error," as given below the "Probable Average," is partly calculated and partly estimated; it seems rather large; perhaps it should not really be so large.

My second series of observations (No. 18 in the table) are not absolute measurements, but only comparisons with α and γ. I have therefore not used them in the calculation of the general averages. This series is most likely affected by constant errors much larger than the probable errors given in the table from calculation. It seems rather curious that the actual errors of my first series (No. 17) are nearly all so much greater than the probable errors; and possibly the same thing may occur in some other cases.

E. B. Kirk's observation (No. 28) (though a very rough one as regards position) is one of the most striking of all; and, being unique, confirmation of it is very desirable. It will be described under the different lines, &c., concerned.

Where I have attached to an observation a Greek letter with a note of interrogation, it means that it is uncertain whether the

[1] But the observations of each observer are placed together, however unequal in accuracy they may be.

line observed was that named at the head of the column. It is not always possible to identify the lines, and, in some cases, my identifications disagree with J. R. Capron's.

This table shows eleven distinct and well-separated lines or bands, the existence of all of which may, in my opinion, be considered proved, all but λ having been seen by two or more observers.

Seven have been seen by numerous observers; I have myself seen them repeatedly, though none of the other four with certainty; but these (θ, ι, κ, and λ) have all been seen by trustworthy observers, and therefore may be accepted, though of course further confirmation would be advantageous.

I shall now consider each line separately.

β is the only line yet seen in the red. It exists in all auroras that are tinged with red, though the spectroscope does not always show it. I have always seen this line most easily with a single prism; but often the slit has to be so wide that it appears merely as a red border to α. In those reddish auroras where I have not been able to detect it, its invisibility is probably due to moonlight or some such cause. There can be no doubt that this line is the cause of the red colour. It varies greatly in brilliancy with reference to α; I have seen it as bright as α, but never brighter.[1] It sometimes exists when no redness is perceptible in the aurora, it being overpowered by the other rays.

α is almost always the brightest line in the spectrum. The only exceptions I have seen were that once β was as bright, and once ϵ brighter (see below). α probably exists in all true auroras. In a few very faint ones I have not been able to see it, perhaps because it has been overpowered by the diffused light of the spectrum, which certainly varies in brightness relatively to the brightness of the lines. Very often when there is no decided aurora, a luminosity overspreads the sky, uniform in all directions, though fading gradually towards the zenith; I have several times seen the line α in its spectrum, but at other times it has been invisible, though the light has appeared equally bright.

α and β are slender lines; it is not clear whether this can be said of any of the other lines or bands.

θ. Wijkander's is the only reliable observation of this line, but Vogel has one not far from the same place, and as he states his position is unreliable, there is no reason to doubt its being the same line. There is more doubt whether Peirce observed the same.

ι was observed both by Wijkander and Parent, and probably by Peirce and Copeland.

ϵ consists of two lines, according to Vogel, which I have called ϵ^1 and ϵ^2; the latter, he says, became very bright whenever β appeared. He is the only observer who describes two lines here (though I have at least once suspected ϵ to be double), and therefore it is difficult to tell which of them other observers have seen, or whether they have seen both combined as one band. In the table I have assumed that the latter has usually been the case, or at least that a band has been seen in this place; possibly this band has been different from either. I have therefore placed the observations which seem to apply to the band, or to the combined lines, in a separate column from those that seem to refer to the individual lines; but the average of the former includes the observations of the latter. Barker and Procter describe a band here; also E. B. Kirk, who, in 1880, August 12, saw it as a band fading towards the violet; but in 1882, August 4, it faded away on both sides, though quite sharp at the edges, and with a pretty narrow slit it was broken up into lines—his impression is there were six or eight; and that the group was broader than the distance between ϵ^1 and ϵ^2. He used one of Browning's "Maclean star spectroscopes," with an ordinary convex lens instead of the cylindrical one.

I have carefully examined my observations of this band, to see whether it has appeared more refrangible when the red line has been visible or bright than when there has been little or no red; but the result of this examination is inconclusive. However, in 1869, April 16, and 1874, October 3, I noted that ϵ was relatively brighter in the red part of the aurora than elsewhere, so far confirming Dr. Vogel.

ϵ exists in nearly all auroras that are bright enough to show any line besides α; perhaps in all. It varies very much in brightness with respect to the other lines. I have sometimes found it the brightest next to α, and once the brightest of all, viz. in 1882, November 20, between 5.50 and 6.5 a.m. There was twilight at the time in addition to the aurora, but I do not see how this could produce the effect. Between 5.40 and 5.50 that morning I estimated α three times as bright as ϵ, which was the second brightest line; but I see no way of avoiding the conclusion that it was the brightest of all a few minutes later.

[1] In considering the relative brightness of the different bands, it must be borne in mind that it varies considerably with the width of the slit, the dispersion, &c.

ζ may be seen in most bright auroras. It is sometimes brighter than ϵ.

η is much more seldom visible; but I have several times seen it brighter than either ϵ or ζ; rarely as bright as γ, or brighter than it; but never so conspicuous as it, as the latter is rendered more visible by its position at the edge of the brighter part of the continuous spectrum.

γ and δ belong, I believe, to all auroras, always being visible when the spectrum is moderately bright. But their brightness varies with respect to each other and to α; indeed I do not know any two auroral lines that always vary together.

γ is usually the brightest line next to α, with my mode of observation. Several observers describe it as a band; Vogel as a double band (if not triple). Capron, on the authority of A. S. Herschel, says it consists of "two lines, the first rather more frequently noted than the second" (the more refrangible). I cannot see that this assertion is borne out by the accompanying table, but if it is correct, the two lines must be about 4700 and 4654. I have several times seen γ as a band fading towards the violet. E. B. Kirk, in 1880, saw it as a band; but in 1882, August 4, resolved it into bright lines—a broader group than ϵ, less distinctly bounded, and with a less bright centre, and containing, he thinks, about twice as many lines.

κ appears to have been noted by two observers; one being Wijkander, who seems very accurate.

δ is invariably fainter than γ to me.

λ. Seen only by Lemström; but he says that in 1871, November 22, he "observed it with certainty three separate times."

The likelihood of the existence of lines in the violet or blue (such as λ or κ) in addition to those commonly seen, is manifest to me from the fact that I have twice seen purple in auroras. The first time was at Sunderland, 1869, May 13, at 10.55 p.m., when for a minute or two there was a large patch of coloured light—deep crimson, exquisite pink, and most lovely pinkish purple, gradually passing into one another. The crimson was the same colour frequently observed; the pink was very different, and far more beautiful. The crimson lasted after the other colours faded. The second time was in Skye, 1872, August 3, about 10.30 p.m., when for two or three minutes there were large patches of a beautiful, but not deep, pinkish purple. I had no time to observe the spectrum in either case. It is manifestly improbable that these colours would be caused by any of the ordinary lines of the spectrum; probably one line in the violet or blue, in combination with the red line, could account for the various tints.

Colours are closely connected with the spectrum; but I cannot say I ever saw any in the aurora, except the purples and pink just referred to, that might not be readily accounted for by the ordinary lines of the spectrum with or without the red line; as the only other decided colours I have seen are red and the usual greenish colour, varying somewhat in intensity and perhaps tint. I have seen other less decided colours; but, considering the extent to which the colours of the aurora might be affected by mist, smoke, twilight, moonlight, &c., and one's judgment by the effect of contrast, I could not say that they certainly belonged to the aurora.

The continuous light of the spectrum always reaches from α to δ; being very faint from γ to δ, rather faint from α to ϵ, and sometimes brighter from ϵ to η or to ζ than beyond. Sometimes when ϵ is too faint to be detected, the abrupt brightening of the continuous spectrum at that point is plainly visible. Kirk, in 1882, on the occasion already mentioned, when the spectroscope was pointed between the streamers, saw the spaces from α to ϵ and from ϵ to γ apparently filled with shifting lines, very numerous and close. Not that the lines really shifted, but their flickering caused them to appear shifting, and possibly also to appear more numerous than they really were. When the spectroscope was pointed on the streamers these lines were obscured by the greater brightness of the rest of the spectrum. I have myself often suspected lines between ϵ and γ, besides ζ and η.

It has been suggested that some of the lines may vary somewhat in position; but there is no evidence yet that the apparent variations are due to anything but errors of observation.

A flickering of the lines has been observed in certain cases; in all probability this occurs with the whole spectrum when the

POSITIONS OF LINES, IN WAVE-LENGTHS.

	Page, &c.	β	α	θ	ι	ϵ^1	ϵ	ϵ^2	ζ	η	γ	κ	δ	λ
1. Wijkander, A.	p. 96			5359±3	5289±5	5239±4			4996±9	4871	4692±2	4366	4280±3	
2. Vogel, Dr. H. C., av.	p. 198		5571·3±·9											
3. " "	pp. 99, 105	6297±14	5569±2	5390		5233±4		5189±9	5004±3		4663±3 (4694 to 4629)			
4. Parent, Lieut.	p. 96				5280±1			? ϵ 5207±11		4873	4708±5		4286±16	
5. Zöllner, F.	pl. xii.	6279												
6. Lemström, K. S.[1]			5569			? ϵ 5250			4959		4686		4256	4112
7. German North Polar Expedition.			5569											
8. Copeland, R., 1880.			5571·6±2		ι? 5275		ϵ? 5120±22		ζ? 4930±21		4714±20		4320±20	
9. Ångström, A. J.	p. 198		5567											
10. " "							? ϵ^2 5210				4720			
11. Von Oettingen.		6290±70	5548±30								4660±25		4240±12	
12. Barker, G. F.	pl. xii.	6300	5550 (this	series is	±20 to	± 6)	(5330 to 5200)		(5050 to 4990)	(4930 to 4850)	4705 (4740 to 4670)		4310	
13. " "	p. 97	6200	5620					? ϵ 5170	5020	? γ 4820				
14. Rowland, H. A. (Troy).		6283	5543											
15. Respighi, L.			5573						5050				4250	
16. Peirce.[2]			5570	θ? 5450	5315			? ϵ 5205		4860	4640		4310	
17. Backhouse, T. W.		6010	5605±14				5225		5025±9	4920	4663±10		4302±6	
18. " "		6280±100					5227±6		5002±5	4874±5			4370±6	
19. Struve, O., 1868.			5545±20											
20. Herschel, A. S.			5555±25											
21. Maclear, Capt. J. P.	pl. xi.		5510								4685	4410	4280	
22. Procter, H. R.		6270	5590				(5200 to 5100) 5170				(4840 to 4650) 4760		4240	
23. Smyth, C. P., 1872	p. 98	6350	5580				? ϵ^1 5300		5100	4900				
24. Ellery, R. J. (Melbourne).		6350	5600											
25. Church, A. H.		6180	5600								4870		4480	
26. Lindsay, Lord.	p. 102		5690				5330		5170		4850		4340	
27. Clarke, A., Jun.	p. 97		5690				5320				4850		4350	
28. Kirk, E. B.[3]			5800				5150				4850		4300	
29. T. F., Torquay.	p. 96	6400	5880								4870		4400	
30. Elger, T. G.	p 96	6550	5880						5280		4870			
Probable Average ...		6289	5569·7	5359	5285	5237	5226	5199	5001	4870	4688	4366	4278	4112
Probable Error		±6	±0·6	±15	±8	±8	±8	±8	±8	±8	±9	±12	±8	±20
Observations used for Average		3, *w* 1. 5, *w* 1. 12, *w* ·2. 14, *w* ·2	2, *w* 1. 6, *w* 1. 7, *w* ·5. 8, *w* ·5. 9, *w* ·5.	1	1, *w* 1. 4, *w* 1.	1, *w* 1. 3, *w* ·5	1, *w* 1. 3. *w* ·5. 4, *w* ·5. 6, *w* ·3. 10, *w* ·25. 12, *w* ·2. 16, *w* ·15. 17, *w* ·1.	3, *w* 1. 4, *w* 1. 16, *w* ·3.	3, *w* 2. 1, *w* 1.	1, *w* 1. 4, *w* ·6. 16, *w* ·2.	1, *w* 2. 3, *w* 1. 4, *w* 1. 6, *w* ·5. 11, *w* ·3. 12, *w* ·3. 10, *w* ·1. 17, *w* ·1.	1	1, *w* 2. 4, *w* 1. 6, *w* ·5. 11, *w* ·3. 12, *w* ·3. 14, *w* ·2. 16, *w* ·1. 17, *w* ·1.	6

[1] *Archives des Sciences Physiques et Naturelles*, 1874. [2] Reported by Winlock: *American Journal of Science*, vol. xlviii. [3] *Observatory*, 1880, p. 542; 1882, p. 272.

aurora flashes, though it does not seem to have been remarked. My own observations have all been made with a very wide slit, or, which comes practically to the same thing, with small dispersion. This has been owing to the usual feebleness of the greater part of the spectrum; and many of the other observers have for the same reason also used a wide slit. It may be useful to explain the method by which most of my observations were made, as it seems successful for perceiving the lines and general character of the spectrum, though not for measuring the positions. I have simply used one or two, or in some cases three prisms, usually of Chance's dense flint glass, and for a slit, the space between window-shutters nearly shut, or between two planks placed against the window. I hold the prisms in my hand on a simple stand, not always fixed, but so that they may be easily moved with respect to each other and to the slit, so varying the amount of dispersion. The best results are usually obtained by holding them in the position of almost the greatest deviation possible. Varying the deviation alters the focus. If one plank or shutter is placed rather further forward than the other, the apparent width of the slit is varied at will by simply moving one's head to one side or the other. By these means it is easy to observe all the different features of the spectrum, which require different widths of the slit and degrees of dispersion. A vacuum-tube or other light for comparison may be placed behind the slit, though it is obvious that with small dispersion accurate comparisons cannot be made.

I have made some observations with a Browning's "Miniature Spectroscope," with the diaphragm off, but it gives less light than simple prisms. I have also tried a "half-prism spectroscope," by Hilger, but unsuccessfully; but I find that by taking off the outer lens of the eyepiece and the diaphragm much more light is obtained; I have not, however, had an opportunity of trying this plan yet on an aurora.

The number of nights of aurora on which I have seen each line, between 1871, November 1, and 1883, March 27, is as follows:—

β	α	ϵ	ζ	η	γ	δ
11	83	34	14	7	33	26

On thirteen nights I could not be sure of any line, but on six of these I suspected α, or else there was an abrupt fading away about where α should come. There were other auroras—mostly faint ones—whose spectra I did not observe.

The lines visible in the spectrum often vary in the course of a few minutes, and indeed are not always the same in different parts of the sky at the same time. I have never been able to detect that any particular feature of the spectrum belongs to any particular type or feature of aurora, except that the line β belongs to red auroras.

Dr. Vogel thinks it probable that the auroral spectrum is a modified air-spectrum. The following are the most striking coincidences or approximations between my revised list of auroral lines and Vogel's lists of lines in the spectra of air and its constituents. They are sufficient to make the subject one worthy of consideration; but perhaps this is as much as can as yet be said. There are other approximations to very faint air or gas lines, which he regards as of some importance; but, as the lines in the latter spectra are so numerous, one would naturally expect such coincidences accidentally.

Aurora Spectrum Coincidences.

Aurora.		Air.		
ϵ^1 5237	...	Moist air	5231	dull.
ϵ 5226	...	N	5224	very bright.
ϵ^2 5199	...	O	5189	very bright.
	...	H	5187	very bright.
ζ 5001	...	N	5004	bright.
	...	Air	5008	very bright.
	...	Air	5002	very bright.
η 4870	...	O	4870	moderately bright.
γ 4688	...	N	4704	very intense.
κ 4366	...	O	4372	moderately bright.
	...	N	4363	bright.
	...	N	4357	bright.
	...	H	4358	very bright.
δ 4278	...	N	4275	very bright.

A. S. Herschel has pointed out the proximity of β to the dark atmospheric band α at 6279.

Sunderland THOS. WILLIAM BACKHOUSE

SCIENCE AT KAZAN[1]

THE Kazan Society of Naturalists, which began its *Memoirs* in 1871 with the remarkable work of M. M. Bogdanoff on the birds and mammals of the black earth region of the basin of the Volga, has continued since to publish a series of most valuable explorations of the region of the lower Ural, Volga, and Siberia. We notice thus in the first eight volumes of its *Memoirs* the researches on earthquakes in Siberia, in Turkestan, and on the Ural, by M. Orloff; several valuable papers on the Geology of the Obschiy Syrt plateau, by M. Sintsoff; of the Government of Vyatka, by M. Krotoff; of the Government of Kazan, by Prof. Stuckenberg; and of the banks of the Kama, by M. Zaitseff; a work on the birds of Caucasus, by M. Bogdanoff; a paper on the Teleostei of the mouth of the Volga, by M. Yakovleff; the history of the development of the *Acipenser sturio*, by M. Zalensky; and mycological researches, by M. Sorokin; several papers on the flora of the Government of Perm, by M. Kryloff; and two papers by M. Levakovsky on the substitution of certain species of plants for others in a given region; as well as several valuable researches into the anthropology of the Bashkirs, Voguls, and Votyaks, by MM. Malieff, Sorokin, and Ostrovsky.

The three last volumes of the *Memoirs*, which we have now before us, contain also many valuable papers. In the department of botany we notice the second part of M. Kryloff's flora of the Government of Perm. It contains a complete list of all Phanerogamæ discovered in this interesting province, which includes the Ural Mountains, completed by special researches into the subarctic and Alpine flora of this region. A map shows pretty well how such plants as the *Viburnus opulus*, the *Cystisus biflorus*, the *Tilia parviflora*, and the cereals are stopped in their extension by the Ural Mountains, reappearing again on their eastern slope; whilst others, like the *Quercus pedunculata*, or the *Acer platanoides*, are stopped in their extension towards the east by the western spurs of the Ural and the lowlands of Siberia, their north-eastern limit meeting nearly together with the south-western limit of extension of the *Pinus cembra*, the *Lonicera cœrulea*, the *Spiræa media*, and *Polygonum viviparum*. The whole list contains 948 Angiospermæ, and 8 Gymnospermæ. The Cryptogamæ are represented by 38 Lycopodiaceæ and 124 Lichens.—Dr. Martianoff publishes valuable materials for the flora of the Minusinsk region in Eastern Siberia, comprising a sketch of its climate (according to five years' meteorological observations by A. Krapotkin) and its physico-geographical characters. The flora of Minusinsk is much varied, as it embodies three separate botanical regions: the Alpine, the forest, and the steppe floras, intermingled with one another. Its general character is that of the Altai region, and out of 777 Phanerogams, no less than 714 are Altaic, whilst only 59 belong to the flora of Eastern Asia. The Alpine flora has but 104 representatives; the forest-flora is the most widely spread, and at the same time the richest; it is represented by 579 species. The steppe flora, which covers nearly one-quarter of the Minusinsk district, and appears sporadically even on the plateaux of the hilly tracts, numbers 315 species. We can only notice here the excellent botanical sketches of separate parts of this "Siberian Italy" which we find in M. Martianoff's work. His list of plants, which contains 760 species of vascular plants, is unusually rich also in lower plants, the number of determined Fungi and Myxomycetes comprising 644 species.—An interesting work which has cost much labour to its author, M. Kryloff, is a description of all drugs—mostly plants—used in the popular pharmacies of the Governments of Kazan and Perm. The list comprises about 200 plants, with a description of their use in popular medicine.

The zoological papers in this volume are but two:—On the innervation of the heart of the *Esox lucius* and *Acipenser ruthenus*, by MM. Kazem-beck and Doguel; and on the ear-labyrinth of the Plagiostomi (*Acipenser ruthenus*, *A. sturio*, and *A. schiffa*), by M. Sizoff. Both papers have appeared also in the *Archiv für microskopische Anatomie*.

Geology is represented by the following papers:—On the upper part of the mottled marls, by Prof. Stuckenberg; on the Permian in the Governments of Kazan and Samara, by A. Zaitseff; and on the geology of the Volga between Nijni-Novgorod and Kazan, by P. Krotoff. The Zechstein appears in the region situated between the Kama, the water-

[1] *Memoirs of the Society of Naturalists at the Kazan University*, vols. ix., x., and xi., 1880-1882.

shed of the Sok and Sheshma, and the Volga, as an island extending from north-east to south-west, and covered on its borders by mottled marls. The former is closely mingled with the latter, as it extends also in the shape of thinner intermediate deposits among the marls; but on the whole it substantially differs from them by its fauna, undoubtedly belonging to the Zechstein. As to the mottled marls, they contain the *Unio umbonatus*, Fisch., the *Estheria* sp. (*Posydonomia minuta*, Bronn.), the *Lingula orientalis*, Golowkinsky, scales of *Acrolepis macroderma*, Eichw., and Calamites. The Post-Pliocene deposits are spread everywhere, and we notice the find of Caspian shells of *Cardium*, together with *Dreissena*, at the sources of the Cheremshan river (left tributary of the Volga, namely, at Balandino, ten miles from the Cheremshanskaya fort). This important find proves thus that the Caspian formerly extended at least as far north as 54° 40′ north latitude. As to the Permian formation to the west of Kazan, M. Krotoff, who includes in this formation both the Zechstein and the mottled marls, calculates that it has a thickness of 810 to 860 feet. Showing further that the fauna of the mottled marls but slightly differs from that of the Zechstein (a complete list of its fossils being given by the author), and that the fossils that are characteristic of these marls (*Unio umbonatus*, Fisch., *Unio castor*, Eichw., *Estheria* sp., *Cythere* sp., remains of fishes, and Calamites) were found elsewhere, either in company with purely Zechstein forms or in deposits subordinate to the Zechstein deposits, he concludes—perhaps too soon—that there is no ground to consider them as Triassic.

Anthropology and archæology are represented by several interesting papers:—M. Krotoff publishes his researches into the age of the stone implements found in the basin of the Oka, and M. Ivanoff on the Perm region.—M. Malieff publishes the results of his most interesting measurements of the Old Bulgarian skulls dug out from the Babiy Bugor, at the Bulgarian village situated on the left bank of the Volga, close to Tetushi, and his paper is accompanied by sixteen photographs of four skulls. He measured the best preserved twenty-five skulls, all belonging to full-grown males. They are all much like one another, but could be subdivided into three groups: fourteen dolichocephalic, with indexes varying from 71·4 to 77·1; five mesatycephalic, their indexes varying from 77·8 to 79·8; and five subbrachicephalic, whose indexes vary from 81·1 to 82·1. The average size of the horizontal circumference of the twenty-five skulls is 515 millimetres, with a maximum of 555 millimetres and a minimum of 490; the average capacity is 1381 cubic centimetres. They completely differ from the skulls of other inhabitants of the same region: not only Kalmuks, or Bashkirs, but also from the Russian, Tartar, or Mordovian skulls. Without expressing a definitive opinion until a comparison of these skulls with those of Bulgarians from the Balkan peninsula is made, the author points out that they are very much like those of the Koorgan inhabitants of the Government of Moscow, who seem to be Old-Sclavonic, and certainly are not Finnish, as results from an inquiry made on 120 skulls by Prof. A. Bogdanoff. They are similar also to the skulls of the old inhabitants of Kieff and to those of the Scythes of Southern Russia. M. Malieff's companion in these researches, who gives in the same periodical a sketch of the Old Bulgarian burying-place at Babiy Bugor, adds that the skeletons they dug out had their heads towards the west, and were lying on the left side, looking towards the north (towards the Volga). Masses of pieces of earthenware were found together with the skeletons, and the pottery was of the roughest kind, made by hand, and burned very incompletely. He argues with much probability that this burying-place did not belong to a Mussulman people, but to idolaters, and supposes that its antiquity may be traced as far back as the Stone period. In any case, the customs of burying, as shown by this burying-place, seem to have been very much like those of the Sclavonians before their conversion. As to the burying-places at Chulpanovka and Ukrech, in the districts of Christopol and Laisher, explored by MM. Malieff and Vysotsky and described by the latter in his second "Anthropological Sketch of the Explorations of the Year 1880," and by M. Malieff in his just-mentioned paper, both explorers agree in considering them as belonging to Chuvashes. The craniological measurements which M. Malieff made on twenty skulls show that six of them belong to the mesatycephalic type, the average cephalic index of which is 74·5, and the others are either dolichocephalic, or belong to women and children, or afford a most pronounced asymmetry, and cannot thus give reliable figures.

UNIVERSITY AND EDUCATIONAL INTELLIGENCE

THE *alumni* and other friends of the University of St. Andrews have been roused to action by the threat (now withdrawn) of its possible dissolution, in consequence of insufficient endowment. An "Appeal" which has just been issued shows that 2700*l*. (in sums of from 100*l*. to 1000*l*.) has been already subscribed towards the better endowment of the Professorial Chairs; and a scheme has been set on foot amongst the younger graduates for the no less essential object of securing the augmentation of the open bursaries. Upwards of 200*l*. (in sums of from 1*l*. to 50*l*.) has been already promised towards this special fund, and an appeal from the Committee appointed for this purpose will shortly be circulated. There is good reason to believe that the withdrawal of the obnoxious clause has been partly occasioned by the practical shape which the defence of the oldest Scottish University has thus assumed.

SCIENTIFIC SERIALS

THE *American Journal of Science*, June, 1883.—On the nature of the induration in the St. Peter's and Potsdam sandstones and in certain Archæan quartzites, in Wisconsin, by R. D. Irving. The author extends the conclusions already arrived at by Sorby in several important respects.—On the existence of a deposit in North-Eastern Montana and North-Western Dakota, that is possibly equivalent with the Green River group, by Charles A. White. The paper embodies a detailed description of the new extinct genus and species of Percidæ occurring in the Dakota rocks, by Prof. E. D. Cope.—On the peculiar concretions occurring in meteoric irons, by J. Lawrence Smith. These concretions are found to contain sulphuret of iron, schreibersite (phosphuret of iron and nickel), graphite, daubréelite, chromite, lawrencite, aragonite.—On mineral vein formation now in progress at Steamboat Springs compared with the same at Sulphur Bank, by Joseph Le Conte.—Observations on the transit of Venus, December 6, 1882, at the Vanderbilt University Observatory, Nashville, Tennessee, by Olin H. Landreth.—On the fauna found at Lime Creek, Iowa, and its relation to other geological fauna, by S. Calvin. A complete catalogue is given of the Lime Creek fauna which are compared with those of the Niagara, Cinderhook, and other Devonian rocks.—Observations on stratified drift in Delaware, by F. D. Chester.—On the western discharge of the flooded Connecticut, or that through the Farmington Valley to New Haven Bay, by James D. Dana.—Results of some experiments made to determine the variations in length of certain bars at the temperature of melting ice, by R. S. Woodward, E. S. Wheeler, A. R. Flint, and W. Voigt. The experiments are chiefly made with zinc and steel bars, and the authors found that zinc is the least reliable metal for the components of a metallic thermometer and standard of length, while steel, copper, and brass do not vary appreciably at any ordinary temperature.—On Scovillite, a new phosphate of didymium, yttrium, and other rare earths, from Salisbury, Connecticut, by George J. Brush and Samuel L. Penfield.

Journal of the Royal Microscopical Society for April, 1883, contains:—On five new Floscules, with a note on Prof. Leidy's genera, Acyclus and Dictyophora, by Dr. C. T. Hudson (Plates 3 and 4).—The President's (Prof. P. M. Duncan) address.—The action of tannin on the cilia of Infusoria, with remarks on the use of a solution of sulphurous oxide in alcohol, by H. J. Waddington.—Summary of recent literature.—Proceedings of the Society.

Journal of the Russian Chemical and Physical Society, vol. xv. fasc. 4.—On solutions, by W. Alexeyeff; being an inquiry into the mutual solutions of liquids, as depending upon temperature. The experiments carried out on aniline, amyl and isobutyl alcohols, phenol, &c., lead to the following conclusions:—The hypothesis of Person as to the liquefaction of bodies before solution is not confirmed. The solubility depends upon the molecular cohesion, and increases as this last becomes feebler. Thus, at the same temperature, more of liquid than of solid salicylic acid is dissolved. The solutions are quite different from chemical compounds, and the liquid mixtures are different from solutions.—On the specific volumes of elements in liquid compounds; second paper, by M. Shalfeyeff. The conclusions of these valuable researches are:—The compounds of the fat series are derived from the uneven-atomic carbon; and those of the

aromatic series from the even-atomic carbon. The specific volume of carbon is C = 21 in the C_nH_{2n} compounds of the former series, and C = 12 in the C_nH_{2n} compounds of the second series.—On Caucasus naphtha, by M. W. Markovnikoff and W. Ogloblin, being a thorough analysis of it.—On the identity $(\Sigma a_i x_i^2)\Sigma a_i = (\Sigma a_i x_i)^2 + \Sigma a_i a_k (x_i - x_k)^2$, and its meaning in physics, by N. Slouguinoff.—On the focal properties of diffraction-nets, by M. Merching.—On the specific properties of indiarubber, by N. Heselius. They cannot be explained by the presence of air vesicles.

To the *Bulletin of the Belgian Académie Royale des Sciences*, for 1883, part 2, M. C. Malaise sends a valuable paper on the constituent elements of the Silurian formations of Brabant. An approximate thickness of 12,000 or 14,000 feet is assigned to the various groups constituting the older schistose rocks of this province.—Ed. Dupont deals with the origin of the Belgian Carboniferous limestones; and papers are contributed by M. Terby on the aspect and positions of the great comet of 1882; by M. Chevron, on the inflammable nature of the gases liberated in the decomposition of beetroot; by Baron Kervyn de Lettenhove, on the Conference of Bayonne of 1565; by Alphonse Wauterson, on the origin and rise of the early Flemish school of painting previous to the Van Eycks.—Part 3 contains contributions by J. de Tilly, on Chasles' theorem of central axes; by Ed. Van Beneden, on some additions to the ichthyological fauna of the Belgian seaboard; by M. Genocchi, on the algebraic functions of Prymand Hermite; by Joan Bohl, on the reforms recently introduced into the commercial jurisprudence of Italy.

Rendiconti of the *Reale Istituto Lombardo di Scienze e Lettere*, April 12, 1883.—Some applications of symbolic variability to mechanical problems, by S. C. C. Formenti. This paper is concluded in the next number, April 26.—On springs, head streams, and underground currents in the Italian Alps, by Prof. T. Taramelli.—Experimental researches on the decomposition of adipose substances in water, in damp earth, damp rooms, and in water charged with 10 per cent. of ammonia, by C. A. Tamassia.—A study of microscopic organisms in sweet, salt, and mineral waters, by Prof. L. Maggi.—Remarks on the equivalence of magnetic and galvanic distributions, by Prof. E. Beltrami.—A preliminary inquiry into Zanardelli's proposed Italian penal code, by Prof. A. Buccellati.—On an unpublished letter of Francesco Maurolico, dated September 11, 1571, in connection with the battle of Lepanto, by L. De-Marchi.—On an example of realism in classic art, by Prof. J. Gentile.

April 26.—A comparative study of the aracnofauna of Abyssinia and Shoa, by Prof. Pietro Pavesi. The author determines thirty new species of spiders, for one of which (*Chiasmopes*) he establishes a new order.—On the determination of the coefficients of specific force for iron independently of Wöhler's numbers, by Prof. C. Clericetti.—Suggestions on a substitute for capital punishment in Zanardelli's new Italian penal code, by Cesare Oliva.—Remarks on banking and the cheque system introduced into the new Italian commercial code, by L. Gallavresi.

Atti of the Roman *Reale Accademia dei Lincei*, April 1.—On Finlay's comet (1882), by S. Respighi.—On the first observer of the optical illusion converting convex into concave and concave into convex surfaces, by S. Govi. The priority of discovery usually assigned either to Joblot (1718) or to Christopher Cock (1688) is here credited to Eustachio Divini (1663) on documentary evidence.—On the presence of native cinnabar and sulphide of silver in the Tolfa Hills, by S. Blaserna.

April 15.—Biographical notice of the late Bertrando Spaventa, by S. Ferri.—On the migrations of the ancient peoples of the Armenian Highlands and Asia Minor, studied in the light of the Egyptian monuments and hieroglyphical inscriptions, by S. Fiorelli.—A notice of the archæological discoveries made in various parts of Italy duriug the month of March, by S. Fiorelli.

Revue internationale des Sciences biologiques for March, 1883, contains:—On the origin of the vertebrates and the principle of the transformation of functions, by Dr. A. Dohrn.—On the excitability of plants, by Dr. Burdon Sanderson.—On dwarfs and giants, by D. L. Delbœuf.—Proceedings of the Academy of Sciences, Paris, of the Belgian Academy, and of the Academy of Amsterdam.

April, 1883, contains:—On the primordial flora, by Louis Crié.—On the origin and relation of sex, by M. Debierre.—On colour and mimicry in insects, by Dr. Hagen.—Proceedings of the Academy of Sciences, Paris, and the Academy of Sciences, Amsterdam.

SOCIETIES AND ACADEMIES

LONDON

Royal Society, May 10.—"Note on the Motor Roots of the Brachial Plexus, and on the Dilator Nerve of the Iris." By David Ferrier, M.D., LL.D., F.R.S., Professor of Forensic Medicine in King's College.

In a communication to the Royal Society (published in the *Proc. Roy. Soc.*, vol. xxxii. 1881) on the "Functional Relations of the Motor Roots of the Brachial and Lumbo-Sacral Plexuses," my colleague Prof. Gerald Yeo and myself gave an account of the results of electrical stimulation of the several motor roots of the brachial and crural plexuses in the monkey. We there described the muscular actions of the upper extremity as resulting from stimulation of the first dorsal up to the fourth cervical nerve.

The careful dissections made at our request by Mr. W. Tyrell Brooks, Demonstrator in the Physiological Laboratory, King's College, and a repetition of the stimulation experiments which I have made, have revealed an error in the enumeration of the roots of the brachial plexus which, in common with Prof. Yeo, I wish to correct. What we took for the first dorsal nerve has proved in reality to be the second dorsal. Hence the results of the experiments must be read as applying to the spinal nerves from the second dorsal to the fifth cervical respectively, instead of from the first dorsal to the fourth cervical, as stated in our paper.

The anterior division of the second dorsal nerve in the monkey apparently invariably gives a well-developed communicating branch to the first dorsal, besides giving off the second intercostal nerve and a branch to the stellate or inferior cervical ganglion of the sympathetic.

The three branches, as seen in a dissection made for me by Mr. Brooks seem pretty equal in size, and all come off from the main trunk together.

The brachial plexis in man is not usually, in text-books of anatomy, considered as deriving any of its component roots below the first dorsal. In "Quain's Anatomy" (ninth edition, p. 619), however, a branch from the second to the first dorsal is given as a variety. On this subject Dr. D. J. Cunningham has published a note in the *Journal of Anatomy and Physiology*, vol. xi. part iii., p. 539, 1877. Dr. Allen Thomson having mentioned to him that he had on one or two occasions seen such a communicating branch in man, he investigated the point, with the result of finding a communicating branch from the second to the first dorsal in twenty-seven out of thirty-seven dissections. Of the ten cases where it was not found, five were so complicated by previous interference in the dissecting-room or by pleuritic adhesions and thickenings, that they may be considered as doubtful. But, even including these, it appears that the second dorsal sends a communicating branch to the first in 73 per cent. of the cases. Hence it should be considered as more than a mere variety. If a perfect homology exists between the roots of the plexus in man and the monkey, the second dorsal root would be the one presiding over the intrinsic muscles of the hand. Presumably in those cases where it is not found, its functions are represented in the first dorsal.

Dilator Nerve of the Iris.—Prof. Yeo and I mentioned in our paper (*sup. cit.*) that in one case in which we directed special attention to the pupil, stimulation of the anterior roots from the first dorsal to the fourth cervical—in reality from the second dorsal to the fifth cervical—caused no change in the pupil, though the movements of the limb occurred with regularity.

I have since investigated this point during the course of another research on which I have been for some time engaged. I have experimented on four monkeys. The animals were thoroughly narcotised with chloroform and kept so during the whole course of the experiments. The posterior roots of the nerves under investigation were cut, and the anterior stimulated within the vertebral canal with a weak induced current from the secondary coil (distant 20 to 15 cm.) of a Du Bois Reymond's magneto-electromotor and one Daniell. As in former experiments, a large flat electrode was placed on the sacrum as a neutral point, the exciting electrode being a hooked needle, by means of which the roots could be easily insulated and separately stimulated.

In the first experiment I failed to obtain dilatation of the pupil from stimulation of the spinal roots from the second dorsal up to the fourth cervical, though the functional activity of the roots was indicated by movements of the limb. In the second I exposed the dorsal roots from the eighth up to the third in-

clusive. Though different strengths of current were tried, no change in the pupil occurred, unless when the current was so strong as to cause diffuse stimulation. In such cases both pupils would occasionally become dilated, as under sensory stimulation in general. The functional activity of the roots under investigation was shown by contraction of the thoracic muscles on the side of stimulation.

In the third experiment, however, results were obtained of such definiteness and uniformity as to indicate almost without further confirmation the origin of the dilator nerve of the iris.

In this experiment the spinal nerves were exposed from the sixth cervical to the eighth dorsal inclusive. The posterior roots were cut on the left side, and the anterior roots stimulated, while the eyes were carefully observed by two assistants—my pupils, Mr. Norvill and Mr. East. Dilatation of the left pupil occurred almost invariably on stimulation of the second dorsal root, whereas no change whatever could be perceived on stimulation of any of the other exposed roots. This was verified over and over again, and the several roots repeatedly compared with each other. The distance of the secondary coil in this experiment ranged from 20 to 18 cm.

Stronger currents not carefully insulated caused dilatation of both pupils wherever the stimulation was applied, an expression only of general sensory stimulation.

After death a careful dissection was made for me by Mr. Brooks, and the effective root, which was marked, proved to be the second dorsal. An examination with a lens showed that the fibres of the posterior root of this nerve had been completely severed.

The results of the third experiment were entirely confirmed by the fourth.

In this I exposed the spinal nerves from the seventh cervical to the fourth dorsal and cut the posterior roots on the left side.

Here again with the utmost uniformity on each stimulation of the second dorsal, the left pupil, and this one only, became widely dilated; whereas stimulation of the other roots was entirely negative in respect to the pupil.

I ascertained in this experiment that a strength of current which would suffice to excite the muscles of the limb or trunk to action would frequently fail to cause any dilatation of the pupil when applied to the second dorsal. Somewhat stronger, but yet barely perceptible on the tongue, the current at once caused the pupil to dilate. Occasionally also if the second root had been stimulated repeatedly the iris failed to respond, probably from mere exhaustion of the nerve.

Circumstances such as these would, I think, account for the absence of the pupil-reaction in my first experiment, and also in the experiment related by Prof. Yeo and myself, where the second dorsal root was really under stimulation.

The general result of these experiments is to show that in the monkey, and presumably also in man, the dilator fibres of the iris contained in the cervical sympathetic are derived from the anterior root of the second dorsal nerve.

Mathematical Society, June 14.—Prof. Henrici, F.R.S., president, in the chair.—Prof. W. Woolsey Johnson, of Annapolis, was admitted into the Society.—Prof. Cayley, F.R.S., spoke on the subject of sever invariants, and Mr. Hammond's recent discovery.—Prof. Sylvester, F.R.S. (who was very cordially welcomed), and Mr. Hammond spoke on the same subject.—Mr. Tucker (Hon. Sec.) read parts of papers by Prof. H. Lamb, on the mutual potential of two lines in space; by Mr. H. M. Jeffery, F.R.S., on bicircular quartics with collinear foci; and made a few remarks on the subject of inverse coordinate curves.

Physical Society, June 9.—Prof. Clifton in the chair.—Dr. Obach described an improved construction of the movable coil of galvanometer for determining currents and E.M.F. in absolute measure. This is a more sensitive, accurate, and powerful instrument than the old form. It is intended for accurate measurements and testing other instruments. The needle of the new form does not dip; and its vibrations are rendered dead beat by an air chamber. The secants of the inclination of the coil are the multipliers of the tangents of the deflections. The coil consists of a single solid rod or band of copper for measuring powerful currents; and on the same ring is a fine coil of German silver wire for measuring E.M.F. No shunt is required, owing to the movability of the coil. Dr. Obach gave figures showing the accuracy of the apparatus, which is very great.—Professors Ayrton and Perry read a paper on the electric resistance of water, being the result of some experiments made by them some time ago. A comparison of the galvanometer and electrometer methods of measuring this resistance was made during the experiments, the results being in favour of the latter, especially with currents of less than 6 volts. When the electrodes or platinum plates in the water were end-on, the resistance was less than when face to face. Mr. Boys thought this curious result might be due to the resistance between the surface of the plates and the water being reduced. In answer to Dr. Coffin, Prof. Ayrton stated that the plates were heated between every two experiments in the blow-pipe. Prof. G. Guthrie remarked that Kohlrausch had found ordinary distilled water to be much more conductive than pure distilled water, which was an insulator, and inquired if Prof. Ayrton chose pure water. The latter replied that as his experiments were to test the merits of the galvanometer and electrometer modes of testing, ordinary distilled water was used. Prof. Jones stated that he found it best to use alternating currents for measuring the liquid resistance of cells, and described a mercury commutator for rapidly reversing the testing current.—Prof. Ayrton then described a lecture apparatus for showing the laws of centrifugal force. A rapidly rotated arm carrying a movable weight springs from the centre of an aneroid chamber filled with mercury. This chamber is on the rotating axle, and as the centrifugal force of the arm pulls out the diaphragm, the mercury falls in the chamber and in a tube opening from it. Prof. Guthrie remarked that the apparatus would serve as a speed counter.—Prof. Perry then read a paper on the kinetic energy of rotating bodies, in which he pointed out the practical drawbacks to the "moment of inertia" calculations, and suggested the use of a new constant (termed for the nonce the "M"). This is the amount of kinetic energy possessed by a rotating body when making one revolution per minute. To find the energy for N revolutions per minute, multiply this by N^2. In the same way the "M" of a machine can be found and used.

PARIS

Academy of Sciences, June 11.—M. E. Blanchard, president, in the chair.—On some properties of a binary form of the eighth order, by F. Brioschi.—On the homogeneity of mathematical formulas, by A. Ledieu.—Four methods of separating gallium from iridium, by M. Lecoq de Boisbaudran.—Process to be adopted in observing the first radicules of the lymphatic system, and in determining whether these radicules communicate or not with the blood capillaries, by E. Sappey. The intimate union of the radicules with the bloodvessels, which had long been assumed on general grounds, is here demonstrated by actual observation.—Researches on rabies, by Paul Gibier. The points examined are (1) the manner of inoculation; (2) transmission of rabies through the mother; (3) the presence of foreign substances in the stomach of the dog in connection with the diagnosis of rabies; (4) attenuation of the virus: (5) the parasites of rabies. The author shows that the canine, like some other kinds of virus, may be attenuated by cold. That hydrophobia is due to a special parasite, although not yet scientifically demonstrated, is rendered highly probable.—Facts and results serving to determine some new properties of sulphate of iron, by M. Rohart.—On the properties of phosphoric glass (the so-called *verre de phosphate de chaux*), by M. Sidot.—M. de Quatrefages presented, on behalf of M. de Lacerda, a memoir on an organism found in the victims of yellow fever, and by him regarded as a fungus. In the accompanying plate are represented the various stages of development of this organism.—On the track of Encke's comet in the years 1871–1881, by M. Backlund.—On a mode of transformation of figures in space, by MM. J. S. and M. N. Vanecek.—On the theory of the binary form of the sixth order, by R. Perrin.—A study of continuous periodical fractions, by E. de Jonquières (continued).—On the reflection of light on the surface of disturbed fluids, by L. Lecornu.—On the variation of the capillary constant of insulating liquid surfaces, such as ether and sulphuret of carbon, in contact with water, under the action of an electromotive force, by M. Krouchkoll.—On the formation of the glycolate of bibasic soda, by M. de Forcrand.—On the hydrates of barytes, by E. J. Maumené. It is shown that barytes makes no exception to the general law of hydrates, with which the numerous results obtained by Fremy, Filhol, Deville, and others, are in harmony.—On the fermentation of bread-stuffs, by V. Marcano.—On the artificial production of barytine, cœlestine, and anhydrite, by A. Gorgeu.—On the origin and process of formation of bauxite and granular iron, by Stan. Meunier.—On respiration in rarefied air, by MM. Fraenkel and Geppert.

June 18.—M. Blanchard, president, in the chair.—A despatch from San Francisco was read announcing M. Janssen's discovery of the Fraunhofer spectrum and of the dark lines of the solar spectrum in the corona, implying the presence of cosmic matter round the sun. Five photographs were taken of the corona and circumsolar regions to a distance of 15° for intra-Mercurial planets.—A new method of determining the right ascensions and absolute declinations of the stars (continued), by M. Lœwy. —On a drawing of the great comet of 1882, executed at M. Bischoffsheim's observatory near Nice, by M. Faye.—On the movements observed in the monolithic pillars supporting the meridian of the Neuchâtel Observatory, by M. Faye. From these observations, which have been regularly recorded since the foundation of the Observatory in 1859, it appears that even the most solid parts of the earth's crust are subject to slight movements, slow, regular, and partly oscillatory; also that the variable intensity of the movements depends on the one hand on the meteorological conditions of the year, while it is connected on the other with the periodical perturbations produced in the solar photosphere.—On a system of optical telegraphy established by M. Adam between the Islands of Mauritius and Réunion, by M. Faie.—On a carbon meteorite which fell on June 30, 1880, near Nogoga, province of Entre-rios, Argentine States, by M. Daubrée.—Experimental and clinical researches on the method of producing anæsthesia in the organic affections of the encephalon, by M. Brown-Séquard.—Numerous experiments made on dogs, rabbits, &c., seem to show that the paralysis caused by an organic affection of one of the various parts of the brain depends scarcely ever, if at all, on the cause usually assigned to it, that is, the loss of function of the part destroyed. —On the determination of the fly-wheels of tool-engines, by M. X. Kretz.—On the sulphurets of phosphorus, by M. Isambert.—On a method of transformation of figures in space, by MM. J. S. and M. N. Vanecek.—On the theory of the binary form of the sixth order, by R. Perrin (continued).—On the continuous reduction of certain quadratic forms, by E. Picard.—On the magnifying power of optical instruments, by M. Monoyer.—Evaporation of sea water in the south of France, and more particularly in the Rhone delta, by M. Dieulafait. From various observations the author concludes that throughout the deltaic region, even to a distance of twelve miles inland, the mean annual evaporation of the sea water is at least 6 mm. every twenty-four hours.—On some properties of the sulphuret, selenide, and telluride of tin, by A. Ditte.—Determination of the carbonic acid of the air in the stations selected for observing the transit of Venus, by MM. A. Muntz and E. Aubin.—Volumetric quantitative analysis of sulphuret of carbon in sulphocarbonates, by E. Falières.—On the emetics of mucic acid, by D. Klein.—On the respiratory organs in the Chelonia, by L. Charbonnet Salle. —On the cellules of the follicule in the ovum, and on the nature of sexuality, by A. Sabatier. From his protracted studies of the processes of gemmation and parthenogenesis, the author concludes that in the reproductive elements there are two principles of opposed polarities, the centripetal (blastophore) and centrifugal (spermatoblast). When the two polarities are in a reciprocal state of equilibrium the cellule is in a state of *sexual neutrality*, and capable of parthenogenesis. But should the equilibrium become disturbed by the disappearance of either element through any biological change, one of the elements becomes predominant and the cellule acquires a determined sexuality, male by the elimination of the centrifugal, female by that of the centripetal element. There may thus be various degrees of sexuality, which become completely differentiated only through successive processes of elimination.—New method of discolouration of the pigment in the eye of Arthropods, by C. E. della Torre.—Observations on the movements of the ground in the Chiloë Archipelago, by Ph. Germain.

BERLIN

Physiological Society, June 1.—Prof. Kronecker reported that in a demonstration of the action of the cooling down of nerves upon their conductibility, he observed a lesser velocity of conduction of the stimulus instead of the greater velocity that he expected, and that he had found this observation confirmed by subsequent experiments. Hence the correctness of an earlier casual observation of Herr von Helmholtz, that the cooling down of a nerve diminished its conductibility, which had been denied by subsequent observers, has been vindicated; but Prof. Kronecker admits that the contrary may also be true, because frogs may present, under different conditions and at different seasons, utterly diverse phenomena. The influence of temperature on the excitability of sensory nerves, the complement of the above observation, was investigated in frogs whose spinal cord was cut through by measuring the length of time occupied by reflex movements when their legs were dipped into dilute sulphuric acid (·5 or 1 per thousand) at different temperatures. In the case of all frogs and at all active degrees of concentration of the acid, the time required for the reflex action was shortest, *i.e.* the immersed leg was quickest drawn out, when the acid was coldest—0° or +4° up to +6°—and the time required for the reflex action was on the contrary longer at the temperature of the air of the room, and longest at the highest temperature that was employed, 30° to 35°. The influence of cooling down, not the peripheral nerves, but the spinal cord itself, will be investigated in future experiments.—Prof. du Bois Reymond communicated a short notice from a letter of Prof. Babuchin's to him, which contains a fact interesting as showing the power of adaptation to their surroundings that electric fish possess. Prof. du Bois Reymond had previously called attention to the fact that the electric eels and malapterurus that live in badly-conducting fresh water show, in as far as they have accommodated themselves to this medium, a considerable development of their electric organ in length compared with the small size of its transverse diameter, whereas in the electric rays that live in sea water, which is a good conductor, the electric organ has a greater transverse development; consequently the electromotor powers of the electric organs of the electric eel and malapterurus on one side, and of the electric ray on the other, were to one another inversely as the conductibility of the surrounding media. The measurements of Humboldt and of Sachs of growing electric eels had shown that in their growth the electric organ increased proportionately more in length than in transverse diameter, which is a teleological adaptation to the badly-conducting fresh water. Now the above-mentioned note of Prof. Babuchin contained the communication that in growing electric rays the electric organ increased proportionately much more in breadth than in height; this is likewise in conformity with the adaptation to the sea water, which is a good conductor.

CONTENTS

THURSDAY, JULY 5, 1883

WILLIAM SPOTTISWOODE

ENGLISH science is still staggering under the blow it received last week in the death of the universally respected President of its leading scientific society. The world is always the poorer for the sudden withdrawal from its many activities of a man *sans peur et sans reproche*, but there is always an inner world where the loss is more keenly felt, and in this case it is the turn of the world of science to mourn one who has made her name so honoured while he has made his own so loved. It is not too much to say that the death of William Spottiswoode is felt as a personal loss by every real student of any department of natural knowledge who ever came within his influence or had the opportunity of knowing anything of the pure and earnest nature of the man. As is but natural, those who have been working along those lines of thought—and they are many—which he had made or almost made his own, will feel the loss most keenly, not merely because the so precious sympathy is gone, but because of the swift insight, valuable criticisms, and happy suggestions as to future work always so freely at the disposal of any one who would consult him either in difficulties or success.

We should however entirely fail in our duty and in our estimate of what he has done for science did we lay too great stress either upon the special work which he did himself or that which he in a greater or less degree influenced in the manner we have just indicated. How much he has personally done we stated some little time ago, little thinking, alas! that what we gave as the results he had achieved and the honours which had followed upon them was anything more than an earnest of what was to follow. It has proved to be the full tale, but it is still one which places him high in the ranks of scientific workers. But, as we have said, high as his place in science would be from this point of view, we doubt whether it is on that that the greatest stress must be laid.

Some men of science of first class working power are so constituted that the less interest they take in the general conduct of affairs connected with science or scientific bodies the better. A man of this kind helps the affairs on very little and he loses his own time. Spottiswoode was exactly the opposite of such a man. In council every word he uttered was pure gold, and when we remember that it is now twenty-two years since he began his council work as Treasurer of the British Association, and that it has never been interrupted till the time of his death, we get an idea of his influence on our national scientific activity. No effort was too great for him, no time spent too long, no margin of time too short, if anything worth doing had to be done; the personal force and the personal example were both there; dullards became enthusiasts if doing was in question, while enthusiasts were checked at times when action was impolitic or premature.

It can easily be imagined that so cultured a man with such qualities as those to which we have referred was a large figure in other than scientific activities; and that both on the ground of his own personal merit, and as representing the Royal Society as its President, he was a marked figure in our English society.

Hence it is that the movement in consequence of which his remains are being buried in Westminster Abbey to-day was one not at all confined to the scientific world, nor was the claim embodied in the memorial to the Dean of Westminster made simply on scientific grounds. As remarked in the *Times*, "no more distinguished body of men, none more thoroughly representative of the community, ever united for a similar object." When we consider that their names were obtained within two days, the quickness of the sympathy and the unanimity of the feeling indicated among the most prominent and gifted sections of our society were certainly remarkable.

The Dean's letter granting the prayer of the memorialists is one again which does such honour to Spottiswoode that we give it in this place:—

"I am deeply sensible of the loss which the country has sustained in the death of the President of the Royal Society. The names appended to the weighty memorial which you have just laid before me are sufficient evidence of the widespread desire that the highest public honours should be paid to the memory of one whose peculiar claims have been urged so forcibly. In addition to that memorial, I have this morning received one expressing the same desire, and bearing the signatures of many hundreds of working men, with whom he was brought in daily intercourse. Although in consideration of the limited space yet remaining for interment within the Abbey I should have myself suggested a monument rather than a grave, yet I cannot but assent, after much anxious consideration, to the wish that your memorial expresses. I recognise in the late Mr. Spottiswoode, not merely a man of special scientific attainments, but one who from his interest in and sympathy with all the many branches and departments of scientific knowledge was peculiarly fitted to represent English science in its widest aspect, and who was at the moment of his death the chosen and the honoured President of the Royal Society. I recognise in him also a man of the very highest and most stainless character—one whose great gifts were only equalled by the purity and attractiveness, and, I may be allowed to add, the devoutness and humility, of his daily life. And, not least of all, I feel that in honouring him we are not only honouring one whose name is dear to men of science and of literature, and of eminence in every sphere of public and of social life, but one whose memory will long be treasured by the working classes, to whose highest interests and welfare he was so deeply devoted."

William Spottiswoode then is buried in Westminster Abbey to-day, by the side of his ancestor, an Archbishop of St. Andrews'; and his remains will be followed to the grave by representatives of the scientific bodies and other interests with which he was connected; nor will sympathy for the widow be wanting to fill up the cup of sadness. English science sorrows, and will long sorrow for the heavy loss, but still she is the richer for Spottiswoode's life and work, not least because his life was so good and so pure, and because, as President of the Royal Society, he has set an example which whoever succeeds him will be proud to follow.

It must not be forgotten that the Presidency of the Royal Society is the highest honour which it is in the power of the Fellows of that Society to bestow. How worthily and how well it was bestowed in the case of Spottiswoode is patent to all. A great responsibility,

therefore, now rests upon them, for he upon whom their choice falls will not be merely the representative of English science in London, he will represent it on the Continent and in America; the choice must bear the criticism of scientific men in other lands. EDITOR

SIR EDWARD SABINE

SPOTTISWOODE, round whose grave in Westminster Abbey so many men, great in so many ways, have stood to-day, is not the only President of the Royal Society, and not the only man of science whose loss we have to deplore. While one, however, was cut off in the full tide of his life, and while there seemed to be a rich promise of many years of valuable work in store, the other had far outlived his working powers, and by many years exceeded those of his activity.

A reference to the life-work of Sabine will clearly show how justly his high position and reputation were accorded to him, how nobly he has worked in the cause of science, and how imperishable a record of his life remains in the existence of a whole branch of scientific research, the foundation of which was mainly due to his untiring industry.

Coming of an old family said to be of Italian origin, which early settled in Normandy, and removed thence to our own country, Edward Sabine was born in Dublin on October 14, 1788, being the son of Mr. Joseph Sabine of Tewin. He received his early education at the Royal Military Colleges of Marlow and Woolwich, obtaining a commission as second lieutenant when but fifteen years of age, and receiving his captaincy eleven years later.

Very early in life indeed, his interest became centred in physical science, and especially in magnetism, the study of which he pursued with indefatigable zeal and marked success. The result of his work in this and other fields is to be found in the many papers which issued from his pen. In 1818, six years before Spottiswoode was born, he was elected a member of the Royal Society, and in the same year was appointed astronomer to the expedition under the command of Sir John Ross which left England in search of the North-west Passage. The careful observations which he made whilst with the expedition were of great value. His published papers begin from this date, commencing with a contribution to the *Transactions of the Linnean Society*, on the birds of Greenland, the result of observations made during the voyage; they range from that date down to the year 1872, thus extending over a period of no less than fifty-four years.

During this long period of active work he contributed to the *Transactions* and *Proceedings* of various societies and contemporary magazines upwards of one hundred papers, some of great length and many of considerable value and importance. Although a large number of these deals with the subject of terrestrial magnetism, many other branches of science are included in them, the voluminous nature of his published works being not less remarkable than the wide fields of study over which they range.

A considerable number are to be found in the *Philosophical Transactions*, to which he contributed upwards of forty. To the *Proceedings of the Royal Society* he made numerous contributions during his long association with it; in the *Quarterly Journal of Science* he published twelve papers, in the *Reports of the British Association* we find ten, to the *Philosophical Magazine* he made eight contributions, the remainder of his published works being scattered among the *Edinburgh Journal of Science*, *Journal of the Geographical Society*, the *Proceedings* of one or two foreign societies, and the pages of foreign scientific magazines.

As we have already said, his scientific contributions date from his voyage to the Arctic regions with Sir John Ross in 1818. Next year he again went to the Arctic regions, this time with an expedition under the command of Sir Edward Parry. As the result of his observations there, he made two communications to the Royal Society, published in the *Philosophical Transactions*, dealing, the one with the irregularities observed in the direction of the compass needle consequent upon the attraction of the iron of the ships, the other with the variations of the magnetic needle, and the intensity of the magnetic force during the voyage, and calling attention for the first time to the extreme importance of founding a widely extended series of observations of those strange magnetical disturbances, the origin of which is still mysterious. With this object in view he left England two years later on a long voyage in H.M.S. *Pheasant*, making numerous observations and bringing many new facts to light. At the same time at several equatorial stations on the coasts of Africa and America he made observations with regard to the swinging of the pendulum, with the object of determining the true figure of the earth, publishing the results in the *Philosophical Transactions*. When on the American coast during this voyage he took up amongst other subjects the question of deep-sea temperatures, and in the *Philosophical Transactions* for 1823, he at that early period published a paper on the temperature at great depths in the Caribbean Sea, whilst in the same year his busy pen was giving an account of the barometrical measurement of the height of the Sugarloaf Mountain at Sierra Leone, and the Pico Ruivo in the Island of Madeira. Three years later he published in the *Quarterly Journal of Science* an account of the ocean currents met by H.M.S. *Pheasant* during the voyage from Sierra Leone to Bahia, and thence to New York, in which he records that the Amazon stream was crossed at a distance of 300 miles from the mouth of the river. In this year (1823) he proceeded on another voyage, going this time in H.M.S. *Griper* to Norway, Greenland, and Spitzbergen, to continue his magnetical observations, and to extend the series of pendulum experiments. Whilst at the latter place he again took up the question of barometrical measurement of heights, publishing in the *Philosophical Transactions* for 1824 a comparison of that method of measurement with the trigonometrical determinations. Then in the *Edinburgh Journal of Science* in 1825 he dealt with the presence of the Gulf Stream on the coasts of Europe as determined by his observations in the year 1822, and proceeded to discuss the question of depression over the region occupied by the Stream.

In 1826 an account of his magnetical observations at Spitzbergen appeared in *Poggendorff's Annalen*.

Continuing his pendulum swingings in 1827, he set about determining by direct observation the difference in

the lengths of the seconds pendulum at Paris and London. The results of these experiments were published in a paper of some length which appeared in the *Philosophical Transactions* for 1828. At the same time he also experimented with the object of ascertaining the ratio of the magnetic forces acting on a needle horizontally suspended in London and in Paris. In 1829, in the *Philosophical Transactions*, he wrote on the reduction to a vacuum of the vibrations of an invariable pendulum; and in the *Quarterly Journal of Science* for the same year he gave an account of experiments concerning the force of the earth's magnetism, and on the then recent magnetical observations in Siberia of M. Hanstein. In the *Philosophical Transactions* for 1831 he describes some experiments made with the object of determining the length of the seconds pendulum at Greenwich.

For many years from this date he worked mainly at that science on which he had most deeply set his mark, that of terrestrial magnetism. In 1835, in conjunction with Lloyd, Humphrey, and J. C. Ross, he contributed to the *Reports of the British Association* (of which he was an early and active member, filling the post of General Secretary for twenty-one years) an account of the terrestrial magnetic force in Ireland. In the following year he himself published in the *Reports* of that Association an account of the magnetic force in Scotland. As an indication of his range of subjects we may here remark that at this time he published in *Froriep Notizen* a paper concerning the volcanoes of the Sandwich Islands. Then in 1837 we find him again contributing to the *British Association Reports*, this time a paper on magnetic intensity, dealing with the variations it exhibits at different parts of the earth's surface. He also wrote on the same subject two years later in *Froriep Notizen*, *L'Institut*, and *Quetelet's Mathematical Correspondence*. In 1838 a memoir on the magnetic isoclinal and isodynamic lines in the British Isles appeared in the *British Association Reports*, being prepared from observations made by Prof. H. Lloyd, J. Phillips, R. W. Fox, Capt. J. C. Ross, and the indefatigable Sabine. In 1840 he continued his papers on terrestrial magnetism in the *Philosophical Transactions*, now taking for his subject the consideration of lines of equal inclination and intensity in the Atlantic Ocean, and on lines of magnetic intensity between the Cape of Good Hope and Australia. He added to this series in the following year by contributing an account of the observations made by Capt. Belcher on the west coast of America and adjacent islands, and the new determination of magnetic elements at Otaheite. Writing in 1838 Sabine had so conclusively demonstrated the importance of magnetical observations being made in every part of the globe, that Capt. James Ross was sent with the *Erebus* and *Terror* to make a magnetical survey of the Antarctic regions. Sabine of course accompanied the expedition. In extension of the work of the magnetic observatory which he had established in England, and which was carried on entirely by his influence, Sabine had induced the authorities to promote the establishment of observatories in the colonies. On the voyage out, therefore, not only were numerous observations made, but magnetical and meteorological observatories were founded at St. Helena, the Cape, and Van Diemen's Land, thus permitting a great increase in the number of possible observations, and a consequent more rapid advance of the science which Sabine had so much at heart. These observatories —to our disgrace be it said, some have now been abolished —were placed under the superintendence of Sabine, and at this period a general magnetic survey of the globe was commenced by him under the direction of the Admiralty, although from what has gone before it is easy to see that the initiative of such a gigantic task had come from himself.

In 1842 he yet further added to his contributions to terrestrial magnetism, publishing in the *Philosophical Transactions* an account of observations made during the voyage of the *Erebus* and *Terror* from England to the Cape, and from thence to Kerguelen Island. Then in 1843 he wrote concerning the extension of these observations from Kerguelen Island to Van Diemen's Island, giving an account also of the various observations made in the Antarctic circle itself during the summer of 1840 and 1841, adding in the year following (1844) an account of the observations from June, 1841, to August, 1842, in the same region. In 1844 and 1845 he made contributions to the *British Association Reports* concerning the meteorology of Toronto and Bombay. During 1846 he again made contributions to meteorological literature, discussing the winter storms of the United States, and the cause of the mild winters which occur sometimes in our own country.

With reference to the survey of the globe to which we have referred, we find him next giving an account of a magnetic survey of a considerable portion of the North American continent, and of the southern hemisphere between the meridian of 0° and 125° east, and parallels of − 20° and − 70°. In 1849, in another contribution, he gave a map of the magnetic declination for 1840 in the Atlantic Ocean, between the parallels of 60° N. and 60° S. latitude. In this year it was that Humboldt's *Cosmos*, for the author of which Sabine had a profound admiration, began to be issued in England, being translated by Mrs. Sabine, and edited by her husband, it being completed in 1858. In the year following he became vice-president of the Royal Society, with which he had been so long connected.

The colonial observatories were, as we have said, under the control of Sabine, and remained so for many years. In 1851 and 1852, and again in 1856, he continued his papers on the magnetism of the earth.

It had been observed (first by Lamont) that the mean of the larger magnetic disturbances gave signs of being bound by some law, and of having a definite but long-period variation. Previously to this it had been shown by Schwabe that the number of spots on the surface of the sun increased and decreased in obedience to regular law, the cycle occupying nearly eleven years for its completion. The results of the observations at the colonial observatories led Sabine to the discovery that magnetical disturbances were intimately bound up with this solar spot period; that the connection between them was of such a nature, that a year of large declination coincided with a year of maximum sunspots, whereas those years when the range in declination was small corresponded with years when there were but few spots on the sun. In the same year the same fact was independently determined by Dr. Rudolf Wolf and M. Gautier.

In 1853, at the meeting of the British Association at Belfast, Sabine occupied the presidential chair. In this year he turned to a consideration of the moon's influence on terrestrial magnetism, writing concerning the effect of that body on the magnetic declination at Toronto, St. Helena, and Hobarton ; and taking up the subject again in 1856, he then discussed the lunar diurnal variation at Toronto. At a later period, in the *Proceedings of the Royal Society*, he contributed a paper on the lunar diurnal magnetic declination obtained from the Kew photograms. In 1857 he made another contribution to the *British Association Reports*, discussing the amount and frequency of the magnetic disturbances and of the aurora at Point Barrow, on the shores of the Polar Sea. In the *Philosophical Transactions* for the same year he discussed the question of the existence of the decennial period in the solar diurnal magnetic variations and its non-existence in the lunar diurnal variation of the declination at Hobarton, as M. Kreil seemed to think was the case. He then stated, as the result of a re-examination of the question by the light thrown upon it by the Hobarton observations, that he was as entirely convinced of the existence of this period in the former case as he was convinced of its non-existence in the latter.

Continuing the investigation of this subject, he contributed to the *Royal Society Proceedings* for 1859-60 a paper on the solar diurnal variation of the declination at Pekin. In the same volume of the *Royal Society Proceedings* he also wrote concerning the laws of the phenomena of the larger disturbances of the magnetical declination at Kew Observatory. In 1861, at the request of the General Committee of the British Association, he prepared a report on the repetition of the magnetic survey of England. In this year he succeeded Sir Benjamin Brodie in the presidency of the Royal Society, which position he occupied for the next ten years. In the *Philosophical Magazine* for 1862 he entered into a discussion concerning the cosmical origin of terrestrial magnetism. Two years later, both in the *Philosophical Magazine* and the *Proceedings of the Royal Society*, he published a comparison of the most notable disturbances of the declination at Kew and Nertschinsk during 1858 and 1859. During the next few years, notably in 1866 and 1871, records of the magnetical observations at Kew were published by him. The chief work, however, of this period of his life consisted in concluding his contributions to the *Philosophical Transactions* by reports and reductions of the work done during the Antarctic expedition. In a lengthy contribution in 1866 he resumed the discussion and co-ordination of the various observations, continuing and concluding this in another paper, which is to be found in the *Transactions* for 1868. His last contribution appeared in 1872, when he gave a magnetical survey of the North Polar regions to serve as a companion to the survey of the South Polar regions which had already appeared. It was his earnest wish that he might be spared to complete this, but the infirmities of age were then stealing over him, and it is doubtful whether it would ever have appeared had it not been for the able assistance of Captain, now Sir Frederic Evans, the Hydrographer of the Admiralty, assistance which the author gracefully acknowledges in a postscript to the memoir.

From this date the work of Sabine may be said to have ceased. He had resigned the presidency of the Royal Society the previous year, and he now sought to spend the evening of his life in that retirement and rest to which his advanced age and great works so fairly gave him a claim. He had received the Copley Medal of the Royal Society in 1821, and the Royal Medal of the same society in 1849. In 1869 he was made K.C.B. He possessed also the Prussian Order *pour le mérite*, and was either an honorary or corresponding member of many foreign societies. We mention these facts to show that he retired from his active life full of well-earned honours. In 1879 he lost his wife, who for more than half a century was the close companion of his labours. In the history of the Royal Society his name will ever be valued as that of one who, both as member and as President, was ever foremost in guarding its honour and maintaining its dignity, whilst the kindness and courtesy which as President he displayed to all, not excluding the younger members, will be always gratefully remembered.

It is chiefly by his pendulum observations and by his magnetic determinations and reductions that, as may be gathered from what has been said, his name is so well known in science. The degree of accordance which some of the early determinations of the former kind exhibited was so much in advance of what was at that time thought likely, that they were received with incredulity in some quarters. The discussion which Sir George Airy made long ago, in his article on the figure of the earth, published in the "Encyclopædia Metropolitana," of the pendulum observations then available for that purpose, shows how large a share belonged to the labours of Sir Edward (then Captain) Sabine.

His own magnetic observations were marked by his wonted accuracy; and his discussion of the results obtained at the colonial magnetic observatories led to new and unexpected results. The most striking, perhaps, of these was the discovery of the relation between magnetic perturbations and the more or less spotted condition of the sun's surface, to which we have already referred. Dissimilar as are these phenomena, and difficult as it then at least was to imagine any possible cause for a connection between them, subsequent observations have fully confirmed the conclusion at which he arrived, that connected they are, though what the precise nature of the connection may be is still a matter of discussion.

Though from the nature of the case the work was one of compilation rather than of original observation, his determination of the magnetic state of the earth at a particular epoch, with its accompanying maps of the isoclinal, isogonal, and isodynamic lines was most noteworthy. The search for the original authorities and the application of the corrections requisite to render the observed results comparable with one another occupied a long time, and the results, as we have pointed out, appeared in instalments, as the various regions into which as a matter of convenience the earth's surface was divided were successively completed.

The establishment of the colonial observatories, too, was the direct result of his exertions; and his name will go down to posterity as that of the man who more than any other laboured for the proper establishment of the science of terrestrial magnetism, interesting and important

in its scientific aspect, and pregnant with so many benefits to mankind at large.

He was buried on Saturday, his remains being placed beside those of his wife in the family vault at Tewin; the funeral, in accordance with his own wish, being of the simplest character. In addition to the members of his family and private friends, the funeral was attended by the Secretary and Treasurer of the Royal Society, the Hydrographer to the Admiralty, and representatives of the other Government services with which he had been so long connected.

A MINISTER OF PUBLIC INSTRUCTION

WE are a longsuffering, patient people. The call of Luther to those around him to educate their children and make men of them, as well as provide them with arms—a call at once answered in Germany—is only just now being answered among ourselves.

One of the most beautiful and one of the most touching sights in London now, and one which in our view is a standing disgrace to the politicians who have held sway during the last hundred years, is the gradual rising above dingy roofs and millions of chimneys of the red brick Board schools. The children in London at all events are now being educated, and our future masters are receiving the first rudiments of their instruction, and this much more on account of the intention of their fathers to have it for them, than on account of any farseeing policy of those who are popularly supposed to look in any and every direction for anything that may conduce to the well-being of our country. We have at last got a public instruction, and it is already in the air that that instruction will in time be as free as it is now compulsory. It is a heartbreaking thing to look back and think what might have been had these all too recently built schools overtopped the squalid dwellings of the poor a century ago. How much less squalid those dwellings would be now. The monumental and extensive prisons would probably be less occupied in their every cell than they are now, but the well-being of the country, the output of the country would have been greater, and the struggle with penury, and dirt, and crime would have been less.

This is only one aspect of education, but yet it seems that in this country at all events it is the mainspring of public opinion with regard to the general question. The cry—on many grounds the mistaken cry—for technical instruction has grown from the work of the Board schools, it has gone along the same line at a higher level, and it will go on still further. The enormous development of the Government Science and Art Classes will also go on, and to the credit of the late Sir Henry Cole be it said here that he was wiser than the politicians, and his clear sight and singlemindedness influenced the head of the department with which he was connected, so that the quiet, slow work in science and art began in 1851, long before the present notions of the importance of education really began to take root in our land.

Now that compulsory education is in our midst, now that the importance of science and of art to the national industries is universally acknowledged, now that it is recognised that the education of our workmen must no longer be so disgracefully neglected as it has been, it is again suggested that there should be a Minister to look after these matters.

Ten years ago, as it was well put, the Kinderpest was the care of the Government side by side with the Rinderpest. Both were practically on the same level, both were acknowledged to be nuisances, both might require a public department to look after them, and then money would have to be spent. This was quite a sufficient argument with "statesmen" to let things go on in the old harum-scarum way; for the policy of a Government is to keep money in its purse, honestly if it can, but in any case to do so, as if England were a miser, acknowledging no responsibilities, spurning all delights, and wishing to live a sordid life like the burghers, caring only for their dykes and pikes, whom Luther shamed out of their indifference centuries ago.

There has again, this week, been a suggestion made that there should be a Minister of Public Instruction, who should be responsible for the preparedness of the country in this respect, just as the Minister of War is responsible for the preparedness of it in another direction. Sir John Lubbock must be congratulated upon the way in which he brought the motion forward last Friday. It was a mild, pleading story. As long ago as 1856, he pointed out, the late Lord Derby said:—

"It appeared to him well worthy of consideration whether it would not be well to have a Minister, or the head of a department, who should have no other duties to perform, and who should be, in fact, responsible for the education of the people. . . . He had a strong feeling that the institution of a Minister of Instruction was desirable, that the subject should be altogether separated from the Privy Council."

But that did no good. In 1862 there was another resolution put to the House calling on it to affirm that for the education estimates and for the expenditure of all moneys for the promotion of education, science, and art a Minister of the Crown should be responsible to the House. That also did no good. In 1865 a Select Committee was moved for to inquire into the constitution of the Committee of the Council on Education. It was then urged that education and science and art were beginning to be considered of such importance that—

"The great duty of superintending the various branches connected with the Department of Education should be intrusted to some one responsible Minister, some Minister who should be regarded as a State officer of high authority who should have the sole conduct of that department, and be solely responsible."

And that was shelved.

Nine years later, in 1874, the same view was urged, and the present Prime Minister then admitted "that there was much to be said in favour of the general principle that the expenditure of money for the promotion of education in science and in art should be placed under the control of a single responsible Minister." It is true he said this, but he supported the previous question, so that again came to nothing.

Now that education and science are the great things of the day, not only in this but in all countries, England enjoys the proud preeminence of being the only country—civilised country, we know nothing of Timbuctoo—in which there is not a Minister of Public Instruction. It is lamentable, terrible, to read the debate of last Friday,

and to see the way in which the question was discussed. Mr. Gladstone was impressed by the condition of the House at nine o'clock, but it does not appear that he was impressed with anything else; the importance of education, the importance of science, the importance of art, the daily, almost hourly, increasing importance of these things does not seem to have entered into the question. To a large extent it was merely a question of Cabinet convenience and Parliamentary tweedledum and tweedledee. How can there be made room in the Cabinet for a Minister of Public Instruction? Are not the affairs of the Duchy of Lancaster of much greater importance, and would not the recognition of the importance of education make the Cabinet unwieldy and give rise to difficulties in Parliamentary procedure? And then there is the Scotch business that must be looked after first, and so on, and so on. Education is evidently not in the region of practical politics.

Heaven knows changes sufficiently great have been made of late years, and it is not absolutely certain that the fundamental bearings of the nature of the changes to be made have in all cases been fully considered; but it seems as though they are to be most carefully considered before any change is made touching the matter of education.

Still it is acknowledged that the question is, after all, one that deserves the attention of Parliament, but Mr. Gladstone had, as usual, three objections to make. In the first place he expressed very great doubt whether, if he had a plan ready to alter the present arrangement, it would be wise to make any declaration on the subject by way of motion. Secondly, he admitted that there was no plan, and he did not think the time had arrived for one; and lastly, he considered that the subject ought to be a great deal more examined before the House committed itself to a final opinion whether there should be a plan or not.

With reference to his first objection he stated that the House knew perfectly well that administrative changes are made piecemeal, and must continue to be so; and he remarked that there was a good deal to be said in favour of what was called a patched house, because most of us found it the most comfortable sort of house to live in. A Minister of Public Instruction would be a new patch. and as there is patching going on elsewhere he objects to this; and so on and so on.

The argument which he used in favour of the second objection was, we imagine, the strongest he could have used against it, namely, that the business of the Council Office in respect to education has been in an almost incessant state of flux and change. Of this there can be no doubt that the flux and change will get more pronounced as time goes on. That is the very reason why everything should be brought to a focus.

We may gather from Mr. Gladstone's speech that the Universities should ever, in his opinion, remain divorced from the general question of education; but if so, what is to become of Prof. Huxley's ladder from the gutter to the university? We think, too, if Mr. Gladstone had been fully informed on the subject he could have urged as an additional objection that a great many questions referring to education are never now touched by the Education Department at all.

Several of the speeches might, if we had more space at our disposal, be noticed at some length. Still, we think it worth while to cull the following from the speech of Mr. Forster, an old Vice-President of the Committee of the Council on Education:—

"The Committee of the Council for Trade, or Agriculture, or Education meant nothing whatever. Persons might imagine that the Privy Council occasionally met for the transaction of business, but they never did so either in England or Ireland. The Minister for Agriculture was the President of the Committee of the Council on Agriculture, but he greatly doubted whether that Committee ever met, or ever would meet. . . . The real objection (to Sir John Lubbock's proposal) probably was that it was undesirable to make too much of education, that if we were to have a Minister of Education he might be pushing things on too quickly. . . . There might be a fear that under one Minister too much money would be spent. . . . What was complained of now was that there was no really defined responsibility. The man who moved the estimates and did the work was not the head of a department, and he ought to be. The work was done by a Minister who was controlled by another, and the latter was scarcely seen by the public. He did not see why we should continue that Japanese mode of managing affairs."

It is satisfactory to see that the House of Commons is gradually getting into a better position to discuss such questions as these, but we have felt that the main point is, that the head of the Government does not yet consider that the question of education is one of an importance sufficient to be discussed side by side with what in his opinion is the much larger questions of Parliamentary procedure, and the saving of so many pounds, shillings, and pence. It is true a Select Committee has been agreed to, but we fear that after Mr. Gladstone's speech very little will come of it, as has happened before.

It would be ungraceful not to state that the debate brought out in the clearest possible way the valuable services rendered under great difficulties by the present Vice-President of the Committee of the Council on Education, Mr. Mundella.

But the result remains that we are not to have a Minister of Education. There is agricultural business, including the Rinderpest, and other matters, and these are larger questions than that of national education! Therefore national education must wait. As we said before, we are a longsuffering and patient people. There is, however, little doubt that in some political programme of the future this question will find a place; equal electoral districts and the payment of members are not the only things to be cared for. F.R.S.

EVOLUTION AND CREATION

A Few Words on Evolution and Creation; A Thesis maintaining that the World was not made of Matter by the Development of one Potency, but by that of Innumerable Specific Powers. By Henry S. Boase, M.D., F.R.S., &c. (London: John Leng and Co., 1882.)

Notes on Evolution and Christianity. By J. F. Yorke. (London: Kegan Paul, Trench, and Co., 1882.)

THE first of these works is, as may be inferred from its title, a most curious production. The chief aim of its author is that of sustaining the Biblical Cosmology against what he regards as the fallacious inroads of the theory of Evolution. In carrying out his design he

devotes the first part of his book to a general criticism of the Evolution theory, and the second part to a consideration of the first chapters of Genesis, which he regards as justifying his view that the world "was made by the development of innumerable specific powers." Our readers must not suppose from this form of expression that Mr. Boase seeks to develop a system of Polytheism; on the contrary he is a Monotheist of the most orthodox type, and by his "innumerable specific powers" means only the properties with which matter has been endowed by its Creator. This, at least, is the only meaning which we have found ourselves able, after a somewhat hasty perusal of his book, to attach to this term, which constitutes the core of his "thesis." But if this is his meaning we fail to appreciate the speculative importance which he somewhat ostentatiously attaches to his opinions. For the great distinction which he draws between these opinions and those which are held by evolutionists consists, as he says, in their making "no assumption of an unknown matter endowed with an imaginary all-becoming potency." But so far as physical causation is concerned the two statements amount to exactly the same thing; the only difference between them is the old and well-worn distinction between theism and non-theism—viz. as to whether the observed "potencies" of matter are or are not God-endowed. We cannot see that Mr. Boase has contributed anything new to this question, and therefore regard his work as lost labour. There is a simplicity about some of his remarks which appeals to us as almost pathetic. For instance:—"From my point of view, the occurrence of some of the same kinds of organisms in the rocks of adjoining formations may arise from the remains of the older rocks being transported into the newer formations, or from the older organisms being created anew as a part of the more recent series. . . . It may here be noticed that such an alternate destruction and reproduction of living creatures is set forth in the civ. Psalm—'Thou takest away their breath, they die, and return to their dust. Thou sendest forth Thy spirit, they are created; and Thou renewest the face of the earth.' This verbal coincidence is curious, but, of course, cannot be adduced to prove that the doctrine of Creation and science are in accord with one another." If the "point of view" in question is to be thus calmly attributed to "science," the concluding sentence of this passage is one of the very few in the book with which we are able cordially to agree.

Again, speaking of the creation of Eve, our author remarks:—"It may also have served as an occasion for the important lesson, that the Lord God was the Creator of these living creatures (animals), for immediately after this, God gave Adam a practical proof of His power to create a living being." Practical proof, no doubt, but we should have thought almost more startling than could have been justified for the purpose suggested. Seriously, however, the absurdity which such passages as these display might be amusing from the mouth of a street-preacher; from a man of cultivation they are, as we have said, pathetic.

The other work which we have to notice stands in every way at the opposite pole of thinking from the one which we have just considered. For the object of Mr. Yorke is to show that the principles of evolution are alone sufficient, without any hypothesis of supernaturalism, to explain the origin and development of Christianity. The book is, therefore, mainly of an historical character, and although its views cannot fail to be obnoxious to orthodox opinion, the temperate manner in which they are stated ought everywhere to commend the approval of good taste. Moreover, whatever his readers may severally be inclined to think of his arguments, they can scarcely fail to agree that Mr. Yorke has written a highly interesting book. His object being, as already stated, to trace in the antecedents of Christianity the natural causes of its rise and progress, he has given a selection of quotations from the Jewish writings about the time or shortly before the commencement of the Christian era, and also of the Buddhistic writings long before it, in order that a just estimate may be formed of the extent to which the world is indebted to Christ as a moral reformer. In our opinion Mr. Yorke has shown a sound critical judgment in making this estimate. On the one hand, he is careful to sift out all the elements of the moral teaching which were, so to speak, in the air at the time when Christ taught; and, on the other hand, he is equally careful to distinguish the points wherein the "originality of Christ" was shown. Here we meet with what appears to us a more full appreciation of this "originality" than is shown by most of the other and some of the more eminent writers of the same school.

We shall conclude this notice by quoting two brief passages, one to show the high development of moral feeling which obtained among the Jews immediately anterior to the teaching of Christ, and the other to show the degradation of moral feeling which now obtains in the Roman Catholic ministry of the Christian Church.

"Wear mourning for the Egyptians, suppress the prayer of glorification on the seventh day of the Passover. It is the anniversary of the day when your enemies perished in the Red Sea, and God desires not to be glorified because his creatures have been drowned beneath the waves."

In painful contrast to the singular beauty of this passage, our other quotation is selected from several pages in the same strain which are republished by Mr. Yorke from two pamphlets written expressly for children by a Reverend Father, whose name is, with a singular appropriateness, Mr. J. Furniss. As Mr. Yorke remarks, the Rev. Father Furniss evidently feels that in these degenerate days "Hell is not pictured vividly enough for purposes of practical terrorism, and has accordingly done his imaginative best to supply this great want. And he deserves every credit for his work, for anything better calculated to drive a sensitive child mad with fright it would be impossible to conceive."

After describing the "*Dress of Fire*," and the "*Red-hot Floor*," in one of which there is represented a girl of eighteen, and in the other a girl of sixteen with the Devil taunting their agonies, "The Sight of Hell" goes on to describe—

"'*The Red-hot Oven.*'—See! it is a pitiful sight. The little child is in this red-hot oven. Hear how it screams to come out. See how it turns and twists itself about in the fire. It beats its head against the roof of the oven. It stamps its little feet upon the floor of the oven. You can see on the face of this little child what you see on the face of all in Hell—despair, desperate and horrible."

The only corrective of immoral publications of this description is to be found in reproducing them before public opinion of another kind from that of the unfortunates whose eyes alone they are intended to meet; and it is partly this consideration that has led us to review Mr. Yorke's essay, which, although excellent in itself, is hardly in close enough contact with natural science to demand notice in these pages.

GEORGE J. ROMANES

OUR BOOK SHELF

Iconographie der schalentragenden europäischen Meeresconchylien. Von Dr. W. Kobelt. 4to. Heft 1. (Cassel: Theodor Fischer, 1883.)

THE object of this work is to supply a want which is continually felt by conchologists, and it deserves the greatest success. Dr. Kobelt is well known to science as the editor of the *Jahrbucher* and *Nachrichtsblatt der deutschen Malakozoologischen Gesellschaft*, which has now been published for between fifteen and sixteen years, and as one of the editors of the new *Conchylien-Cabinet* of Martini and Chemnitz; and he is also the author of several works and papers on conchological subjects. It appears from the prospectus of the present work that its scope will be confined to the coasts of Europe, including the English Isles, the Faroes, and Scotland, and bounded by the north coast of Africa, but excluding not only tropical and subtropical species of Mollusca, but those Arctic species from Spitzbergen and the north of Iceland which are not found on the coasts of Upper Norway. This scope, although extensive, is not very definite; and it scarcely accords with our usual notion of the European seas. We do not know what may be the author's limit of depth, whether it is the line of soundings or 100 fathoms; nor whether he will even take the Mollusca now about to be published from the *Triton* cruise between the Faroes and Scotland. The expeditions of the *Josephine*, *Lightning*, *Porcupine*, *Challenger*, *Vöringen*, *Travailleur*, *Washington*, *Knight Errant*, and several others, have of late years done much to aid in the exploration of the European seas at various depths; and the number of species thereby added to the Mollusca has been very considerable and is still increasing. Some additions have likewise been made from time to time to the Mediterranean Mollusca, especially by myself during the present month. Taking into account all these discoveries, I am inclined to reckon the number of species hitherto described as inhabiting the littoral zone and moderate depths in the European seas as not less than 1000; probably 1200 would be nearer the mark.

The first part of the present work, which has now appeared, gives figures of four species only and their varieties, one of which species (*Murex gibbosus*) is Senegalese, and has never (to the best of my knowledge and belief) been found in any part of the European seas. This reduces the number of figured species to 3. Perhaps the species will not be so profusely illustrated in the next and following parts. The published prospectus does not give any idea of the extent of the work. But assuming even that twenty species (large and small) may on an average be figured in each part, the entire work would take not less than from fifty to sixty parts, and would cost for an uncoloured copy 10*l*. to 12*l*., and for a coloured copy 15*l*. to 18*l*. If all the species known to inhabit the European seas, including the abyssal and benthal zones, are to be figured—and I think this ought to be done—the extent and cost of the publication must be increased by probably a fourth more.

However, such calculations have doubtless been considered by the author or his publisher. The work will assuredly be far more scientific and valuable than the very irregular but expensive *Conchologia Iconica* of the late Mr. Reeve, and be not merely an "ouvrage de luxe."

The family *Muricidæ*, which is the first selected for publication, does not seem to be placed in the usual order of classification. All the figures are admirable. The descriptions are in Latin, the text in German. The geographical, hydrographical, and geological distribution, as well as the odontophore and synonymy, are carefully worked out.

J. GWYN JEFFREYS

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications.*

[*The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to insure the appearance even of communications containing interesting and novel facts.*]

Sand

I HAVE recently been favoured with a reprint of Mr. J. G. Waller's paper upon sand, read before the Quekett Microscopical Society. The subject is so full of interest that I trust I may be allowed to give it a wider publicity in your columns. To render the study of practical use to geologists and physicists, the first step appears to me to ascertain whether it is possible to distinguish with certainty, by aid of the microscope, sand that has been worn by action of wind from sand that has been for long exposed to surf, and this again from sand brought down by torrents. The degree of rounding and the average size of the grains would be, I presume, among the chief characteristics, and it is to be hoped that naturalists abroad will kindly forward examples of undoubted blown and torrential sand, so that this point at least may be settled.

If it should prove that the origin of sand can be pronounced upon with any degree of certainty, from a microscopical examination, we should come into possession of a most valuable aid to the study of at least Tertiary geology. It is well known that marine and freshwater deposits succeed each other repeatedly throughout our Eocene formations, and where deposits of sand are in juxtaposition, it is at present impossible to draw any line between them. It is only possible to surmise that they are of different origin and therefore age, when pebble or oyster beds on the one hand, or films of clay with plant impressions on the other, are accidentally included in them. So far as our own Eocenes go, it appears from Mr. Waller's results that their sands, when of marine origin, possess a percentage of flint grains, but that purely fluviatile sands do not possess any. Marine and freshwater sands are in direct contact in very many of our Eocene sections, and I hope Mr. Waller's researches will enable us to distinguish them and apportion the proper thickness to each.

With regard to the relative rarity of flint-grains and preponderance of quartz, in all the Tertiary and recent sands hitherto examined, it appears just possible that the concussion the flint grains must undergo when beaten for ages in the surf, might induce a molecular change from the colloid to crystalline state, but in the absence of any fact or argument to support such a theory it cannot be seriously entertained. It is however possible that quartz grains reach a final state of subdivision, and then suffer relatively little by attrition, and are therefore almost indestructible, while flint grains become rapidly degraded into mud. This appears to be very much the opinion Mr. Waller has formed. It does seem at first sight matter for surprise that the grinding of flint should not more largely affect the composition of our sea sand; but we must on the other hand reflect on the indestructible nature of the quartz grains that chiefly compose it, that it may have been accumulating since palæozoic times, and the enormous bulk of the quartzose rocks that must have been ground down to supply it during such vast ages, and then compare the sources with flints which in comparison only appeared yesterday, and then but as scattered segregations in a limited portion of a single formation. Flints and flint beaches, recent and ancient, are at our gates, and are continuously renewed by the wearing away of the chalk of which so much of our part of England is composed, and their aggregate mass therefore astounds us; but they after all occur over only a

limited area, and mostly in unconsolidated beds, and it is quite probable that they would not outlast the destructive influences to which they are subjected if these were continued throughout a geological period. The coast-line occupied by flint-shingle is almost limited to portions of Western Europe, and is relatively insignificant. J. S. GARDNER

Science Club

The Great Comet *b* 1882

M. RAOUL GAUTIER, of Geneva, has recently published, in *Astronomische Nachrichten*, No. 2519, three sets of elements of this comet, calculated from a few observations before perihelion. He says that, as it is possible to represent with the same curve, either a parabola or an ellipse, the nearest observations before and after perihelion, he believes "que si la comète a subi une perturbation dans son mouvement lorsqu'elle a passé à son périhélie, cette perturbation a dû être insensible."

As I am not so far advanced with my calculations, for I have begun a thorough discussion of the movement of that comet, I do not know whether there has been or not any considerable perturbation during the passage near the sun; but can the simple fact alluded to by M. Gautier give us much information on that point?

In fact, we can easily understand that although the orbit after perihelion might be quite different from the orbit before that point, still the positions of the comet at a short distance from perihelion may be pretty well represented, within the limits of the errors of observations, by a single curve, which of course will be of second order, but which will not certainly give the calculated positions of the comet at a certain distance from perihelion agreeing with the observations. If we could prove that the orbits calculated, for instance, from observations between September 7 and 12 and between the 22nd and the 30th of the same month agree together, and give the positions of the comet immediately before and after perihelion according to the observations, then we could say that the movement of the comet was not perturbated during the passage near the sun. But this fact is not proved at all, and instead it seems that the passage through the corona has had some effect upon the movement of that remarkable comet. E. RISTORI

13, Pembridge Crescent, Bayswater, June 16

THIS comet was visible here with the naked eye up to February 28. I so saw it myself on the evening of that day. Owing partly to cloudy weather, partly to moonlight, I had not seen it for ten days or a fortnight previously, but found it on that evening with little difficulty and without any optical help. In my telescope (4-in.) it appeared, roughly, like a long, flat-sided, oval nebula, the central part of the major axis being the brightest of the whole. Two cloudy evenings intervened, and on the following night (March 3) I could not see it with the naked eye, even after finding it with the telescope and knowing exactly where to look, and though the optical condition of the air seemed the same. During April I saw it, with the same telescope, on sixteen evenings, cloudy weather and moonlight interfering on the others. In the present month (May) I saw it five times, that is, up to the 6th certainly, and I believe I saw it on the 9th, but decreasing visibility and increasing moonlight prevented verification. I have just received a somewhat larger instrument (5-in.), with which after the moon has passed I hope to see it again. A. S. ATKINSON

Nelson, N.Z., May 19

Sun Pillar seen in Jamaica

AT sunset on May 15 I saw for the first time in my life the phenomenon called the *Sun Pillar*. A few days later the mail-packet arrived from England, and in NATURE I found much correspondence on its appearance on April 6 at several places in England and Wales between Hull and St. David's.

Major Gibney's admirable description of its general appearance on April 6 (vol. xxvii. p. 605) was so fully confirmed on May 15 in Jamaica that a very brief description may here suffice.

At 6h. 30m. p.m. Kempshot mean time it appeared as a bright ray of light of a faint roseate hue, 2° in width and 30° in height above the horizon, vertical, but not passing through the sun. A rough sketch was made at the time, and the circles of the equatorial were afterwards employed to determine the azimuth of the point where the pillar cut the horizon. This was 70° from the north towards the west; and as the sun's azimuth was 69° at the same time, the pillar passed 1° to the west of the sun. In the sketch the pillar is represented as passing its own breadth to the west of the sun, but as the sun was then just below the horizon the former measure is likely to be more correct.

Now with regard to the nature of the phenomenon, it certainly was not the usual display of the zodiacal light. The light is here seen to perfection; every fine night when there is no moonlight the zodiacal light may be seen following the ecliptic from the one horizon to the other with but little variation, except perhaps as to the *gegenschein* or stronger illumination near the point in the heavens diametrically opposite to the sun. And so clearly is it seen, that some years ago I carefully measured its breadth at different distances from the sun, and so formed the following table:—

Ang. dist. from Sun. °	Breadth of Z.L. °	Ang. dist. from Sun. °	Breadth of Z.L. °
30	41·4	110	20·3
40	38·7	120	17·8
50	36·1	130	15·3
60	33·4	140	13·0
70	30·7	150	10·8
80	28·1	160	8·9
90	25·5	170	7·6
100	22·9	180	7·0

From various considerations based upon the figure corresponding to these measures I consider the zodiacal light a terrestrial phenomenon—rays of light are swept back from the sun, chiefly from the tropical parts of the earth, and tend to accumulate at the point in the heavens diametrically opposite the sun.

If there be any truth in this theory, the sun pillar may be a strong and comparatively local development of the same light; this is the only explanation I can give; the explanation given by Mr. G. J. Symons, the well known meteorologist, "that it is merely a portion of a halo passing through the sun" (vol. xxviii. p. 7), will not apply to the Jamaica observation at all; the sky was far too pure and transparent at the time, and there was not the least trace of *cirrus* cloud. MAXWELL HALL

Kempshot Observatory, Jamaica, June 7

Error in Hutton's Tables of Logarithms

AT the end of Hutton's "Mathematical Tables" (new edition, 1858, Longmans and Co., London) there is a very useful table containing the logarithms of certain constants frequently used in calculation. The tropical revolution of the earth in days is there given as 365·24226, and the logarithm of this most important constant is given as 2·5625910 instead of 2·5625810.

I would be glad to know from any of your readers whether there are any other important errors in this edition, especially among those tables of logarithms in frequent use.

Jamaica, June 4 MAXWELL HALL

Palæozoic Sclerotic Plates

IN the course of my researches among the coal shales of Northumberland I discovered two specimens of ossicular rings known as sclerotic plates. The external diameter of one ring is five-eighths of an inch, and the orbital orifice is one-quarter of an inch; this ring of sclerotic plates consists of nine bones arranged as are the eye bones of Ichthyosaurus, Plesiosaurus, and eagles, viz. in tolerably uniform segments. The second specimen is a quadrant of a ring, and consists of six plates of larger size than the other specimen. I shall be glad to learn if any of your readers have discovered similar sclerotic plates in the Palæozoic rocks of the British Isles, as specimens are not exhibited in the British Museum, Jermyn Street Museum, or Edinburgh Museum. T. P. BARKAS

Newcastle-on-Tyne, June 25

Graft-Hybridisation

ST. PAUL, in his Epistle to the Romans, says (ch. xi. v. 17), in illustration of the admission of the Gentiles to the religious privileges of the children of Israel, "If thou, being a wild olive, wert grafted in among them, and didst become partaker with them of the root of the fatness of the olive tree," &c. Olshausen, in his commentary on this epistle, says (English translation, p. 369),

"Whereas, according to the image in this place, the wild branches are ingrafted into the generous tree, reversing the usual process by which good branches are grafted into wild trees, we are informed by both ancient and modern writers that such a process is practicable in this very tree, the olive, and is often practised in the East. Compare Columella 'De Rebus Rusticis,' *v. g.*" Can this be confirmed? It seems scarcely credible. The question bears on the subject of graft-hybridisation, about which many curious facts are collected in Darwin's work on "Variation under Domestication." JOSEPH JOHN MURPHY

Old Forge, Dunmurry, Co. Antrim, June 29

Wild Duck and Railways

LAST autumn I visited Canada and made a journey to the extreme west point (then reached) of the Canada Pacific Railway, on which three or four thousand men were at the time employed laying down the rails on the prairie, at the very rapid rate of three or four miles a day, or more than twenty miles a week. There are many ponds and lakelets along the track, which abounded at that season with a variety of ducks—mallard, teal, widgeon, &c.—usually very shy, and not easily approached by the sportsman. Yet these ducks had in the short space of from two to five days become so accustomed to the noisy and (to most of the birds) novel engine moving along, that they remained sitting quietly in the water within easy shot when the train was passing. On my return journey I was sorry to find that this confidence on the part of the ducks was taken advantage of by the conductor and other wretched sportsmen (?) who shot at the poor birds from the platform of the cars whilst in motion, although when they did *kill*—I am glad to say there were more misses than hits—they could not stop to pick up the game. A sportsman, to get equally near to the ducks as they permitted the train to approach them, had to use the cover of the long grass and some artful dodging to attain his object. This quick intelligence on the part of the ducks seemed to me something remarkable, as the senses both of sight and hearing must have been, one would suppose, at first alarmingly affected by the great noisy, smoking monster rushing along their favourite and hitherto usually silent haunts. JOHN RAE

4, Addison Gardens, June 30

Large Hailstones

A SEVERE thunderstorm passed over Woodlesford, six miles south-east of Leeds, between 3.10 and 4 p.m. this afternoon, proceeding from south-west to north-east. Flashes of lightning during that time were almost continuous. At 3.15 heavy rain began to fall, becoming so thick at 3.25 as to render objects a short distance away almost indistinct; at 3.30 this changed to hail, the stones during the worst period being generally irregular parallelopipeds of ice, with two edges of about one inch each, and the third of one-quarter of an inch. These blocks consisted of hard, colourless, transparent ice, surrounding a central, irregularly-shaped mass of opaque white, small air-bubbles of roughly ellipsoidal shape being ranged round this. The white nucleus was not quite so hard as the exterior transparent coating. The force of collision on the railway line was sufficient to make the masses bound to a vertical height of two or three feet. At 3.45 the hail had moderated, when a few light loose clouds were observed quickly passing from north-east to south-west, and thus directly opposite to the direction of the storm, and at a much lower level. R. WEBB

June 30

Extinction of Flatfish

I HAVE been advised by Mr. Murray of the *Challenger* expedition to inquire, through your columns, whether the experience of any of your correspondents coincides with mine as to the gradual failure—in some places almost the extinction—of flatfish where whelk-gathering is prosecuted. MALCOLM MCNEILL

The New Club, Edinburgh, June 30

Garfish

IN March last I was being pulled off from the shore to H.M.S. *Himalaya* in the harbour at Aden, when a fish jumped out of the water over the boat, and in doing so struck the hat of another officer and knocked it into the water. When the hat was recovered we found in the *hard* felt a slit about four inches in length. Unfortunately the fish escaped, but the impression of those who saw it was that it was some kind of garfish, and that the damage done was inflicted by the beak. It appeared to me to be about ten inches long. It is obvious that had the fish struck my friend in the face or neck, or even in the chest, it might have resulted in a fatal injury. S. ARCHER

Sheerness, June 29

The "Spirogyra quinina"

CAN any of your readers inform me of any practical method of exterminating, in a lodge or reservoir, confervoid algæ, more especially the fine filamentous variety *Spirogyra quinina?*

Hanley, Staffordshire, June 19 FREDK. HAIGH

ACTION OF LIGHT ON INDIARUBBER

IN continuation of the experiments described in NATURE, vol. xxvii. p. 312, two pieces of caoutchouc tube, about 48 mm. long and 7 mm. wide, were introduced on January 23, 1883, into test tubes containing oxygen confined over mercury. One of these tubes was surrounded by a case of black paper, and both tubes were placed side by side in a north window. On June 27 the tubes were examined: in that exposed to light about 17 cc. of oxygen (about three-quarters of the gas the test tube at first contained) had been absorbed, and the india-rubber had become altered, so that on pressing the tube between the fingers superficial cracks were produced. In the other test tube no appreciable diminution of gas had taken place, and the caoutchouc was unchanged, thus fully confirming the results of the former experiments. We may therefore conclude that caoutchouc alters under the combined influence of light and oxygen, but that neither alone produces any effect.

Cooper's Hill, June 29 HERBERT MCLEOD

ON WHALES, PAST AND PRESENT, AND THEIR PROBABLE ORIGIN[1]

II.

THOUGH the early stages by which whalebone has been modified from more simple palate structures are entirely lost to our sight, probably for ever, the conditions in which it now exists in different species of whales, show very marked varieties of progress, from a simple comparatively rudimental and imperfect condition, to what is perhaps the most wonderful example of mechanical adaptation to purpose known in any organic structure. These variations are worth dwelling upon for a few minutes, as they illustrate in an excellent manner the gradual modifications that may take place in an organ, evidently in adaptation to particular requirements, the causation of which can be perfectly explained upon Darwin's principle of natural selection.

In the Rorquals or fin-whales (genus *Balænoptera*), found in almost all seas, and so well known off our own coasts, the largest blades in an animal of 70 feet in length do not exceed 2 feet in length, including their hairy terminations; they are in most species of a pale horn colour, and their structure is coarse and inelastic, separating into thick, stiff fibres, so that they are of no value for the ordinary purposes to which whalebone is applied in the arts. These animals feed on fish of considerable size, from herrings up to cod, and for foraging among shoals of these creatures the construction of their mouth and the structure of their baleen is evidently sufficient. This is the type of the earliest known extinct forms of whales, and it has continued to exist, with several slight modifications, to this day, because it has fulfilled one purpose in the economy of nature. Other purposes for which it was not

[1] Lecture delivered at the Royal Institution on the evening of Friday, May 25, 1883, by Prof. Flower, LL.D., F.R.S., P.Z.S., &c. Concluded from p. 202.

sufficient have been supplied by gradual changes taking place, some of the stages of which are seen in the intermediate conditions still exhibited in the Megaptera, and the Atlantic and Southern Right Whales. Before describing the extreme modifications in the direction of complexity, I may mention, to show the range at present presented in the development of baleen, that there has lately been discovered in the North Pacific a species called by the whalers the Californian Gray Whale (*Rachianectes glaucus*), which shows the opposite extreme of simplicity. The animal is from 30 to 40 feet in length; the baleen blades are only 182 on each side (according to Scammon) and far apart, very short (the longest being from 14 to 16 inches in length), light brown or nearly white in colour, and still more coarse in grain and inelastic than that of the Rorquals. The food of these whales is not yet known with certainty. They have been seen apparently seeking for it along soft bottoms of the sea, and fuci and mussels have been found in their stomachs.

In the Greenland Right Whale of the circumpolar seas, the Bow-head of the American whalers (*Balæna mysticetus*), all the peculiarities which distinguish the head and mouth of the whales from other mammals have attained their greatest development. The head is of enormous size, exceeding one-third of the whole length of the creature. The cavity of the mouth is actually larger than that of the body, thorax and abdomen together. The upper jaw is very narrow, but greatly arched from before backwards, to increase the height of the cavity and allow for the great length of the baleen, the enormous rami of the mandibles are widely separated posteriorly, and have a still further outward sweep before they meet at the symphysis in front, giving the floor of the mouth the shape of an immense spoon. The baleen blades attain the number of 350 or more on each side, and those in the middle of the series have a length of ten or even twelve feet. They are black in colour, fine and highly elastic in texture, and fray out at the inner edge and ends into long delicate, soft, almost silky, but very tough hairs.

How these immensely long blades depending vertically from the palate were packed into a mouth the height of which was scarcely more than half their length, was a mystery not solved until a few years ago. Capt. David Gray of Peterhead, at my request, first gave us a clear idea of the arrangement of the baleen in the Greenland whale, and showed that the purpose of its wonderful elasticity was not primarily at least the benefit of the corset and umbrella makers, but that it was essential for the correct performance of its functions. It may here be mentioned that the modification of the mouth structure of the Right Whale is entirely in relation to its food. It is by this apparatus that it is enabled to avail itself of the minute but highly nutritious crustaceans and pteropods which swarm in immense shoals in the seas it frequents. The large mouth enables it to take in at one time a sufficient quantity of water filled with these small organisms, and the length and delicate structure of the baleen provides an efficient strainer or hair sieve by which the water can be drained off. If the baleen were, as in the Rorquals, short and rigid, and only of the length of the aperture between the upper and lower jaws when the mouth was shut, when the jaws were separated a space would be left beneath it through which the water and the minute particles of food would escape together. But instead of this, the long, slender, brush-like ends of the whalebone blades, when the mouth is closed, fold back, the front ones passing below the hinder ones in a channel lying between the tongue and the bone of the lower jaw. When the mouth is opened their elasticity causes them to straighten out like a bow that is unbent, so that at whatever distance the jaws are separated, the strainer remains in perfect action, filling the whole of the interval. The mechanical perfection of the arrangement is completed by the great development of the lower lip, which rises stiffly above the jaw-bone, and prevents the long, slender, flexible ends of the baleen being carried outwards by the rush of water from the mouth, when its cavity is being diminished by the closure of the jaws and raising of the tongue. The interest and admiration excited by the contemplation of such a beautifully adjusted piece of mechanism is certainly heightened by the knowledge that it has been brought about by the gradual adaptation and perfection of structures common to the whole class of animals to which the whale belongs.

Few points of the structure of whales offer so great a departure from the ordinary mammalian type as the limbs. The fore limbs are reduced to the condition of simple paddles or oars, variously shaped, but always flattened and more or less oval in outline. They are freely movable at the shoulder-joint, where the humerus or upper-arm bone articulates with the shoulder-blade in the usual manner, but beyond this point, except a slight flexibility and elasticity, there is no motion between the different segments. The bones are all there, corresponding in number and general relations with those of the human or any other mammalian arm, but they are flattened out, and their contiguous ends, instead of presenting hinge-like joints, come in contact by flat surfaces, united together by strong ligamentous bands, and all wrapped up in an undivided covering of skin, which allows externally of no sign of the separate and many-jointed fingers seen in the skeleton.

Up to the year 1865 it was generally thought that there was nothing to be found between this bony framework and the covering skin, with its inner layer of blubber, except dense fibrous tissue, with blood-vessels and nerves sufficient to maintain its vitality. Dissecting a large Rorqual, 67 feet in length, upon the beach of Pevensey Bay in that year, I was surprised to find lying upon the bones of the fore-arm well-developed muscles, the red fibres of which reached nearly to the lower end of these bones, ending in strong tendons, passing to, and radiating out on, the palmar surface of the hand. Circumstances then prevented me following out the details of their arrangement and distribution, but not long afterwards Prof. Struthers of Aberdeen had an opportunity of carefully dissecting the fore-limb of another whale of the same species, and he has recorded and figured his observations in the *Journal of Anatomy* for November, 1871. He found on the internal or palmar aspect of the limb three distinct muscles corresponding in attachments to the flexor carpi ulnaris, the flexor profundus digitorum, and the flexor longus pollicis of man, and on the opposite side but one, the extensor communis digitorum.[1] Large as these muscles actually are, yet, compared with the size of the animal, they cannot but be regarded as rudimentary and being attached to bones without regular joints and firmly held together by unyielding tissues, their functions must be reduced almost to nothing. But rudimentary as the muscles of the Fin-whales are, lower stages of degradation of the same structures are found in other members of the group. In some they are indeed present in form, but their muscular structure is gone and they are reduced in most of the toothed whales to mere fibrous bands, scarcely distinguishable from the surrounding tissue which connects the inner surface of the skin with the bone. It is impossible to contemplate these structures without having the conviction forced home that here are the remains of parts once of use to their possessor, now, owing to the complete change of purpose and mode of action of the limb, reduced to a condition of atrophy verging on complete disappearance.

The changes that have taken place in the hind-limbs are even more remarkable. In all known Cetacea (unless

[1] The muscles of the forearm of an allied species, *Balænoptera rostrata*, were described by Macalister in 1868, and Perrin in 1870.

Platanista be really an exception) a pair of slender bones are found suspended a short distance below the vertebral column, but not attached to it, about the part where the body and the tail join. In museum skeletons these bones are often not seen, as, unless special care has been taken in the preparation, they are apt to get lost. They are, however, of much importance and interest, as their relations to surrounding parts show that they are the rudimentary representatives of the pelvic or hip bones, which in other mammals play such an important part in connecting the hind-limbs with the rest of the skeleton. The pelvic arch is thus almost universally present, but of the limb proper there is, as far as is yet known, not a vestige in any of the large group of toothed whales, not even in the great Cachalot or Sperm Whale, although it should be mentioned that it has never been looked for in that animal with any sort of care. With regard to the Whalebone Whales, at least to some of the species, the case is different. In these animals there are found, attached to the outer and lower side of the pelvic bone, other elements, bony or only cartilaginous as the case may be, clearly representing rudiments of the first and in some cases the second segment of the limb, the thigh or femur, and the leg or tibia. In the small *Balænoptera rostrata* a few thin fragments of cartilage, embedded in fibrous tissue attached to the side of the pelvic bone, constitute the most rudimentary possible condition of a hind-limb, and could not be recognised as such but for their analogy with other allied cases. In the large Rorqual, *Balænoptera musculus*, 67 feet long, previously spoken of, I was fortunate enough in 1865 to find attached by fibrous tissue to the side of the pelvic bone (which was sixteen inches in length) a distinct femur, consisting of a nodule of cartilage of a slightly compressed, irregularly oval form, and not quite one inch and a half in length. Other specimens of the same animal dissected by Van Beneden and Prof. Struthers have shown the same; in one case, partial ossification had taken place. In the genus *Megaptera* a similar femur has been described by Eschricht; and the observations of Reinhardt have shown that the Greenland Right Whale (*Balæna mysticetus*) has not only a representative of the femur developed far more completely than in the Rorqual, being from six to eight inches in length and completely ossified, but also a second smaller and more irregularly formed bone, representing the tibia. Our knowledge of these parts in this species has recently been greatly extended by the researches of Dr. Struthers of Aberdeen, who has published in the *Journal of Anatomy* for 1881 a most careful and detailed account of the dissection of several specimens, showing the amount of variation to which these bones (as with most rudimentary structures) are liable in different individuals, and describing for the first time their distinct articulation one with the other by synovial joints and capsular ligaments, and also the most remarkable and unlooked-for presence of muscles passing from one bone to the other, representing the adductors and flexors of mammals with completely developed limbs, but so situated that it is almost impossible to conceive that they can be of any use; the whole limb, such as it is, being buried deep below the surface, where any movement, except of the most limited kind, must be impossible. Indeed, that the movement is very limited and of no particular importance to the animal was shown by the fact that in two out of eleven whales dissected the hip-joint was firmly anchylosed (or fixed by bony union) though without any trace of disease. In the words of Dr. Struthers, "Nothing can be imagined more useless to the animal than rudiments of hind-legs entirely buried beneath the skin of a whale, so that one is inclined to suspect that these structures must admit of some other interpretation. Yet, approaching the inquiry with the most sceptical determination, one cannot help being convinced, as the dissection goes on, that these rudiments really are femur and tibia. The functional point of view fails to account for their presence. Altogether they present for contemplation a most interesting instance of those significant parts, rudimentary structures."

We have here a case in which it is not difficult to answer the question before alluded to, often asked with regard to rudimentary parts, Are they disappearing or are they incipient organs? We can have no hesitation in saying that they are the former. All we know of the origin of limbs shows that they commence as outgrowths upon the surface of the body, and that the first-formed portions are the most distal segments. The limb, as proved by its permanent state in the lowest Vertebrates, and by its embryological condition in higher forms, is at first a mere projection or outward fold of the skin, which, in the course of development, as it becomes of use in moving or supporting the animal, acquires the internal framework which strengthens it and perfects its functions. It would be impossible, on any theory of causation yet known, to conceive of a limb gradually developed from within outwards. On the other hand, its disappearance would naturally take place in the opposite direction; projecting parts which had become useless, being in the way, would, like all the other prominences on the surface of the whales, hair, ears, &c., be removed, while the most internal, offering far less interference with successful carrying on the purposes of life, would be the last to disappear, lingering, as in the case of the Greenland Whale, long enough to reveal their wonderful history to the anatomist who has been fortunate enough to possess the skill and the insight to interpret it.

Time will not allow of more illustrations drawn from the structure of existing Cetacea; we turn next to what the researches of palæontology teach of the past history of the order. Unfortunately this does not at present amount to very much. As is the case with nearly all other orders of mammals, we know nothing of their condition, if they existed, in the mesozoic age. Even in the cretaceous seas, the deposits at the bottom of which are so well adapted to preserve the remains of the creatures which swam in them, not a fragment of any whale or whale-like animal has been found. The earliest Cetaceans of whose organisation we have any good evidence, are the Zeuglodons of the Eocene formations of North America. These were creatures whose structure, as far as we know it, was intermediate between that of the existing suborders of whales, having the elongated nasal bones and anterior position of the nostrils of the Mystacocetes, with the teeth of the Odontocetes, and with some characters more like those of the generalised mammalian type, than of any of the existing forms. In fact *Zeuglodon* is precisely what we might have expected *a priori* an ancestral form of whale to have been. The remarkable smallness of its cerebral cavity, compared with the jaws and the rest of the skull, so different from that of modern Cetaceans, is exactly paralleled in the primitive types of other groups of mammals. The teeth are markedly differentiated in different parts of the series. In the anterior part of both jaws they are simple, conical, or slightly compressed and sharp pointed. The first three of the upper jaw are distinctly implanted in the premaxillary bone, and so may be reckoned as incisors. The tooth which succeeds, or the canine, is also simple and conical, but it does not greatly exceed the others in size. This is followed by five teeth with two distinct roots and compressed pointed crowns, with denticulated cutting edges. It has been thought that there was evidence of a vertical succession of the molar teeth, as in diphyodont mammals, but the proof of this is not quite satisfactory. Unfortunately the structure of the limbs is most imperfectly known. A mutilated humerus has given rise to many conjectures; to some anatomists it appears to indicate freedom of motion at the elbow-joint, while to others its characters seem to be those of the ordinary Cetacea.

Of the structure of the pelvis and hind limb we are at present in ignorance.

From the middle Miocene period fossil Cetacea are abundant, and distinctly divided into the two groups now existing. The Mystacocetes, or Whalebone Whales, of the Miocene seas were, as far as we know now, only *Balænopteræ*, some of which (as the genus *Cetotherium*) were, in the elongated flattened form of the nasal bones, the greater distance between the occipital and frontal bone at the top of the head, and the greater length of the cervical vertebræ, more generalised than any now existing. In the shape of the mandible also, Van Beneden, to whose researches we are chiefly indebted for a knowledge of these forms, discerns some approximation to the Odontocetes. Right Whales (*Balæna*) have not been found earlier than the Pliocene period, and it is interesting to note that instead of the individuals diminishing in bulk as we approach the times we live in, as with many other groups of animals, the contrary has been the case, no known extinct species of whales equalling in size those that are now to be met with in the ocean. The size of whales, as of all other things whose most striking attribute is magnitude, has been greatly exaggerated; but when reduced to the limits of sober fact, the Greenland Right Whale of 50 feet long, the Sperm Whale of 60, and the Great Northern Rorqual (*Balænoptera Sibbaldii*) of 80, exceed all other organic structures known, past or present. Instead of living in an age of degeneracy of physical growth, we are in an age of giants, but it may be at the end of that age. For countless ages impulses from within and the forces of circumstances from without have been gradually shaping the whales into their present wonderful form and gigantic size, but the very perfection of their structure and their magnitude combined, the rich supply of oil protecting their internal parts from cold, the beautiful apparatus of whalebone by which their nutrition is provided for, have been fatal gifts, which, under the sudden revolution produced on the surface of the globe by the development of the wants and arts of civilised man, cannot but lead in a few years to their extinction.

It does not need much foresight to divine the future history of whales, but let us return to the question with which we started, What was their probable origin?

In the first place, the evidence is absolutely conclusive that they were not originally aquatic in habit, but are derived from terrestrial mammals of fairly high organisation, belonging to the placental division of the class,—animals in which a hairy covering was developed, and with sense organs, especially that of smell, adapted for living on land; animals, moreover, with four completely-developed pairs of limbs on the type of the higher vertebrata, and not of that of fishes. Although their teeth are now of the simple homodont and diphyodont type, there is much evidence to show that this has taken place by the process of degradation from a more perfect type, even the fœtal teeth of Whalebone Whales showing signs of differentiation into molars and incisors, and many extinct forms, not only the Zeuglodons, but also true dolphins, as the Squalodons, having a distinct heterodont dentition, the loss of which, though technically called a "degradation," has been a change in conformity to the habits and needs of the individuals. So much may be considered very nearly if not quite within the range of demonstrated facts, but it is in determining the particular group of mammals from which the Cetacea arose that greater difficulties are met with.

One of the methods by which a land mammal may have been changed into an aquatic one is clearly shown in the stages which still survive among the Carnivora. The seals are obviously modifications of the land Carnivora, the Otaria, or Sea-Lions and Sea-Bears, being curiously intermediate. Many naturalists have been tempted to think that the whales represent a still further stage of the same kind of modification. So firmly has this idea taken root that in most popular works on zoology in which an attempt is made to trace the pedigree of existing mammals, the Cetacea are definitely placed as offshoots of the Pinnipedia, which in their turn are derived from the Carnivora. But there is to my mind a fatal objection to this view. The seal of course has much in common with the whale, inasmuch as it is a mammal adapted for an aquatic life, but it has been converted to its general fishlike form by the peculiar development of its hind-limbs into instruments of propulsion through the water; for though the thighs and legs are small, the feet are large and are the special organs of locomotion in the water, the tail being quite rudimentary. The two feet applied together form an organ very like the tail of a fish or whale, and functionlly representing it, but only functionally, for the time has I trust quite gone by when the Cetacea were defined as animals with the "hinder limbs united, forming a forked horizontal tail." In the whales, as we have seen, the hind-limbs are aborted and the tail developed into a powerful swimming organ. Now it is very difficult to suppose that, when the hind-limbs had once become so well adapted to a function so essential to the welfare of the animal as that of swimming, they could ever have become reduced and their action transferred to the tail;—the animal must have been in a too helpless condition to maintain its existence during the transference, if it took place, as we must suppose, gradually. It is far more reasonable to suppose that whales were derived from animals with large tails, which were used in swimming, eventually with such effect that the hind-limbs became no longer necessary, and so gradually disappeared. The powerful tail, with lateral cutaneous flanges, of an American species of Otter (*Pteronura sandbachii*) or the still more familiar tail of the beaver, may give some idea of this member in the primitive *Cetacea*. I think that this consideration disposes of the principal argument that the whales are related to the seals, as most of the other resemblances, such as those in the characters of their teeth, are evidently analogous resemblances related to similarity of habit.

As pointed out long ago by Hunter, there are numerous points in the structure of the visceral organs of the Cetacea far more resembling those of the Ungulata than the Carnivora. These are the complex stomach, simple liver, respiratory organs, and especially the reproductive organs and structures relating to the development of the young. Even the skull of Zeuglodon, which has been cited as presenting a great resemblance to that of a seal, has quite as much likeness to one of the primitive pig-like Ungulates, except in the purely-adaptive character of the form of the teeth.

Though there is, perhaps, generally more error than truth in popular ideas on natural history, I cannot help thinking that some insight has been shown in the common names attached to one of the most familiar of Cetaceans by those whose opportunities of knowing its nature have been greatest—"Sea-Hog," "Sea-Pig," or "Herring-Hog" of our fishermen, *Meerschwein* of the Germans, corrupted into the French "Marsouin," and also "Porc-poisson," shortened into "Porpoise."

A difficulty that might be suggested in the derivation of the Cetacea from the Ungulata, arising from the latter being at the present day mainly vegetable feeders, is not great, as the primitive Ungulates were probably omnivorous, as their least modified descendants, the pigs, are still; and the aquatic branch might easily have gradually become more and more piscivorous, as we know from the structure of their bones and teeth, the purely terrestrial members have become by degrees more exclusively graminivorous.

One other consideration may remove some of the difficulties that may arise in contemplating the transition of land mammals into whales. The Gangetic Dolphin (*Platanista*) and the somewhat related *Inia* of South

America, which retain several rather generalised mammalian characters, and are related to some of the earliest known European Miocene forms, are both to the present day exclusively fluviatile, being found in the rivers they inhabit almost up to their very sources, more than a thousand miles from the sea. May this not point to the freshwater origin of the whole group, and thus account for their otherwise inexplicable absence from the Cretaceous seas?

We may conclude by picturing to ourselves some primitive generalised, marsh-haunting animals with scanty covering of hair like the modern hippopotamus, but with broad, swimming tails and short limbs, omnivorous in their mode of feeding, probably combining water plants with mussels, worms, and freshwater crustaceans, gradually becoming more and more adapted to fill the void place ready for them on the aquatic side of the borderland on which they dwelt, and so by degrees being modified into dolphin-like creatures inhabiting lakes and rivers, and ultimately finding their way into the ocean. Here the disappearance of the huge Enaliosaurians, the *Ichthyosauri* and *Plesiosauri*, which formerly played the part the Cetacea do now, had left them ample scope. Favoured by various conditions of temperature and climate, wealth of food supply, almost complete immunity from deadly enemies, and illimitable expanses in which to roam, they have undergone the various modifications to which the Cetacean type has now arrived, and gradually attained that colossal magnitude which we have seen was not always an attribute of the animals of this group.

Please to recollect, however, that this is a mere speculation, which may or may not be confirmed by subsequent palæontological discovery. Such speculations are, I trust, not without their use and interest, especially when it is distinctly understood that they are offered only as speculations and not as demonstrated facts.

THE AMERICAN OBSERVATIONS OF THE ECLIPSE

NEWS of the American observations of the last eclipse has now arrived, and although details are yet wanting, enough information has been sent to show us that, as was to be expected, the American observers have left their mark upon the work. The telegram given below has been forwarded to me by the editors of *Science*, and is one transmitted by Prof. Holden to Prof. Young on the arrival of the former at San Francisco :—

"*San Francisco, Cal., June* 11

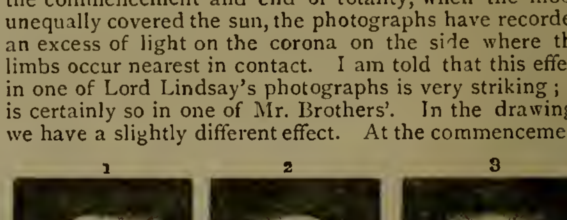
"American Eclipse Expedition arrived at St. Francisco June 11. Holden reports no Vulcan as bright as $5\frac{1}{2}$ magnitude. Hasting's observations prove the corona to be largely a phenomenon of diffraction by the great change in lengh of 1474 line on east and west sides of sun. No black lines in corona spectrum but D. Full observations with grating spectroscopes, prismatic telescope, and integrating spectroscope, by Rockwell, Upton, and Brown. Contacts by Preston. English and French parties successful. (Signed) E. S. HOLDEN"

It will be seen from the above that the spectroscopic attack was a very strong one, and although the telegram gives only the results of the work of Prof. Hastings, these are of unusual interest. I propose, therefore, to devote attention to them in the present notice. It will, however, be well to anticipate my remarks by a prefatory notice of the eclipse work on which it throws light. For this purpose I can scarcely do better than give the following extract from an article which appeared in the *Times* on Monday last :—

"It was only really in the eclipse of 1869 that we began to know anything about the corona, and it was only in the eclipse of 1870 that we began to appreciate what a very difficult problem was presented to us by that phenomenon. The then Astronomer-Royal and Prof. Maedler, to cite some among the eminent authorities writing after the eclipse of 1860, had come to the conclusion that the corona was mainly a non solar phenomenon. That *part* of it, however, was undoubtedly solar was admitted by all, for the reason that it was seen before and after totality. In the eclipse of 1870 the idea that part of it was really non-solar was enormously strengthened by a comparison of observations made by different astronomers. Its shape seemed to change as the moon swept over it, and this obviously, if it were true, implied some action of the moon's edge and reflection by something between the observer and the moon. In 1871, when the Government of India and the British Association took steps to have the corona photographed at the same time that it was carefully observed by the naked eye, the strange fact was first clearly indicated that the corona seen by the eye was a perfectly different thing to that recorded on the photographic plates. The explanation given at the time was that the coronal light was much more actinic than ordinary solar light of the same visible intensity, so that in the eye and on the photographic plate two different images were built up by different qualities of light proceeding from different sources. Hence the view was distinctly enunciated that the corona seen during eclipses was a dual phenomenon, partly solar, partly non-solar in its origin, the true solar corona being filamentous with variously-curved streamers, the visible corona being non-filamentous and consisting mainly of radial lines and rifts, extending to different distances from the edge of the moon"

This slight sketch may now be expanded by the following details. Thus, for instance, in March, 1870, Prof. Young, discussing the then current views of the corona, wrote :—"It is not impossible that the so-called corona may be complex, some portion of its radiance may perhaps originate in our own atmosphere, though I do not yet find myself able to agree with the conclusion of Dr. Gould and Mr. Lockyer in this respect, and I am strongly disposed to believe that the whole phenomenon is purely solar."

With reference to the eclipse of 1870 I wrote :—"At the commencement and end of totality, when the moon unequally covered the sun, the photographs have recorded an excess of light on the corona on the side where the limbs occur nearest in contact. I am told that this effect in one of Lord Lindsay's photographs is very striking; it is certainly so in one of Mr. Brothers'. In the drawings we have a slightly different effect. At the commencement



of totality, when the western or right-hand limbs were in contact, we get (see figure) 1 ; at the end of totality the appearance recorded was like 2 ; the picture at the middle of totality compounding both these appearances, and being roughly represented by 3, in which the rectangular appearance comes out in its full strength."

Let us pass on to the eclipse of 1871. This was my description, written at the time, of what I saw :—"There, rigid in the heavens, was what struck everybody as a decoration, one that Emperors might fight for; a thousand times more brilliant even than the star of India,—where we then were,—a picture of surpassing loveliness, and giving one the idea of serenity among all the activity that was going on below, shining with a sheen as of silver essence, built up of rays almost symmetrically arranged round a bright ring above and below, with a marked absence of

them right and left, *the rays being composed of sharp radial lines*, separated by furrows of markedly less brilliancy."

After there had been time to examine the photographic records of the eclipse in connection with the above description, the enormous difference between the photograph and the eye picture was fully recognised, and in my lecture at the Royal Institution on the eclipse, after referring to the actinic corona, to the striking similarity in the details of the photographs taken at different times and in different places, I said : "The solar nature of most, if not all, of the corona recorded on the plates is established by the fact that the plates, taken in different places, and both at the beginning and end of totality, closely resemble each other, and much of the exterior detailed structure is a continuation of that observed in the inner portion independently determined by the spectroscope to belong to the sun."

Passing from the photographs to the drawings, I pointed out that in Mr. Holiday's sketch, for instance, we got an infinite number of dark radial lines extending down to the moon, with a greater extension than in the photographs, though in some places the shape of the actinic corona and some of its details were shown.

Thinking that this difference might be explained by different lights being superposed, so that of two superposed lights the naked eye used one, and the photographic plate the other, I asked the question whether the facts might not be reconciled, and really harmonised with what was actually seen in the telescope, even by supposing that the visual image, this glare let us call it, was sifted in the telescope by using greater or less magnification in the same way as it was separated out on the photographic plates and in our eyes by the different qualities of the light producing the visual and photographic images.

From this point of view, therefore, I regard Mr. Hasting's observation as one of very great interest, and I believe that it throws light upon a good many prior observations. I do not think, however, that any one will go with him when he proposes to abolish a true corona at the sun, for the reason that the observations to which I have drawn attention show that it is really a dual phenomenon as I pointed out in 1870, and although diffraction at the moon's edge may be the cause of one part, it cannot be the cause of the other. It is, perhaps, almost too early yet to speculate upon the changes in our views of the chemical nature of the external boundary of the sun's atmosphere which may be brought about by a complete discussion of the question which these observations again bring to the front. I long ago pointed out that the fact of getting in the spectroscope an indication of a line at so many minutes of arc from the limb of the dark moon, was by no means a proof of the existence of a vapour or gas at that height above the sun. Maclear's observation in 1870 was of course the test, for the reason that if such a *caveat* were not available we must assume the existence of coronal matter between us and the dark moon. But in any case it is not too early to bear this in mind, that if in our spectroscopes we have been dealing with a true glare, from whatever cause produced, there will be an almost complete inversion necessitated, and in this way: the brilliancy of any particular wave-length of the glare may either depend upon the area of the surface at the sun producing light of that wave-length, or upon its inherent intensity. Now if we assume that only the inherent intensity is to be considered, then obviously the region of greatest temperature will cause the brightest light. The brightest light will therefore be produced in the lowest level of the solar atmosphere, but because of the glare it will appear to extend to the greatest distance from the sun. It may therefore have been that the line 1474, instead of indicating, as it has been supposed to do, that a substance which gives a line at the part of the solar atmosphere most removed from the photosphere is really produced by that part of the atmosphere, was produced at that part of the atmosphere nearest the photosphere, and really at first sight—although this is by no means a matter on which one would wish to commit one's self hastily—it does seem as if this view would harmonise a great many facts which are very difficult of explanation in any other way.

I discovered the line 1474 in the chromosphere on June 6, 1869, and up to that time no bright line had been observed beyond those belonging to the spectra of hydrogen, sodium, and magnesium, with the exception of one line of barium, which was first seen in March, 1869. Now we know from the long-extended series of such observations for which we have to thank the industry of the Italian observers, that the line 1474 is now seen more persistently than any line which is not recorded in the spectra of hydrogen, magnesium, and sodium. The eclipse of last year taught us, if it taught us anything, that the lines which are thus persistent are the lines produced at the temperature of the hottest layers, and, if subsequent inquiry strengthens the view that the height to which the line 1474 appears to extend is really due to the depth at which the substance which produces it is restricted, the persistence of 1474 in ordinary chromospheric observations will be at once explained.

J. NORMAN LOCKYER

AGRICULTURE IN JAPAN[1]

DR. LIEBSCHER'S little work is the result, the author tells us, of his investigations during an eight months' sojourn in Japan in 1880. A cursory glance at the contents shows that it bears the physiognomy of a strictly scientific work. The work is divided into five parts :—(1) The condition of the climate and its influence upon the land-products ; (2) the condition of soils and its influence upon the land-products ; (3) the social condition before the year 1868 (before the reformation) ; (4) the reformation and reorganisation of the State since the year 1868 ; (5) foreign commerce. I shall notice shortly each chapter with some remarks. Beginning with the first chapter, Dr. Liebscher commences with the monsoon, within whose sphere Japan is situated. It has, as is well known, a certain determined direction during the whole year. The summer (south-west) monsoon comes from the south-west from April to September, while the winter (north-east) monsoon comes from the north-east during the rest of the year. To the first, according to Dr. Liebscher, Japan owes its tropical flora, such as *Chamærops excelsa*, *Thea viridis*, *Cycas revoluta*, &c., and to the same he attributes the chief land-products, such as cotton, sugar-cane, tobacco, Indian corn, and rice. Why the summer monsoon is so favourable to the growth of the land-products is because, says the author, it causes a warm temperature, and the abundant precipitation of rain (maximum 1794 mm. in a year). He ignores then altogether the geographical position of Japan, that on one side she lies partly in a subtropical and temperate zone, on the other she is surrounded on all sides by a large body of water. The north-east monsoon brings a dry and terribly cold winter, though somewhat modified by the "Kuro-Siwo" current and this monsoon is the sole factor that renders the climate unfavourable, causing the remarkable phenomenon of the "freezing of the soil." Dr. Liebscher says, the regular course of the monsoon assures the people who happen to inhabit those lands which lie within the sphere of that wind, of a never-failing good crop of rice. Thus we are accustomed to depend solely upon rice, and

[1] "Japan's landwirthschaftliche und allgemeinwirthschaftliche Verhältnisse nach eigenen Beobachtungen dargestellt." Von Dr. G. Liebscher. (Jena 1882.)

consequently we become vegetarians. We cannot entirely agree with him, but rather lay more stress upon the influence of the Buddhist religion, which once wielded sway over us. As to the unfavourableness of the climate due to the monsoon, he is unfortunate in selecting as an example the Hakone region. The volcano of Fuji San (3784 m.) is a high peak covered with everlasting snow, and at the foot of this lies the Hakone Pass (804 m.). Here Dr. Liebscher had seen on the western side (toward Fuji) around the Lake Hakone, a dreary sterile slope; while the opposite mountains, lying on the south, are covered with a luxuriant growth of forest. He ascribes the cause of the sterility of the northern to the cold winter monsoon, and the thickly wooded ranges to the summer monsoon. I explain this striking contrast quite in another way. Fuji San is an active volcano, and at the foot of this lies the region referred to. It is natural that no tree will flourish at or near recent volcanoes, which send out an enormous quantity of scoriæ. Moreover the Hakone Pass is situated at a high altitude (804 m.). We find thick forest at the top of the Brocken in the Harz mountains. Could we expect the same at the summit of Vesuvius? The climate of Japan is not so ineffective as Dr. Leibscher has depicted in his work; in reality it is far more conducive to fertility than that of Germany.

The second chapter deals with the soil and its influence upon the agriculture. "A large tract of plain not far from Tokio is left uncultivated," says the author; "while the mountain slopes are turned into useful land. Such an irrational course is not difficult to understand when I consider the other deeds done by the Government and people." This is not so serious as it seems; he did not understand the irrigation of soils, which is of particular importance in the rice-producing countries. He mentions in another page that the total area of the empire is 38,243,640 hectares, of which the cultivated land occupies 4,508,482 hectares, *i.e.* 11·8 per cent.; while the area of Prussia is 34,823,420, with 17,435,605 hectares, *i.e.* 50·7 per cent. of the cultivated land. The balance is evidently against Japan. I must here remark that Prussia is not mountainous. The only notable range in the heart of Prussia is the Harz; the Thuringian forest, the Riesen and Sudeten mountains lie at the southernmost boundaries of that country; all the rest forms what is called the "North German Plain," levelled down uniformly by the Scandinavian glaciers in the Diluvial period. On the contrary, Japan is very mountainous. Moreover, it must be taken into account that we have newly taken possession of the Riū-Kiū Islands. The island Hokkaidô, Chi-Sima (the Kurile Islands) were neglected till thirteen years ago. They are now substantially incorporated into Japan, and the present Government is energetically striving to convert these into utility. From these circumstances the author is not justified in jumping to the conclusion as to the present state of things. We are glad to find that the yield per hectare in Japan is 35·62, while in Germany it is only 6·11 (Dr. E. Naumann). As to the geology, the bearing of which is of great importance to the soil and subsoil, Dr. Liebscher closely follows Rein's "Japan," without contributing his own observations. The chief rock-groups are: (1) the crystalline massive rocks (granite, diorite, diabase, porphyries); (2) the palæozoic schists; (3) the more recent volcanic rocks (trachyte, rhyolite, andesites, dolerite, basalt); (4) the alluvium and diluvium. Among the first group are phosphates, salts of potash and soda in the form of felspar, and apatite; and the same minerals are richly contained in the third group. In Rein's palæozoic schists, recent trias and cretaceous formations are ascertained by geologists of the Geological Survey, and must be separated from the second group of Rein's geological category. The author lays great stress upon the sterility of soils, to the extensive development of the talc and chlorite schists and so-called "tuff soils"—a fine volcanic ejectamenta poured from the vent, and sediment under water. Indeed, I saw, myself, in the provinces of Musasi, Sanuki, and Rü, the phyllite system, in which the talc and chlorite schists form an essential member; still they sink into insignificance when compared with the other rock groups. Moreover, the so-called talc schist is in reality micaceous clay slate, and the pseudochlorite schist is chloritic epidote hornblende schist. These facts will somewhat modify the author's conclusion. As to the "tuff soil," he discusses and repudiates the uncertain analyses of Prof. E. Kinch and Herr von Korschelt. Neither of these gentlemen, I think, are correct, supposing that their analyses have been carefully prosecuted. They select as samples the "tuff soils" from the neighbourhood of Tokio. This city lies in the plain, surrounded by lofty volcanic chains—Fuji, Asama, Sirane, and many other ranges of volcanic nature, bounded on the south-east by sandstone mountains of the Awa province. Tuff and sandstones, *a priori*, could not produce fertile soils, and indeed "tuff soils" are the poorest in Japan. It is not found everywhere in that country, and appears exclusively confined to the neighbourhood of Tokio. I doubt very much the nature of the so-called "tuff soil." It may perhaps be an accumulation of diluvial sand and gravels. If samples for chemical analyses were obtained, the soils from the Mino province among others, we should be able to get a true insight into the Japanese soils. The author's conclusions, based upon these unfortunately ill-chosen samples, could, of course, not be correct, because the premises are already wrong. It is remarkable that Dr. Liebscher, as a professional agriculturist, after travelling through the greater part of Japan, should not be able to throw some new light on this point.

On the third chapter I have little to say, for the description relates to the bygone world prior to the year 1868. At present, our social condition assumes quite a new phase. Moreover the facts are compiled from the *Transactions of the Asiatic Society of Japan* and from the *Mittheilungen der deutschen Gesellschaft für Natur und Völkerkunde Ostasiens*. Most of the *Transactions* are translations from old obscure Japanese documents under the guise of new titles. One thing cannot however be passed unnoticed, that is the footnote on p. 72, which runs as follows:—"The Mikado may have, according to the Land Statute, 12 wives; the nobles, 8; the samurai, 2; the commons, 1." No such law ever existed in Japan. We are neither Mormons nor Mohammedans!

The fourth chapter deals with the political and social changes since the year 1868. It presents nothing new, except some odd remarks of a fanciful nature. The full accounts are already worked out by Le Gendre in "Progressive Japan," in Griffis's "Mikado's Empire," and lastly in Rein's "Japan." On p 105 it is stated that the Japanese Government lays a heavy tax upon the farmer which may amount to half what he has won by patient labour; this oppressive measure would hinder future agricultural progress. In reality the legitimate tax is only 2½ per cent. of the net product.

The last (fifth) chapter treats of the historical development of foreign commerce and the balance of exports and imports. It is seen from the elaborately compiled tables that Japan is now in a favourable condition. In spite of the author depreciating and underrating what the Japanese have done, and the apparently incurably unfavourable physical conditions of the country, the author has, in the concluding chapter, a somewhat reassuring statement. He says that Japan will gradually produce more and more agricultural products if the heavy tax is taken off and serviceable roads are constructed throughout the interior. If this should be the case, the buying power of the country will be increased, and Germany will have to look for an opportunity to engross the export commerce. I must remark, lastly, that the author seems to me not fair-

minded in doing a great injustice to the Government of Japan, by which he was temporarily employed.

Munich B. Kotô

NOTES

At the annual general meeting of the Society of Arts, which was held on the 27th ult., Sir William Siemens being in the chair, the following resolution relative to the death of Mr. Spottiswoode, who was a vice-president of the Society, was passed:—"That this meeting of the Society of Arts desires to express the deep regret with which it has received the news of the death of Mr. William Spottiswoode, one of its vice-presidents, and its sense of the loss which the Society has sustained by his decease. In him England loses one of her most remarkable men of science, science itself one of its greatest ornaments, and all who knew him a sincere and valued friend. Besides devoting his own time and thought to the advancement of knowledge, he was ever ready to lend to all engaged in like pursuits the assistance of his experience and his wise counsel. In thus placing on record their own appreciation of his services, the Society desires to express its feelings of sympathy with his widow and his family, and also with the Fellows of the Royal Society, of which he was the honoured and beloved President."

The report of the Council for the past year, which was then read, makes it abundantly evident that the useful work of the Society is being carried on as successfully as heretofore. The *conversazione* of the Society previously fixed for the 11th inst. has been postponed to the 25th. On that day it will be held at the Fisheries Exhibition, South Kensington, when the Prince and Princess of Wales will be present.

Intelligence has been received from Vivi, on the Congo, of the sudden death of the well-known Swedish explorer, Capt. T. G. Een. Mr. Een, who was on his way to join Mr. Stanley on the Upper Congo, fell down dead from heart disease, just as the signal for his caravan to start was given.

The Vienna Academy of Sciences offers two prizes of 1000 florins each (about 84*l.*) for the best treatises (1) on the capacity of various crystals for conducting electrical currents; and (2) on the chemical constitution of albumen matter.

The well known Russian merchant Sibiriakoff is about to send another vessel to the Siberian rivers this summer. This is the steamer *Obe*, built of Bessemer steel at Motala in Sweden, and which will leave Gothenburg this week. The vessel, which is provisioned for sixteen months, is commanded by the Russian Capt. Weide, who has for many years sailed on the Yenisei and Lena. She will proceed to Tromsö, where she will meet his other steamer, the *Nordenskjöld.* A schooner with building materials will accompany the steamers as far as Novaya Zemlya, where it is intended to erect some storehouses at Yugor Scharr for the reception of cargoes when ice prevents the approach to Obi or Yenisei. At Novaya Zemlya a member of the expedition, Capt. Grönbeck, with two Samoyedes, will be left behind to study the ice and make meteorological observations during the winter. The *Obe* and *Nordenskjöld* will proceed to Port Dickson and the River Yenisei, in the mouth of which, in the Sastorovsky, the *Nordenskjöld* discharges her cargo, viz. merchandise, and loads a cargo of Siberian produce, with which she returns to Europe. The *Obe* proceeds up river with what cargo she can carry as far as Yeniseisk, and remains there for river navigation during next summer.

M. Thollon is now working in the Observatory at Paris. We are informed that the Pic du Midi Observatory is making great progress towards completion, and that Admiral Mouchez, M. Thollon, and other astronomers will visit it towards the end of August.

The monthly meeting of electricians has developed into a new institution, which is to be called Société des Électriciens. A committee has been established for determining the regulations to be proposed at a general meeting next October. M. Cochéry, Ministre des Postes et Telegraphes has been appointed honorary president of the society.

A remarkable instance of the fidelity and sagacity of the dog happened on Friday last at Milford Haven, and is recorded in the daily papers. Two men named Davies and Taylor were out in a boat which was swamped. The former of these was the owner of a dog, and whilst the men were struggling in the water the animal caught hold of Taylor with the object of supporting him; finding, however, that it was not his master to whom he was rendering this assistance, he relinquished his grasp and went to the aid of Davies, his master, supporting him until he was rescued by a passing steamer, the other man being drowned.

On June 13 at about 2 p.m. an earthquake was felt in the neighbourhood of Vossevangen in Norway. There was one continuous shock lasting several seconds, accompanied by a noise as that of a heavy train passing.

A new electric boat, exceeding in size all that have hitherto been designed, is now being fitted up at Millwall by the Electrical Power Storage Company, and is, we understand, nearly ready for her formal trial trip. The new craft is of iron, and measures forty-six feet in length. Her "engine" is a Siemens' dynamo of the D2 type, and works direct on the screw shaft without any gearing. The screw is of unusually narrow pitch, in order to enable the dynamo to run with a high velocity. She carries sixty-five accumulators of the Faure-Sellon-Volckmar pattern of the same size as those used in the smaller electric boat constructed last autumn by the same company. In the private trials made, a speed of eight miles per hour was maintained. This boat will be sent to Vienna, and will doubtless attract much notice at the forthcoming Electrical Exhibition in that city.

Rignold's panorama of the Arctic regions will be exhibited at the Royal Victoria Coffee Hall during the present month. This panorama, which was painted by the late Clarkson Stanfield, R.A., has the reputation of being the finest marine painting extant.

Under the title of "Hardy Perennials and Old-fashioned Flowers" Mr. L. Upcott Gill of 170, Strand, has issued the first number of what will be, if carried out on the lines here laid down, a rather bulky book, and moreover an expensive one, inasmuch as the number before us, which bears date April, is priced at 6*d.*, contains only forty-eight pages, and proceeds only to Cal. in an alphabetical arrangement of the names of the flowers which are recommended for cultivation. The aim of the work is a good one, namely, the bringing to notice many flowers for cultivation in our gardens that are now totally neglected or forgotten. Many old familiar friends are brought to mind in glancing through these forty-eight pages. The arrangement of the plants in alphabetical order of their scientific names is the best that could have been adopted. The wholesale use of capital letters for the specific names should be altered, and more care should be taken in the spelling, such mistakes occurring as *Achillea Ægyptica* for *ægyptiaca*, *Calthus* for *Caltha*, &c. Some of the figures also are extremely poor.

Since the above was written we have received the June number of this little work, which brings it down to *Helleborus* or the Christmas Rose. In this latest number the same lavish use of capitals occurs for the initial letter of the specific name

and the average number of mistakes also occurs. Thus under the genus *Funkia* the common name given is *Planting* Lily, whereas it should be *Plantain* Lily, a name that has been quite recently accorded to these plants by a gardening contemporary.

THE annual Reports of Colonial Botanical Gardens are so frequently reaching us and the matter contained in them is of such value and importance that we regret we have not space at our disposal to give a more extended notice of some of these records of scientific work in our widely spread dependencies. Two of these reports lie before us, namely, that of Dr. Trimen on the Royal Botanic Gardens, Peradeniya, Ceylon, for the year 1882, which report is dated at Peradeniya on January 1 of the present year, and that of Mr. Charles Ford on the Hong Kong Gardens, or rather on his work as Superintendent of the Botanic and Afforestation Department, Hong Kong. A large portion of Dr. Trimen's report is given to the consideration of economic plants, the first mentioned being coffee. Under this head it is with no satisfaction we learn that "Leaf disease has in no degree diminished, and the continued failure of crop during the past year has added to the difficulties of all concerned in the planting enterprise of the colony." Dr. Trimen continues, "No combined effort whatever to prevent the disease on the lines indicated by its known nature has been even attempted, whilst the waste of money and time in local applications of 'cures' has continued. As at the same time high cultivation and liberal manuring have become generally impossible from pecuniary necessities, the existing state of things, however much to be lamented, cannot be considered surprising. A remarkably wet season, too, has aggravated the condition of the badly nourished trees, and the low prices ruling for coffee have intensified the loss by short crops. Thus the cultivation of coffee has been in many places found not to cover expenses, and the necessity of growing other products has been more than ever forced upon proprietors." From this we gather that the prospects of coffee cultivation in Ceylon are anything but promising, and with regard to Liberian coffee, upon which the hopes of planters were at one time founded, we find that it likewise has had to bear the severe attacks of leaf disease, and consequently rises and falls in the estimation of planters. In suitable soils and localities, however, it does well, and the old trees now between eight and nine years old, though badly diseased, show no diminution in their crop-bearing capabilities. No record however is kept of the exports of Liberian coffee from Ceylon distinct from the produce of the other kind. Dr. Trimen remarks that the *Hemileia* not unfrequently attacks the fruits of Liberian coffee. As might be supposed, the subject of cinchona cultivation occupies a large portion of the report, and next to it comes tea and cocoa. The past year, we are told, has witnessed a very striking rise in the export of the first-named beverage, the exports amounting to 623,292 lbs., an advance on the previous year of 345,702 lbs. Tea estates have been opened at all elevations, and many old coffee estates not suited for cinchona culture are now cropped with tea. Indiarubber, guttapercha, and many other industrial and medicinal plants come under Dr. Trimen's review of a year's work at Ceylon, proving once more, if proof were needed, the value of the Peradeniya Gardens amongst others in promoting the advance of applied botany; and the same may be said of Mr. Ford's report of the Hong Kong Garden, for we find there that a considerable amount of attention has been given to the growth of such plants as *Cinnamomum cassia*, the tree furnishing Cassia Lignea of the London market, the Chinese varnish tree (*Aleurites cordata*), and the mahogany tree (*Swietenia mahogani*).

A NEW form of dry pile has been described in *Wiedemann's Annalen* by J. Elster and H. Geitel. In the previous forms of dry pile, from the time of Zamboni downwards, the disks of foil and paper have been placed in glass tubes, with the result that the film of moisture collecting on the inner surface of the tube has always exerted a more or less destructive influence. In the new dry piles the disks are strung with a sewing-needle upon a single strong silk thread, which insures better insulation. Messrs. Elster and Geitel have made the very interesting observation that piles of this type can be charged from a Holtz machine. An ordinary Zamboni pile of 11,000 pairs of disks of tin and copper foil gave, after ten minutes' charging, sparks one millimetre long, and was able to illuminate a small Geissler tube for some time with a discharge continuous at first and afterwards intermittent. Following up this analogy dry piles were constructed on the plan of a Planté battery. Thin disks of lead foil alternating with disks of silk paper painted with a mixture of soluble glass and peroxide of lead were strung upon silk strings. A charged pile of 7000 such plates gave for ten minutes a spark one millimetre long; and after twenty four hours still showed electrification.

THE last report of the British Consul at Tientsin supplies us with information respecting the only colliery at present in complete working order in China. This is at Kaiping, not far from Peking. The coal is said to belong to the true carboniferous system, and the bed dips to the south some forty-five degrees, forming a large basin under the Gulf of Pehchihli. No fear is entertained that the measures will run short. So far as has been ascertained, the coal bearing stratum is about one thousand feet, containing thirteen seams. During the winter months two hundred tons per day of the inferior kinds of coal can be sold to natives in the vicinity, who use it for pottery, brick, and lime-kilns; indeed, one of the most important results achieved by the opening of the colliery has been the revival of several industries in the vicinity which were languishing or extinct, on account of the surface coal of the district being mostly worked out, and the price of other coal being too high to be used with profit. In connection with the colliery is a small railway, the only one in all China. Its length is but six and a half miles, and at the terminus the coal is placed in barges and carried down by canal. After a little opposition the locomotives were allowed to run freely. But ironworks, which it was also intended to start, could not get over the superstitious opposition raised on the score of the proximity of the Imperial tombs, and the consequent geomantic disturbances caused by sinking shafts, &c. The iron ore is said to exist in enormous quantities, but it is not easy to work owing to the amount of silica present.

WE have received Parts 4–6 of the *Transactions of the Yorkshire Naturalists' Union*, which do credit to that energetic body of local naturalists. They are entirely occupied by lists and notes concerning the fauna and flora of this, our largest, county, so arranged that each subject has a separate pagination, and most of the authors give evidence of considerable bibliographical research; some of the articles are of far more than local importance.

FROM several parts of Sweden the appearance of an unknown caterpillar, which consumes the crops, is reported. Its length is from one inch to one and a half, and its colour grey-brown with green stripes. In one place it put in an appearance immediately after a violent storm with rain. The Academy of Agriculture has despatched an entomologist to visit the places from which it is reported.

THE German Society of Analytical Chemists offers two prizes, of 25*l*. and 15*l*. respectively, for the best treatises on cocoa and cocoa manufactures, with reference to their commercial value and efficacy in nutrition.

ADMIRAL MOUCHEZ will not be reappointed at the expiration of his term of office. The Government is fully convinced that it is useless to resort to this formality, and that it would be better to continue his appointment by *toute reconduction*, as is customary in France under peculiar circumstances.

MISS FIELDE, an American missionary lady stationed at Swatow, has, it is stated, completed a voluminous dictionary of the Swatow dialect, which will be published shortly.

THE additions to the Zoological Society's Gardens during the past week include a Feline Dourocouli (*Nyctipithecus vociferans* ♀) from Columbia, presented by Mr. H. H. Thiele; an Indian Civet (*Viverricula indica* ♀) from India, presented by Capt. Wilson; two Squirrel-like Phalangers (*Belideus sciureus*) from Australia, presented by A. Pretyman; a Vulpine Phalanger (*Phalangista vulpina*) from Australia, presented by Mr. J. E. Dothie; an Australian Crow (*Corvus australis*) from Australia, presented by Mrs. A. H. Jamrach; a Nicobar Pigeon (*Calænas nicobarica*) from the Philippine Islands, presented by Mr. Hugh Low; two Common Gulls (*Larus canus*), British, presented by Mr. C. W. Jervis Smith; a Spotted Mud Frog (*Pelodytes punctatus*) from the South of France, presented by Mr. H. P. Cambridge; a Cape Ant Bear (*Orycteropus capensis*), twelve Derbian Zonures (*Zonurus derbianus*) from South Africa, two Canadian Beavers (*Castor canadensis* ♂ ♀) from Canada, a Viperine Snake (*Tropidonotus viperinus*) from North Africa, a Tree Boa (*Corallus hortulanus*) from South America, purchased; a Hairy-footed Jerboa (*Dipus hirtepes*) from Arabia, a Simon's Dwarf Jerboa (*Dipodillus simoni*) from Algeria, received in exchange; a Japanese Deer (*Cervus sika* ♂), a Hybrid Syrian Wild Ass (between *Equus hemippus* ♂ and *Equus onager* ♀), an Impeyan Pheasant (*Lophophorus impeyanus*), four Amherst's Pheasants (*Thaumalea amherstiæ*), bred in the Gardens.

ON THE CAUSES OF GLACIER MOTION[1]

THE question of the causes which produce the movement of glaciers, which was at one time so eagerly discussed, would appear to have slumbered for the last ten years. This cannot be said to arise from the fact that a perfectly satisfactory theory has been developed, and recognised as such by all inquirers. The ambiguous allusion to the subject in Sir John Lubbock's presidential address to the British Association is an evidence that such certainty has not been attained. It is indeed generally supposed that the fact of the melting-point of ice being lowered by pressure is somehow at the root of the matter; but a full explanation of the origin of this pressure in the case of glaciers and of the mechanical features of the problem has yet to be given. I may therefore be pardoned if I draw attention to a different solution, proposed not by myself but by one of the greatest of English mechanicians. My apology for doing so is that I approach the question as an engineer, not as a physicist; and that it is in its essence, as will be shown immediately, a mechanical rather than a physical problem.

The following are leading facts of glacier-motion which must be accounted for by any valid theory on the subject:—

(1.) The phenomena of the movement of a glacier are simply those of a solid body in a state of flow.

(2.) The present glaciers of Switzerland and Norway, which are the only ones which have been critically examined, are mere shrunken fragments of the glaciers of the Great Ice Age. To take one instance, the present glacier of the Rhone is about 6 miles long and perhaps 500 feet deep; but the old glacier of the Rhone, which abutted against the Jura, was 120 miles long, and must have been 2000 to 3000 feet deep. The movement of such glaciers as this must also be accounted for in any satisfactory theory.

(3.) The glaciers of the present day are not confined to the temperate region; they are found in much larger numbers and of much greater size in the Arctic regions.

(4.) Both in the temperate and in the Arctic regions glaciers move in winter as well as in summer, and by night as well as by day.

That a glacier is in a state of flow was first proved by Forbes, and has since been confirmed by the measurements of Tyndall and others. Whilst the whole mass moves downwards, the top moves faster than the bottom and the sides than the middle; the upper layers must therefore be continually shearing over the lower, and the medial over the lateral. A glacier, being a body in a state of flow, must move under the influence of forces powerful enough to overcome its resistance, and so produce this condition.

The general phenomena of the motion of a glacier are exactly reproduced when a viscous body moves through a channel under the influence of its own weight. We have therefore first to inquire whether the shearing resistance of ice is sufficiently low to enable us to regard a glacier as a viscous mass.

The only experiments known to me on the shearing resistance of ice, are those of Moseley (*Phil. Mag.*, January, 1870). He found that, with pressures from 100 to 110 lbs per square inch, cylinders of ice sheared slowly across the two planes in contact, sliding over each other without losing continuity. The distance sheared through was about five-eighths of an inch in half an hour. A load of 119 lbs. per square inch was sufficient to shear through a cylinder of $1\frac{1}{2}$ inches in diameter in two to three minutes. From these experiments it would appear that the lowest shearing stress which will cause ice to flow is about 100 lbs. per square inch; but sufficient time was not allowed in the experiments to make this a matter of certainty.

There is another way in which the shearing resistance of ice may be tested. In the case of a block of ice of vertical sides, gravity of course produces a shearing resistance along all planes passing through the base. Let h be the height of such a block in feet, and consider the shearing force due to gravity on any square foot of a plane making an angle θ with the vertical. This shearing force is given by—

$$\frac{\dfrac{wh \times h \tan\theta}{2} \times \cos\theta}{h \sec\theta} = \frac{wh}{2} \sin\theta \cos\theta.$$

This expression is a maximum when $\theta = 45°$, and its value is then—

$$\frac{wh}{4}.$$

What is the greatest height at which a vertical cliff of ice will stand? I am not able to state this precisely, but it is very considerable. Mr. Whymper mentions crevasses in South America 300 feet deep. Cliffs of fully that height have been seen standing out of water in the case of icebergs, and as so small a part of an iceberg projects above water, these cliffs probably extend below to a considerable depth. Taking, however, only 300 feet for the value of h, or for the maximum height of an ice cliff, this would give about 30 lbs. per square inch as the lowest shearing force upon a plane of ice which would cause it to assume the condition of flow.

Let us now suppose a glacier of thickness a, lying upon a slope whose inclination to the horizontal is β; then the force per square foot, tending to shear the ice at its junction with the slope, is clearly $aw \sin\beta$.

Supposing $\sin\beta$ to equal $\frac{1}{4}$, and that the shearing resistance is 30 lbs. per square inch, we get $a =$ about 290. Hence we may say that a glacier lying on a slope of 1 in 4 will not move at all under its own weight unless it be at least 300 feet thick, and that, if it be more than this, the upper 300 feet will move as one solid mass, the part below alone representing the conditions of flow.

It is needless to say that there are hundreds of glaciers which are less than 300 feet thick, and which at no part of their course have a slope anything approaching 1 in 4.

We have now to show that the theories generally propounded for glacier action are all of them negatived by some of the foregoing considerations. These theories may be stated as follows:—

(1.) The glacier simply slides over its bed as a solid body. This is negatived by the fact that some parts move faster than others.

(2.) The glacier flows under the action of its own weight, exactly as a viscous body flows. This is the theory of Forbes. It is disproved by the facts given above, which show that even on a slope of 1 in 4 a glacier would not flow unless it was at least 300 feet thick.

(3.) The glacier moves by the crushing of its base. This has been disproved by Moseley's experiments, which showed that the crushing resistance of ice was considerably higher than the shearing resistance.

(4.) The glacier moves by the melting of its base. This is the theory of Hopkins. He placed a block of ice at 32° F. on a slab at a small angle, and found that it slowly descended as it melted. On this view the bottom of the glacier must always

[1] Paper by Walter R. Browne, M.Inst.C.E., read at the Royal Society, June 15, 1882.

be in a melting state. But glaciers are of all sizes and thicknesses, and they move in winter as well as summer. Bessels ("Die Amerikanische Nordpol Expedition," p. 398) measured the motion of an Arctic glacier (not apparently very thick), in the month of April, which is just when the winter cold would have sunk deepest, and found it considerable. Again in the *Zeitschrift des deutschen Geologischen Gesellschaft*, vol. xxxiii. p. 693, is an account of measurements of a Greenland glacier, both in winter and summer, which show that the motion in winter is only 20 per cent. less than in summer. It has been suggested to me that the interior heat of the earth may be sufficient to keep the bottom of the ice from freezing; but this cannot apply near the sides, where the ice is shallow, and the freezing of a very small strip on each side would be sufficient to keep the whole mass from descending. Moreover, this cause would apply to masses of snow as much as to ice. But it is known that masses of snow, though lying on steep slopes, do not descend in this way, even in summer, but melt away where they lie.

(5.) According to the theories of Tyndall, Croll, and others, the glacier moves not in the form of ice but of water. These theories are based on the known fact that the freezing point of ice is lowered by pressure. Hence it is supposed that certain parts of a glacier are continually being exposed to so much pressure that they melt. The water escapes downward, and the pressure being relieved it freezes again. The continuity of the glacier is further kept up by the process of regelation, according to which two pieces of ice, if placed in contact, form into one solid mass.

The advocates of this theory hardly seem to consider how very small the lowering of the freezing point is for any ordinary pressure. It is only ·0075° per atmosphere. In other words, it will require a pressure of 2000 lbs. per square inch to liquefy ice at 31° instead of 32°. This is equivalent to the weight of a column of ice about 5000 feet high. It is needless to ask whether such a pressure can exist within an ordinary glacier, while on the other hand glaciers undoubtedly move at temperatures far below freezing point—in the Arctic regions below zero.

It seems to be generally supposed that the pressure in the lower part of a glacier is due to the steeper upper portions: the glacier channel is spoken of as a mould, through which the ice is forced by pressure from behind. But in the upper glacier, slopes of ice or *nevé* are not uncommon at angles of 30° or even more. Such slopes usually do not even touch the more level parts of the glacier below them, but are separated from them by a wide, deep crevasse called a Bergschrund. Of this the well-known ice wall of the Strahleck is a conspicuous example. In other cases such slopes do not end in a glacier at all, but die away upon the mountain side. It is certain, therefore, that ice or *nevé* is able to maintain itself at a high angle upon its slope of rocks, and therefore cannot possibly exercise pressure upon the parts of the glacier far in advance of its foot. The fallacy of this idea may be further illustrated by referring, not to modern glaciers, but to those of the Great Ice Age. Can we suppose that the pressure of the snows about the sources of the Rhone was sufficient to drive that glacier down the valley to Martigny, round a sharp angle to the Lake of Geneva, through the bed of that lake, and on to the slopes of the Jura, a distance of more than 100 miles, in which the average slope was about 1 in 200; giving a propelling force per ton of ice of about 11 lbs. only?

All these theories have this in common, that they regard gravity as the sole and direct agent in the movement of glaciers, and the above considerations seem to prove that it is an agent far too weak for the work it has to do.[1]

The only other agent which has been suggested, or seems likely to be suggested, to account for the motion of glaciers, is heat. This suggestion, as is well known, is due to the late Canon Moseley, F.R.S., and was to some extent worked out by him in papers published in the *Phil. Mag.*, 1869 and 1870.

The mode of operation, on this theory, is well known. Ice is here considered merely as a solid body, obeying the ordinary laws of expansion and contraction under differences of temperature. This it is known to do, the coefficient of linear expansion, for 1° F., being ·00002856 (Moseley, *Phil. Mag.*, January, 1870), which is very high. When a mass of ice, such as a glacier, suffers a rise in temperature, either through conduction or radiation, it will expand; this expansion will take place mainly in the direction where movement is easiest, that is, down the valley. If from any cause the temperature falls, the glacier will again contract; but since the expansion is assisted by gravity whilst the contraction is opposed by it, the latter will be somewhat less in amount than the former, and when the ice has returned to its original temperature, its centre of gravity will have moved a certain small distance down the valley. By such alternate expansions and contractions the glacier moves gradually from the top to the bottom of its course.

That variations of temperature do take place in a glacier cannot be doubted, whatever be the condition in which it lies. This granted, the fact that it should move in the way described appears to me no more surprising than that the sheets of lead on which Canon Moseley made his well-known experiments did so move; and that the motion thus produced is of the character which answers to all the facts of the case, so far as they are at present known, can, I believe, be established.

The controversy occasioned by Canon Moseley's articles was unfortunately terminated by his illness and death, before the matter had been fully cleared up. The main objections urged to his theory were two. The first was that a glacier is not one continuous body (as assumed by Canon Moseley in his mathematical investigation), but is broken up into many parts by crevasses. But in the first place, the assumption above mentioned is merely one of convenience, and not in the least necessary to the theory. A detached piece of ice would move in the same way as a glacier, or as the sheet of lead did in Canon Moseley's experiments. Secondly, if a glacier is anywhere divided in its whole thickness by a crevasse, this is absolutely fatal to the gravitation theories, since there can be no pressure between the portions above and below this division. The only possible explanation of crevasses, on these theories, is that they are due to the glacier bending over a convex part of its bed. In that case the bottom half will be in compression, and only the top half in tension, so that the crevasse cannot possibly extend more than half way through the thickness.

The second objection was that the conductivity of ice is low; hence the effect of the heat would be confined to the layers near the surface, and could not account for the motion of the glacier as a whole. This objection does not seem to be confirmed by careful reflection upon the way in which such forces act. Let us suppose a glacier 100 feet deep, of which each successive foot expands and contracts alike throughout, but adheres with a definite shearing resistance to the layers above and below. Let there be a rise in temperature, which does not extend beyond the uppermost 10 feet. This layer will expand, and if it were free would expand to the full amount due to the increase in temperature. But its lower surface is not free. In expanding it will therefore drag the next layer after it, or in other words will cause it to expand also. The amount of expansion, however, will not be so great, because there will be a certain shearing extension at the plane of division between the two. The second layer will similarly cause an expansion in the third, and so on to the bottom. In consequence the energy which would all have been exerted on the top layer, had that been free, will be distributed over the whole of the layers; and the extension of the top layers will of course be much smaller than it otherwise would have been. Should the temperature then remain constant, the layers will retain their position, and adapt themselves to the new circumstances. If the temperature falls, the layers will contract, but from the now opposing effect of gravity they will not return to their original position. The top layer, which has extended furthest, will be the furthest below its original position; the second layer next and so on. If we suppose the layers to be indefinitely thin, we have the condition of things in an actual glacier. The ice in any vertical section will, on the whole, move down the slope, but the top will move faster than the middle, and the middle than the bottom, exactly as it is known to do. The same holds with regard to a horizontal section. At the sides the ice will be held back, not only by the friction, but also by the protuberances of the rock, which compel the ice to shear over them. Hence the velocity there will be retarded, and will be less than that in the middle, which is comparatively free.

A more important objection remains to be considered, which is this. On the present theory the motion at any point on the surface of a glacier will be not continuous, but oscillating alternately downwards and upwards, and the net distance by which it has descended, say, in a day, will be a mere fraction of the total distance through which it has moved in that period. If

[1] Another evidence against pressure from behind as a cause of motion is furnished by the very small size of many glaciers. Some of these, notably those of the class called "glaciers remaniés," are only a few hundred yards long, and cannot be many feet deep.

so, this alternate motion ought to have been noticed in the various observations which have been made upon glaciers, and this does not appear to have been the case. But, in reply to this, it may be remarked that most of the observations have only given the net movement of points on the glacier during intervals of a day or more, and therefore would not show the oscillations. Again, such observations have always been at points near the end of a glacier. Now the variations in temperature of a glacier will be very different at different parts, and the motion of the end of the glacier will, to a great extent, show the average result of these different advances and retreats in different parts of the higher regions. This average result will, of course, be a steady progression down the valley, and the oscillatory movement at the end of the glacier may be so much masked by this as not to be readily observable. Lastly, it may be suggested as possible that a certain amount of expansion by heat may have the effect of giving a *set* to ice, so that it does not return to its original length when brought back to the same temperature. If this be so, the oscillations would be much less marked, and at the end of the glacier would probably be indistinguishable.

I may now draw attention to some phenomena of glacier action, which are explained by the heat theory, but which do not seem explicable on the gravitation theory.

(1.) It is well known that glaciers, when they emerge from a narrow gorge into a comparatively wide valley, spread out into a fan shape. The Rhone glacier is a well-known instance. A still better one is a small glacier in Norway, mentioned by Prof. Sexa, which spreads out to five or six times its previous width. Now the effect of gravity, acting on a mass as a whole, is to carry it in one single direction, that of the steepest slope. The only way in which gravity can produce such a spreading out is by the parts of the glacier shearing over each other in the manner of a viscous solid. But the phenomena of ice cliffs, as mentioned above, show that ice does not spread from this cause, so that the fact seems impossible to explain by gravitation alone. On the heat theory it is, of course, perfectly easy: the expansion and contraction will take place in all directions where there is freedom to move.

(2.) Connected with this phenomenon is that of the longitudinal crevasses seen near the edges of glaciers, and particularly where they spread out in the manner just described. Now on the gravitation theory, as remarked above, the only possible explanation of a crevasse is that the ice is bending over a convex surface, and that its upper part is thus placed in a state of tension, under which it breaks. Since, on the gravitation theory, every part of a glacier is exposed to a severe pressure from behind, this explanation does not fit very well even for transverse crevasses; but to longitudinal crevasses it is clearly inapplicable, since the bottom of a valley is seldom or never convex in the direction of its width. On the heat theory the explanation is simple. We may suppose the heat energy communicated per square foot of surface to be about the same, whether near the middle or edge of a glacier. This energy is expended in producing an expansion throughout the whole thickness of the glacier, as described above. Hence the smaller this thickness, the greater will be the amount of expansion, and the greater therefore the net motion which results. Hence the thinner parts of a glacier will always be tending to tear themselves off from the thicker, and thus longitudinal crevasses will frequently be found.

(3.) The striæ which are so marked a feature of glacier-worn rocks become more easily explained on this theory. I have seen such striæ, even in the hard hypersthene of Skye, which were a considerable fraction of an inch in depth. When we consider the enormous force necessary to plough out such a furrow in hard rock, it is almost impossible to believe that it was done by the simple passage over it, once for all, of a stone imbedded in the ice. If, however, the stone descended by a series of oscillations, so that it passed many times over the same spot, this difficulty is greatly lessened.

(4.) In conclusion I may point out that the advocates of the gravitation theory are bound to explain what becomes of the heat energy which is poured into a glacier. When the sun is shining this radiant energy is always very large, although the temperature of the air may be low. In such cases the glacier does not melt; it is perfectly clear that it must expand, as any other solid must expand under the action of heat. If so, it seems unreasonable not to hold that the gradual descent by alternate expansion and contraction must follow, as it is known to follow in the case of other materials.

On the subject of the motion of Arctic ice, Dr. Rae, F.R.S., has kindly permitted the publication of the following particulars:—

"When in Greenland, in the autumn of 1866, I was ice-bound at the head of one of the fiords, and slept a couple of nights at an Eskimo's house. A glacier about half a mile distant was then in full activity, the movement of which might, I believe, have been as visible to the eye as it certainly was audible to the ear.

"My own idea is that Arctic glaciers must have a downward motion more or less during the whole year, summer and winter. I believe the alternation of heat and cold—or, I should rather say, of temperature—would of itself cause motion, especially near the upper surface.

"We know that ice 2 or 3 feet or more thick contracts very considerably in a few hours by a sudden fall of 15 or 20 degrees of temperature. I have found cracks in Lake Winnipeg 3 or 4 feet wide, formed by this cause during a single night, almost stopping our sledge journey. This gap soon freezes up. Then the weather gets milder, the ice expands, and with the new additional formation is too large for the lake, and is forced up into ridges. This process goes on at every 'cold snap,'[1] alternating with milder weather. Now supposing a glacier for 10 or more feet of its depth contracts by cold, as lake ice is known to do, it will get a series of cracks probably in its longest axis, say from inland seaward; the first snowdrift will fill up these cracks or some of them, and this filling up will to some extent perform the same office as the freezing of the cracks in the lakes. The longitudinal extent of the glacier will be increased. A snowstorm always brings milder weather, which would expand the glacier, but as this expansion would naturally tend downhill, instead of up, the whole motion would be downwards. But even if the cracks I mention did not take place, the contraction by cold would pull the ice downhill, not up, whilst the expansion by increase of temperature would tend to *push* the glacier downhill, so that these opposite actions would produce similar effects in moving the glacier, or such part of it as could be acted upon by external temperature, downwards.

"I may also add that when a crack, however slight, is formed by contraction, the cold is admitted into the body of the glacier, and increases the contracting power or influence."

SCIENTIFIC SERIALS

Annalen der Physik und Chimie, xix., part 4.—Electrical experiments: electric pressure on solids, by G. Quincke. This paper forms a continuation to a series of experiments in electrostatics published by the author in previous numbers of the *Annalen*, under the title of "Electric Expansion." It is illustrated with twenty-six cuts, and will be followed by a communication on the resistance of insulating fluids to electric force.—On electric disturbance at contact of gases with bodies in combustion, with four illustrations, by Julius Elste and Hans Geitel. The authors arrive at the general conclusion that all flames may be regarded as streams of hot gas, which generate negative electricity in burning electrodes introduced from without, as well as in small bodies in combustion suspended in them.—On electric vibration, and more especially on the phenomena of polarisation produced by vibratory movements, with four illustrations, by A. Overbeck.—On the dependence of gases as heat conductors on the state of the temperature, with three illustrations, by A. Winkelmann.—On the fundamental equations of E. Ketteler's theory of optics, by W. Voigt. The author shows that, so far from flowing from the principles of the doctrine of elasticity, Ketteler's fundamental equations are diametrically opposed to them.

THE *Journal de Physique* (May, 1883) contains the following original papers:—On the difference in barometrical pressure at two points in the same vertical line, by J. Jamin.—On the action of heat upon boracite and upon sulphate of potash, by E. Mallard.—On the penetration of actinic rays into the eye of man and that of vertebrates, and on their vision of ultra-violet rays, by E. de Chardonnet.—On a new apparatus for verifying the laws governing the fall of bodies, by M. Paquet.—On an experimental demonstration of the unequal velocity of the transmission of sound in gases and solids, by F. Griveaux.

[1] "Cold snap," an American term meaning a rather sudden increase of cold.

Zeitschrift für wissenschaftliche Zoologie, Bd. xxxviii. Heft 2 (April 27, 1883), contains:—Contribution to a knowledge of the infusoria, by Dr. G. Entz (Plate 8).—On the primordial skull of some mammalia, by Fred. Decker (Plate 9).—On some Cœlenterata of the South Sea, by Dr. R. v. Lendenfeld, of Melbourne, Part II.—On new Aplysinidiæ (Plates 10 to 13).—On the embryology of Hydra, by Dr. A. Korotneff (Plate 14).—On the larval development of *Phoxichilidium plumulariæ*, nov. sp., by Dr. R. v. Lendenfeld, with woodcuts.

Rendiconti of the *R. Istituto Lombardo di Scienze e Lettere*, May 10 and 17.—Preliminary inquiry into Zanardelli's proposed penal code (continued), by Prof. A. Buccellati.—On the commentaries of Gaius and the paraphrase of Theophilus, by Dr. C. Ferrini.—A few remarks on the first five sections of Ricardo's chapter on value, by Prof. E. Nazzani.—On the mortality of infants in the various provinces of Italy, by Prof. G. Sormani. The death-rate during the first month is shown to be much higher in winter than in summer, and in the northern than in the southern provinces. Thus: 50 per 1000 in Palermo, 190 in Padova, medium for the kingdom 91·9.—On the formation of the primitive line and primitive cleft in the gastrula of the Mexican axolotl, by Prof. G. Bellonci.—Alterations in the lower hollow vein aggravating hepatic cirrosis, by Prof. A. de Giovanni.—Observations on the comet of Brooks made in the Brera Observatory, Milan, by G. V. Schiaparelli.—On a deposit of fossiliferous Pliocene clay recently discovered near Taino, to the east of Angera, in Lombardy, by Prof. T. Taramelli.

SOCIETIES AND ACADEMIES

London

Linnean Society, June 21.—Prof. P. M. Duncan, F.R.S., vice-president, in the chair.—The following gentlemen were balloted for and elected Fellows of the Society, viz. Messrs. E. J. Baillie, J. Borland, K. McKean, E. C. Malan, and H. A. A. Nicholls.—A specimen of *Polyporus sulfureus* was exhibited for the Rev. A. A. Harland, obtained from the stem of a yew tree in the Cliveden Woods, Bucks.—A series of fossil fruits, &c., from Australia were shown for Dr. Charles E. Barnard; among these were species of *Phymatocaryon*, *Eisothecaryon*, *Ochthodocaryon*, *Spondylostrobus*, *Plesiocapparis*, and others.—Mr. W. T. Thiselton Dyer exhibited several interesting vegetable economic products, and made remarks thereon. Of a species of wax extracted by Mr. D. Morris of Jamaica from *Myrica microcarpa*, it was stated that while the berries are used for obtaining wax in South Africa, the West Indian fruits had not hitherto been used for this purpose. A gray, camphor-like substance, the product of *Artemisia noxa*, he mentioned as a rare example among the Compositæ; and there was a probability that this camphor was used in the production of Indian ink by the Chinese, and gave the peculiar aromatic odour to the true China ink. A rosary was shown made of fruits of *Trapa verbanensis*, De Not. (locally called Frutti de' Lago), from the Lago di Varese, Italy; also specimens of wax and candles made from *Rhus vernicifera* of Japan; the latter preparation is quite a local industry, which unfortunately is now ceasing on account of the rivalry of the cheap American oils.—The following plants were exhibited, viz., *Arnoseris pusilla* and *Hypochæris glabra*, obtained by Mr. Thomas Howse in West Surrey, and specimens of the Cheddar Pink (*Dianthus cæsius*), which had been grown freely by Mr. C. F. White on his garden wall at Ealing.—A paper on the structure of the hard parts of the Fungidæ (part 2, Lophoserinæ), was read by Prof. Duncan, and another by Mr. R. A. Rolfe of Kew, on the Selagineæ described by Linnæus, Bergius, and Thunberg.—A communication was read from Mr. H. G. Doran, on the malleus of *Rhytina stelleri*, based on a specimen obtained in the voyage of the *Vega*, and exhibited in the Swedish Department of the International Fisheries Exhibition, under the charge of Prof. Smitt of Stockholm. The author concludes that this auditory ossicle in the extinct Northern Sea Cow (*Rhytina*) is larger than in the Manatee (*Manatus*), and therefore it is the largest and bulkiest malleus to be found in the whole section of the animal kingdom where such a bone exists. In the character of its body it resembles that of the Manatee rather than that of the Dugong (*Halicore*); while in the manubrium it differs in *Rhytina* from the other Sirenia, and is far more generalised.—The following paper was taken as read, Notes on some new economic products recently received at the Royal Gardens, Kew, by W. T. Thiselton Dyer. Therein he treats of the West African indigo, the Inhambane copal, and the Ogea gum as exhibited at a previous meeting.—On the testis of *Limulus*, formed a communication from Mr. W. B. S. Benham. He describes the structures in question, noting the apparent isolation of many of the spermatic sacs, and the probability that they are not diverticula of the spermatic duct, but secondarily acquire connection therewith, the two structures being independently developed. He remarks that in no crustacean do the ducts of the generative glands form a network, whereas in the King Crab, as in the Scorpion and other Arachnids, they do.—There followed a paper on the Mollusca of H.M.S. *Challenger* (part xx.), by the Rev. R. Boog Watson. This contains a continued descriptive account of the family Bullidæ, dealing with the genera *Atys* and *Scaphander*, along with the group *Aplysiidæ*, genus *Dolabrifera*.

Zoological Society, June 5.—Osbert Salvin, F.R.S., vice-president, in the chair.—Mr. Sclater exhibited and made remarks on two birds obtained near Lima by Prof. W. Nation, C.M.Z.S., and on a collection of birds made in New Britain, New Ireland, and the Solomon Islands, that has been sent to him for examination by the Rev. George Brown, C.M.Z.S.—Mr. Sclater also called the attention of the meeting to a Condor from Peru, living in the Society's Gardens since 1877, which he was induced to believe was a specimen of the "*Condor pardo*," or *Sarcorhamphus æquatorialis*, Sharpe.—Mr. G. French Angas exhibited a collection of butterflies made during a recent visit to the island of Dominica, W.I.—A communication was read from Prof. Owen, C.B., entitled "Embryological Testimony to General Homology."—A communication was read from the Rev. O. P. Cambridge on some new genera and species of spiders. Eight spiders, representing as many new genera, were described: two of them belonged to the family Theraphoridæ, one to the Drassidæ, and the others to the Thomisidæ. Three of these species were from Ceylon, three from Caffraria, one from New Zealand, and one from California.—A communication was read from Mr. A. G. Butler containing an account of the Lepidoptera collected by Mr. H. O. Forbes in the islands of the Timor-Laut group. Examples of twenty-three species were obtained.—A communication was read from Mr. Herbert Druce containing descriptions of some new species of moths of the families Zygænidæ and Arctiidæ, mostly collected in Ecuador by Mr. C. Buckley. The number of new species described was fifty, belonging to twenty-four genera.—A paper was read by Messrs. Godman and Salvin, containing remarks on the variations of certain species of butterflies of the genus *Agrias*.—Mr. G. A. Boulenger read a report on a collection of reptiles and Batrachians from the Timor-Laut group of islands, formed by Mr. H. O. Forbes. Two new species were described—the one a lizard of the Australian genus *Lophognathus*, and the other a snake of the Indian genus *Simotes*, proposed to be named respectively *L. maculilabris* and *S. forbesii*. The snake was of special interest, as no species of the genus *Simotes* had hitherto been previously known to occur eastward of Java.

Chemical Society, June 21.—Dr. W. H. Perkin, president, in the chair.—The following gentlemen were elected Fellows:—G. S. Bowler, C. Beringer, T. H. Coleman, A. Esilman, H. E. Harrison, C. Hulke, H. Heap, B. Hobbs, C. T. Heycock, W. J. Livingston, B. P. Lascelles, H. R. Mill, M. F. Purcell, J. E. Richardson, F. G. Roberts, W. R. Reffel, A. Smith, E. H. B. Stephenson, A. W. Soward, A. H. Samuel, D. Wilson, and R. Williams.—The following papers were read:—On evaporation in vacuo, by H. McLeod. The author has contrived several forms of apparatus, and in the present paper describes two. One in which the water was evaporated in a glass dish with ground top, at a temperature not exceeding 50°, 50 c.c. evaporated in two hours; a Körtings water pump was used to obtain the vacuum. Instead of the dish a test tube or a combustion tube may be employed. In the second form of apparatus sulphuric acid was allowed to trickle down the tube into which the aqueous vapour passed, and thus the use of a condenser was avoided.—Note on a hydrocarbon and some substitution derivatives from camphor, by H. E. Armstrong.—On the preparation of the pentathionates, by G. S. Shaw. The author has reinvestigated this subject, because Prof. Spring states in *Liebig's Annalen* that he was unable to obtain pentathionates by using the method described by V. Lewes. The author completely confirms the results obtained by Lewes, and has obtained beautifully crystalline salts in which the ratio of potassium to sulphur was as 2 atoms to 5. A note is appended to the paper by Watson Smith.—On the decomposition of ammonium nitrate;

an investigation into the rate of chemical change, by V. H. Veley.—Note on the action of allylic iodide upon phenol in the presence of zinc or aluminium foil, by P. Frankland and T. Turner. Orthopropyl phenol was obtained.—On a new gas burner for heating combustion tubes, by W. Ramsay.—On a by-product of the manufacture of aurin, by A. Claparède and Watson Smith. When aurin is prepared from phenol, oxalic acid, and sulphuric acid, some quantity of white crystals appears on the lids of the aurin-pots. These were examined by the authors, and were found to consist of a phenyl ortho-oxalic ether.

Meteorological Society, June 20.—Mr. J. K. Laughton, M.A., F.R.A.S., president, in the chair.—The following papers were read:—On the structure of the ice-cloud disposed in threads, proposed to be called "cirro-filum," by the Rev. W. Clement Ley, M.A., F.M.S. Of the cirriform clouds one of the most important to the weather forecaster is that to which the author has given the name of "cirro-filum." Having from the time he was twelve years of age carefully studied this cloud whenever visible, and having for the last twenty-five years made it the subject of minute study, he is enabled to bring forward some results which may prove of value. The author then gives, first, a short account of the mode in which he was led to prosecute this study; secondly, a classification of the more recent and reliable observations; and lastly, an explanation of the principal phenomena observed.—Notes on a second series of experiments on the distribution of pressure upon flat surfaces perpendicularly exposed to the wind, by Richard H. Curtis, F.M.S. The results obtained in these experiments agree very closely with those of the former experiments.—On the reduction of wind records, by the Hon. Ralph Abercromby, F.M.S. The author discusses the significance and best method of deducing from anemographic records the total quantity, the quantity from different points of the compass, the relative frequency, the mean and annual velocity, the mean velocity from different quarters, the resultant, and the mean and diurnal direction of the wind.—The spectroscope as an aid to forecasting weather, by F. W. Cory, M.R.C.S., F.M.S.—Note on river temperatures as compared with air temperatures at Greenwich and Bremen, by Robert H. Scott, M.A., F.R.S. The author compares the results given in a recent paper by Sir G. B. Airy on a comparison between the records of the temperature of the Thames and those of air temperature taken at Greenwich with those published by Herr von Freeden for the temperature of the Weser as compared with that of the air at Elsfleth, close to Bremen, for the ten years 1858–67.

Physical Society, June 23.—Prof. Clifton in the chair.—New member, Mr. Stearn.—Prof. D. E. Hughes, F.R.S., exhibited a number of experiments illustrating his theory that a magnet is made up of magnetic molecules each of which is a small magnet. When a magnetic metal is in a neutral state he showed that there is a symmetrical arrangement of the molecules such as to make them satisfy their mutual attractions; not as on Ampère's theory a "higgledy-piggledy" arrangement. Prof. Guthrie stated that a piece of watch-spring magnetised retains its magnetism when impregnated with mercury. Prof. Everett, Mr. W. H. Coffin, and others remarked that Ampère's theory tried to account for the magnetism of the molecules. Professors Perry and Ayrton observed that when soft iron is between red and white hot, it ceases to be attracted by a magnet.—The new absolute sine galvanometer of Prof. Minchin was then exhibited to the meeting by Prof. G. Carey Foster. It is intended for the Cornell University, and measures less than the E.M.F. of a Daniell cell. The principle of the instrument was described at a former meeting of the Society. Prof. Ayrton, Lord Rayleigh, Mr. Coffin, and Prof. Clifton offered some remarks on the apparatus.—A note on the induction-balance effect and the densities of alloys of copper and antimony, by Mr. George Kamensky, A.R.S.M., was then explained by Prof. Chandler Roberts. These experiments were to determine whether the curve of the electrical resistance of the copper antimony alloys would be a straight horizontal line, U-shaped or of the L type. They were found to belong to the last type. It is seen from the curve exhibited that there is a rapid fall from copper to the alloy containing only 10 per cent. antimony, and this decrement is continued until the alloy $StCu_4$ is reached, when the curve turns rapidly and rises to $StCu_2$, then turns again, and passes to pure antimony. Prof. Roberts has shown that the alloy $SnCu_4$ occupies the lowest point of the curve, namely, the position that in the copper-antimony series is occupied by the alloy $StCu_4$. In the copper-tin series the second critical point is held by $SnCu_3$, and in the copper-antimony curve this point is held, not by Cu_3Sb, but Cu_2Sb, the formula for the violet alloy known to alchemists as the "regulus of Venus." The specific gravities were also plotted in curves, showing that the alloy Cu_2Sb does not stand out from the rest, while the alloy Cu_4Sb has a higher density than copper.

EDINBURGH

Royal Society, June 18.—Prof. Maclagan, vice-president, in the chair.—The Astronomer-Royal for Scotland presented a paper which was read by Prof. Crum Brown, on bright clouds in a dark night sky. This phenomenon Prof. Smyth had twice witnessed, on April 8, 1882, and April 30, 1883. On both these occasions the meteorological conditions were peculiar, the air being for a few hours remarkably dry. The explanation given was that the glow on the clouds was due to reflection of the gas-lights of Edinburgh from the hollow water-drops in the cloud, which from their floating in a very dry atmosphere had become sufficiently thin-walled to throw back a strong reflection from the two surfaces.—Prof. Tait read a mathematical note by Mr. Anglin, in which a solution was given of the problem to express x^m in terms of powers of x lower than n, when x^n is given in terms of these lower powers, and m is greater than n.—Prof. Tait communicated the results of his recent measurements of the compressibility of water. The water was compressed in a tube silvered inside and dipping with its lower and open end in a trough of mercury. The whole was placed inside the hydraulic press, and exposed to pressures of 1, 2, $2\frac{1}{2}$, and 3 tons weight per square inch, the compression of the water being measured by the height of ascent of the mercury, which was given at once by the lower limit of the silver film. For water, both fresh and salt, the compressibility was found to diminish with increase of pressure, diminishing at much the same rate in both cases, although to begin with the fresh water was more compressible than the sea-water in the ratio of about 72 : 67. The results obtained for the fresh water could be very accurately represented by the formula $c = \cdot 0072 (1 - \cdot 043 p)$, where c is the true compressibility per ton at pressure p tons weight per square inch. The mean temperature of the water was 12° C. At the same temperature alcohol of density ·83 showed a much greater compressibility (·01202 for one ton weight per square inch), which also diminished with increase of pressure—·01043 being the average compressibility for 3 tons weight.

SYDNEY

Linnean Society of New South Wales, April 25.—The following papers were read:—Notes on a collection of fishes from the Burdekin and Mary Rivers, Queensland, by William Macleay, F.L.S., &c. The new species described are *Serranus estuarius*, *Therapon fuliginosus* and *parviceps*, *Diagramma labiosum*, *Corvina argentea*, *Caranx compressus*, *Cybium semifasciatum*, *Platycephalus Mortoni*, *Eleotris planiceps*, *Atherinichthys maculatus*, *Mugil Ramsayi*, *Chatoëssus elongatus*, *Anguilla marginipinnis*, and *Taeniura Mortoni*.—By J. J. Fletcher, M.A., B.Sc., notes on a viviparous lizard. The author's attention had been drawn to the subject during last January, when he obtained at Burrawang several examples of female lizards in an advanced stage of pregnancy. The embryos were from two to three inches long, enveloped in a thin and transparent chorion quite devoid of the calcareous matter with which it is more or less impregnated in the oviparous species.—Notes on a method of obtaining water from *Eucalyptus* roots, as practised by the natives of the country between the Lachlan and Darling Rivers, by K. H. Bennett.—Prof. Stephens exhibited a photograph and a sketch forwarded by Mr. C. Jenkins, representing a fossil from the Devonian formation of the Murrumbidgee valley, near Yass. Mr. Jenkins is inclined to refer it to *Asterolepis* (which is closely connected with *Pterichthys*), but chiefly on account of the character of sculpture of the scales. On the same ground he doubts its relationship to *Cacosteus* or *Cephalaspis*. Prof. Stephens added that without the actual specimen before them with all its collected fragments, it would be premature to determine even the genus of this ancient fish, but pointed out that it appeared to have some points of resemblance to *Macropetalichthys* of the North American Devonians.—Mr. J. J. Fletcher exhibited a specimen of a giant earthworm, 25 inches long, from Burrawang, N.S.W. It probably belongs to Prof. M'Coy's genus *Megascolides*, and its existence in this colony is now recorded for the first time.

BERLIN

Physical Society, May 25.—Dr. Aron spoke on the glow-light coal, which, as is well known, is distinguished by its electrical conductibility and by its resistance to combustion when exposed to the atmosphere in an incandescent condition, and which thus resembles graphite, which possesses both these properties in a high degree. Experiments which were made in order to determine whether such good conducting and indestructible coal, as well as artificial graphite, could be made artificially, led to the result that organic substances, *e.g.* paper, cloth, wadding, when charred in vacuo at very high tempratures in graphite crucibles, acquire the property of resisting combustion and afterwards become good conductors. Wood-coal also, which, though it is with difficulty combustible, is a bad conductor, was converted into a good conductor by strong incandescence. When the incandescence and the subsequent cooling down were conducted in a stream of hydrogen, this had no effect upon the resistance to combustion. Soot, which was made incandescent under similar conditions, also acquired the properties of graphite in a high degree, so that for many purposes (*e.g.* in galvano-plastic work) soot that has been made strongly incandescent can be made to replace graphite. The property of leaving an impression which graphite possesses, and which makes it so well adapted to the manufacture of lead-pencils, was not acquired by the different kinds of carbon in the process of incandescence; very probably this property depends upon the crystalline composition of the graphite.

VIENNA

Imperial Academy of Sciences, March 8.—E. Mach, experiments and notes on the system of lightning-conductors of Mr. Melsens.—C. von Ettingshausen, contributions to the knowledge of the Tertiary flora of Java.—L. Pfaundler, on the mantle-ring machine of Kravogl, and its relation to the machine of Pacinotti-Gramme.—F. Hochstetter, sixth report of the Prehistoric Commission: on the mounds recently found at Watsch and St. Margarethen (Carniola).—F. Steindachner, on Japanese fishes.—G. Goldschmidt and R. Wegscheider, on the derivatives of pyrene.—R. Wegscheider, on some derivatives of opianic acid.—E. von Bruecke, on alcophyr, and on the true and the so-called biuret reaction.

April 5.—E. Mach, preliminary communication on new experiments made with the influence-machine.—F. Lukas, on the knowledge of the absolute strength of vegetable tissues.—W. Simerka, on the power of conviction (a mathematical study).—T. V. Tanovsky, on nitro and amido derivatives of azobenzene.—A. Nalepa, contributions to the anatomy of Stylommatophora.—A Lieben and L. Haitinger, preliminary communication on chelidonic acid.—E. Lippmann, on azylines.—B. Schwarz, on an eclipse of the sun mentioned by Archilochos. T. M. Pernter, psychrometrical studies.

PARIS

Academy of Sciences, June 25.—New methods of determining the right ascensions and absolute declinations of the stars (continued), by M. Lœwy.—Experimental studies in relation to the photometric observation of the eclipses of the satellites of Jupiter, by MM. A. Cornu and A. Obrecht.—A study of the deformations produced by sharp-edged tools in drilling, by M. Tresca.—On the employment of partial photographs in studying human and animal locomotion, by M. Marey. The object of this process is to avoid the great confusion caused by the superposition of numerous reflections in the case of slow locomotion. It is illustrated by a cut showing the attitude of the left leg of a man walking at a moderate pace and reflected at the rate of about sixty per second. The partial photographs obtained by this method enable the observer to analyse all kinds of motion, such as walking, running, leaping, and even action confined to one place.—On the action of mixtures of air with vapour of chloroform, and on a new process of anæsthesia, by Paul Bert. The experiments were made on dogs, which were treated with doses of chloroform diluted in varying proportions with air. From the effects observed it is hoped that many important problems may be solved connected with the action of this anæsthetic. But although all risk may thus be avoided in its application, the disadvantages inherent in chloroform itself cannot be overcome, and protoxide of azote still maintains its preeminence above all the anæsthetics.—On the reciprocal of homogeneity; similitude of mathematical formulas, by A. Ledieu.—Methods of separating gallium from ruthenium, osmium, arsenic, and selenium, by M. Lecoq de Boisbaudran.—On a case of long-standing hysteria, all the symptoms of which disappeared under the influence of aluminium, by M. Burcq.—On a method of computing secular perturbations in the elements of the orbits of planets, asteroids, comets, &c., by O. Callandreau.—A new generalisation of a formula of Lagrange, already generalised by Cauchy, by Em. Barbier.—On the relations of induction to electrodynamic action, and on a general law of induction, by M. Quet.—Automatic impression of telephotic despatches, that is, of despatches transmitted by light, memoir by M. Martin de Brettes.—On a method of determining by constant registration the slight movements of the crust of the earth. This method of recording microseismic movements was first suggested by MM. Bertelli and de Rossi, and forms the subject of a paper published in the *Engineer* for December 17, 1875.—On the sulphate of thorium, by Eug. Demarçay.—On a base derived from crotonic aldehyde, by Alph. Combes.—Researches on mesitylene, by MM. Robinet and Colson. A new glycol is described, and it is shown that the dichloride and the dibromide of mesitylene obtained by the action of chlorine and bromine on mesitylene gas are identical with the dichlorhydric and dibromhydric ethers of this gas.—Observations on the fermentation of breadstuffs, by M. Moussette.—On the concomitance of the anatomic and organographic characters of plants, by M. J. Vesque.—Borings at Rilhac, in the Brassac basin, east of Arvant, by M. Daubrée. These borings were most successful, revealing at a depth of eighty-six metres productive carboniferous strata underlying horizontal beds of clay and more or less argillaceous sandstones.—Borings at Toussieu, department of Isère, by M. Grand'Eury.—After piercing various alluvial, limestone, clay, and sandstone formations, coal was reached at a depth of 364 metres. These borings were begun after those of Chaponay had revealed carboniferous beds at the depth of 212 metres immediately below the marine molasse. The chief object of both is to determine the extension of the coal measures of the Loire basin under the tertiary plain in the north of Lower Dauphiny.—Scientific results of Col. Prejevalsky's journeys, and especially of his third expedition towards Tibet and the sources of the Yellow River, by M. Venukoff. Amongst the more important results were the animal and vegetable collections, comprising 408 specimens of 90 species of mammalia, 3425 of 400 species of birds, 976 of 50 species of reptiles, 423 of 53 species of fishes, 6000 of insects, and 12,000 of 1500 species of plants.

CONTENTS

THURSDAY, JULY 12, 1883

HYDRAULIC MANUAL

Hydraulic Manual. By L. D'A. Jackson. 4th Edition. Pp. xiv. + 307 Text + 184 Tables. (London : Crosby Lockwood and Co., 1883.)

THIS well-known text-book having reached its fourth edition, it is unnecessary to review it as a new work. The changes from the third edition are very great; the chief is the omission of the whole of the "Hydraulic and Meteorological Statistics" (about 224 pp. of tables); these relate chiefly to India, so that their omission is an advantage to the "Manual" as a general text-book, as it has enabled the text to be increased from 221 to 307 pp., and the general working tables from 104 to 184 pp., without increasing the bulk of the volume; the chief increase of the text is the introduction of an account of the great Roorkee hydraulic experiments.

Much stress is rightly laid on the small value of the old hydraulic knowledge; thus (p. 3) it is said, "Taken generally the mass of hydraulic science . . . prior to about 1856 may be considered superannuated. . . ." The most useful feature of this work is indeed its freedom from what is "superannuated," and its thorough adoption of recent experiment; the text is in fact in great part a short account of the great modern experiments. In detailing field operations the author has indeed preferred to give a "brief account of the modes adopted by various hydraulicians" as being "a far better guide to the engineer about to undertake the execution of gauging operations than any arbitrary advice or set of rules could possibly be." These concise accounts are on the whole well condensed; but the recapitulation—in some cases verbatim—of the several experimenters' own conclusions has the disadvantage that in several cases contradictory conclusions appear on different pages; this is inseparable from the progressive state of our knowledge of the motion of water when stated in this way; a little more discussion of the contradictory views would have been useful.

Kutter's general formula for mean velocity was early adopted by the author; its use as the formula to be preferred to all others for the case of canals (whenever velocity-observation has to be dispensed with) is now insisted on, much evidence in favour of it having been brought out by the recent large Roorkee experiments, with the very fair reservation however that Kutter's rugosity-coefficient (n) should at present be determined by actual experiment for each new channel, the data for its *a priori* determination (from the mere nature of the channel) not being as yet good enough. On the other hand it is rightly said that "to determine with accuracy the discharge of any ordinary or large river independent of velocity-observation is at present impossible."

A few minor details are worth notice. The units of measure, &c., adopted are an extremely simple and useful decimal system; they include the foot, the "foot-weight" of 1000 fluid ounces, *i.e.* the weight of a cubic foot of water at its greatest density, and a "league" of two London miles of 5000 feet each; this league is particularly suited to measurement of hydraulic slopes, a fall of 1, 2, &c., feet per league being at once seen to give a slope of 1, 2, &c., in 10,000. Two new very expressive names are introduced for two velocities, which recur very frequently in discussions on flow of water, viz. "verticalic velocity" and "transversalic velocity" for velocities past any vertical line or any (horizontal) transverse line in a channel section; these short terms will be a great relief from the wearisome periphrases hitherto in use, and merit general adoption.

A few suggestions towards improvement of the work may now be made. (1) In a purely professional work such expressions as "international recrimination," and "bureaucratic and heated with vanity" (p. 37) are surely out of place. (2) About one page of text and three of tables are devoted to the variation of gravity in different latitudes and at different heights; now the variation is so small that for the rough calculations of practical hydraulics this is an unnecessary refinement. (3) Among the "general notation" (p. 11) occurs the rather awkward phrase "g = velocity acquired by gravity in one second." (4) In finding the (trapezoidal) "section of maximum discharge" from the expression for discharge $Q = A\ V$ where $V = 100\,c\ \sqrt{R\,S}$ and $R = A \div P$, the argument used is that "under the condition of maximum discharge, A will be a maximum, so also will R; and when these are temporarily constant, P will be a minimum;" this argument might be considerably improved, somewhat as follows:—"Since $Q = 100\,c\,A\ \sqrt{R}\,.\,\sqrt{S}$, therefore Q is greatest (provided S be kept constant) when c, A, and R are all maxima together; now c is known (from experiment) to increase with R, and $R = A \div P$; hence Q will be greatest when A is a max. and P a min. at same time (provided of course that S is constant)": this argument is more general than that in the text; the effect of the S-variation is unknown. (5) About certain formulæ for "mean verticalic velocity," quoted from the Roorkee work, it is said (p. 209)—"The defect in these methods is evident; it consists in making the parabolic curvature dependent on one point or on two points, whereas three points are the least necessary." This last statement is a mistake; three points are necessary (for finding a mean ordinate) only if they be taken at random, but *two points are sufficient when suitably chosen*, as in the "two-velocity formulæ" quoted on pp. 87, 208 from the Roorkee work; these formulæ are in fact accurate for the parabolic form, and the proof of this (from the Roorkee work) is actually given at p. 87. The "one-velocity formulæ" are of course only approximate. It may be mentioned here that the writer has lately[1] discovered another (and far better) "two-velocity formula," also accurate for the parabola, viz. $U = \frac{1}{2}(v_{\cdot 211\,H} + v_{\cdot 789\,H})$, showing that the "mean verticalic velocity" is the arithmetic mean of the velocities at ·211 and ·789 (or say $\frac{2}{10}$ and $\frac{8}{10}$) of the depth: this new formula has several great practical advantages over any other yet published; among others, the two velocities can be measured at one operation with a single instrument (a compound "double-float" with two equal subfloats at the depths named), which is itself moreover susceptible of being made a more accurate instrument than any other of its class (double-float).

ALLAN CUNNINGHAM

[1] See *Proc. Inst. Civil Engineers*, vol. lxxi. pp. 18, 19, where the formula and instrument are both discussed.

ORIGINES CELTICÆ

Origines Celticæ (a Fragment), and other Contributions to the History of Britain. By Edwin Guest. Two Vols. (London: Macmillan and Co., 1883.)

A MAN'S foes are indeed those of his own household. More than one literary or scientific reputation has been injured by the injudicious zeal of a writer's friends to publish after his death the fragments and papers he has left behind. It is natural to imagine that the work and suggestions of a scholar must all be equally valuable, and that by omitting to print any portion of it the world may be a loser. But it must be remembered on the other side that a good deal which a scientific worker commits to manuscript is never intended to see the light, and that in any case it is unfair to him to publish fragmentary remains which he has never had the chance of revising and correcting.

Dr. Guest's name is deservedly one of power among all those who have interested themselves in the earlier history of our country. His papers on the Invasion of Britain by Julius Cæsar, on the Campaign of Aulus Plautius, on the Four Roman Ways, and on the Saxon Conquests in Britain, are all models of sound scholarship and careful method. Dr. Freeman acknowledges him as a master, and declares that "whenever they meet on the same ground, he ranks above Palgrave and Kemble." Friends and public alike, therefore, might have expected to find in the fragments of his unfinished work, "Origines Celticæ," a fresh monument to his historical sagacity and another contribution of importance to the ethnology of our islands.

But friends and public alike must be grievously disappointed by what is actually placed before them. It would have been far better to spare the paper and ink that has been expended upon it, and, what is of more consequence, the fair fame of the author himself. The "Origines Celticæ," which occupy the whole of the first volume and the opening pages of the second volume of Dr. Guest's posthumous works are a barren waste of unscientific theorising and uncritical collection of facts. The work carries us back to an age when the application of the scientific method to history was unknown, when ethnology and comparative philology were as yet undreamt of, and when the most amazing generalisations were built on the chance coincidence of proper names. In our search for the fathers of the Kelts we are transported to the Caucasus, to Egypt, and even to Ur of the Chaldees, and no shadow of doubt is allowed to cross the mind that Kimmerians and Kimbrians and Kymry are all one and the same people. The fact that there were Iberians in Georgia and Iberians in Spain is considered quite sufficient to prove that the early population of the Spanish Peninsula came from the sources of the Euphrates.

Dr. Guest's philology is as wild as his ethnology. He has heard of "Grimm's Laws"; but as their existence is inconvenient to his own etymological mode of procedure he denounces both the "laws" and their observers, though without understanding what they really mean. When Indo-European philology is treated in this way it is not surprising that the Rutennu of the Egyptian inscriptions are connected with the Assyrians of Resen, that initial *k* and *h* are said to interchange in Phœnician, or that an Egyptian settlement in Kolkhis is declared to admit of "no reasonable doubt."

Dr. Guest's turn of mind, in fact, was literary rather than scientific. Wherever the question was a purely literary one, he displayed erudition, patience, and common sense; where, on the contrary, it was ethnological or philological, he showed himself as helpless as a Jewish rabbi. The old well-threshed statements of Greek and Latin writers are heaped together, and tricked out here and there with references to the discoveries of Egyptian and Assyrian research. How little he knew of the latter, however, may be judged from the frequent mistakes he makes when appealing to them, as when, for instance, he insists on calling Sumer Sommari, or tells us that Assurbani-pal lived in the ninth century B.C.

Had the "Origines Celticæ" appeared a hundred years ago they would have been hailed as a profoundly learned and interesting book. There is no place for them in an age when the departments of knowledge with which they deal have been occupied by the method and spirit of inductive science. To know what Dr. Guest really was and of what he was really capable we must turn to the papers reprinted in the second volume of his remains, though even here we shall from time to time be reminded of the literary spirit which accepts what is not disproved rather than of the scientific spirit which doubts everything and holds fast only to that which is proved.

A. H. SAYCE

OUR BOOK SHELF

Handbook of Vertebrate Dissection. Part II. "How to Dissect a Bird." By H. Newell Martin, D.Sc., M.D., M.A., and William A. Moale, M.D. (New York: Macmillan and Co., 1883.)

SOME months ago we noticed in these columns (vol. xxvii. p. 335) the first of a series of Handbooks of Vertebrate Dissection, by Drs. Martin and Moale—"How to Dissect a Chelonian." The second, "How to Dissect a Bird," has now appeared, and, as the type selected is the pigeon, this volume will doubtless be appreciated by a large number of students.

The general arrangement of the book is much the same as that of its predecessor, directions being given how to proceed step by step, so that the student, with its aid, ought to be able to gain a good knowledge of the anatomical characters of a bird. The skeleton, in particular, is described in great detail, and there are four good figures and a diagram of the skull, as well as a figure of the hind limb. It is, however, to be regretted that there are no illustrations of the soft parts, for figures of the skeleton—at any rate of allied forms—can be got in almost any text-book on Comparative Anatomy, while satisfactory drawings of the viscera, &c., are not so easily obtainable.

The directions are on the whole excellent, with one or two slight exceptions. The description of the air-sacs, for instance, is very indefinite, and gives no idea of their true relations. If the authors had glanced through Prof. Huxley's recent paper on the subject in the *Proceedings of the Zoological Society*, and compared the air-sacs of the pigeon with the description there given, there is no doubt that the position of these structures and their relations to the lungs would have been stated more clearly.

We must also call attention to the following points, which are not very accurate:—

Only one pancreatic duct is described instead of three.

The inferior mesenteric artery, instead of the median sacral, is stated to be the termination of the aorta.

The descriptions of the thymus and thyroid glands appear to have been transposed.

The three divisions of the cloaca are not described, and the rudimentary right oviduct is not mentioned, though the "*Fallopian tubes*" are said to open into the cloaca.

It is a mistake to introduce questionable homologies into a book of this kind, especially when they are unsupported by fact. Thus the statement in § 118 that the "thin sheet of muscle which is closely adapted to the concave ventral surface of the lungs . . . represents the diaphragm of mammals," is certainly misleading. In the first place, the position and relations of these muscles are entirely different from those of the mammalian diaphragm, and, moreover, they receive their nerve-supply from the intercostals, the phrenic being absent. The fact that the phrenic arises so far forwards appears to indicate an entirely different origin for these two structures.

With these slight exceptions, however, the descriptions and directions leave little to be desired for clearness and accuracy. It is certain that accurate detailed directions are far more valuable for elementary teaching than more general ones; the student, once having mastered them, finds little difficulty in grasping the wider bearings of the subject. Such works as the present, therefore, which entail a careful examination of every point mentioned, not only save both student and demonstrator much trouble, but insure more accuracy in work. A series of pamphlets such as the authors intend to publish, treating of all the more important vertebrate types usually dissected in an ordinary course on comparative anatomy, will certainly prove most valuable.

An Easy Introduction to Chemistry. Edited by the Rev. Arthur Rigg, M.A., and Walter T. Goolden, M.A. New Edition Revised, pp. 148. (London: Rivingtons, 1883.)

THIS book is based on a "First Book of Chemistry," by Dr. Worthington Hooker, and is intended, we are told in the preface, "to convey information in respect to changes which are likely to attract the attention of young persons who observe and inquire."

It is questionable whether "young persons" do well in attempting to study chemistry; the chemical laboratory is not a place in every way suited to the requirements of youth, but without steady work in a laboratory no real progress in chemistry can be looked for. Should, however, any youth desire information regarding material changes which he observes around him, he will find a considerable amount of information in this little book; but should he be desirous to study chemistry, he will not we are afraid derive much assistance from this "Easy Introduction." Many experiments are described and numerous well-executed illustrations are given, but several of these experiments could not be performed by a beginner without the aid of a teacher or of much more detailed description than is given in the text. To read statements of the results of experiments is not the way by which young persons can acquire interest in or a knowledge of chemistry.

Although excellent in many ways, yet we cannot think this book will prove an efficient introduction to chemistry; a perusal of it may, however, serve to stimulate young persons to seek for an introduction to the science, some of the materials for the construction of which are put before them in this work.

Practical Electric Lighting. By A. Bromley Holmes, Assoc.Inst.C.E. Sixty-two Illustrations. (London: E. and F. N. Spon, 1883.)

IT is with pleasure that we shall watch the success of Mr. Holmes's little book on "Practical Electric Lighting." Mr. Holmes has clearly and simply put before the unelectrical public as much and no more of theoretical electricity as is necessary for his purpose, together with a good general summary of the chief machines and appliances in present use.

Besides this, much useful information is given in the last two chapters, first, on the present economic state of electric lighting, and second, on the best means of applying the power to electric machinery. If there be one point in the book not so strong as the rest it is that the descriptions of the dynamos and lamps would have been better if they had entered a little more into detail.

C. C. S.

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications.*

[*The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to insure the appearance even of communications containing interesting and novel facts.*]

Geology of the Congo

I HAVE received from one of the Baptist missionaries in the front of the Congo mission a basket of specimens of rocks and a letter giving some particulars of the geological structure of the localities. The letter may be interesting as the first news of this kind from the centre of Africa, on its western side, and I therefore place it at your service.

S. R. PATTISON

5, Lyndhurst Road, Hampstead, N.W., July 7

Liverpool Station, B.M.S., Stanley Pool, Congo River, S.W. Central Africa, March 15, 1883

S. R. PATTISON, Esq.

DEAR SIR,—Before leaving England in 1879 you made us a kind offer to render us any help in geological and mineralogical matters that lay in your power, and that kindness has been recalled to mind every time I have examined a piece of rock. I had seen such geological variety in the few parts of England that I had visited, that in my ignorance I was expecting to find a much greater variety out here and at least some fossil treasures. But in this, as you may guess, I have been disappointed.

In sending home some curios the other day, I inclosed a few native ingots of lead and copper, which I thought might interest you. They are "mined" in a district of the Bizunseke tribe called Noama, some twenty-five miles west-north-west of the Ntombo Mataka Falls of the river. I have not visited the place, but I believe de Brazza (concerning whom there have been some paragraphs in the newspapers lately) passed through this "mining district."

Although I believe that no metal from that part is ever sold to white traders, it is an important item in native trade with the far interior. Some are used to make bullets, others are recast to make anklets, &c. If the district is rich, or there is much silver in the lead, the French will perhaps work there.

The case of curios was sent away before I had intended. I had hoped to have added other things, and a piece of sandstone from the cataracts here.

Mr. Stanley indirectly hinted that Stanley Pool might be the crater of a volcano (extinct of course) which in old time had rent a rift in the hills, forming an exit to the pent-up waters of a vast interior sea. I am not sure how much of this was made public by him. It was, however, his opinion on our return from our first visit to the Pool. He speaks also of lava reefs, granite, gneiss, trap, &c.

Dr. Pechuël Lösche, late of the German Expedition, which spent some time (about five or six years ago) on the coast about the Kioilo and Chiloango Rivers, was for a short time in charge of the Belgian expedition here during Mr Stanley's absence. He assures me that there is no trace of igneous action above Isangila at least. There may be traces of such action at the Yelala Falls, which form the first bar to navigation from the sea. Between there and Isangila Falls quartz, slate, micaceous, and granitic (apparently) rocks are the rule.

Above Isangila limestone is abundant for about ten miles, above that slaty rocks are prevalent. Limestone crops out again about the country of the Basundi. This gives place to a red shale at the western boundary of the Babwende, and at the Ntombo Mataka Falls a red sandstone appears under the shale.

This sandstone is soft and is broken up and rounded into huge boulders, which are covered with a smooth, chocolate-coloured, ferruginous glaze, deposited by the river, and hardened by the sun. These boulders thus glazed might well have been regarded by Mr. Stanley as trap and lava, &c., while the large grain of the stone, together with the appearance of the blocks in some places might suggest its being granite. This sandstone with the exception of a little which is quartzitic, is the only rock I have seen between Manyanga and Stanley Pool, and is certainly the rock at the great Ntamo cataracts here.

On the hills and cliffs about the Pool there are some white shining patches, which I hear are sand, but I believe there is no calcareous rock in the neighbourhood. The pool itself is a strange break in the lines of sandstone hills, which, although now much eroded by water, are the remains, doubtless, of what was once a plateau, at the level of about 1500–1800 feet above the sea.

On the road to San Salvador from our old Musuka station we find boulders of ironstone and small nodules of the same, mixed with clay, on the top of quartz, micaceous, and granitic rocks. Limestone crops up in several places, but the principal formation visible is the ironstone clay. In all this country I have not met with a trace of a fossil of any kind.

When at Landana, about two miles south of the mouth of the Chiloango River, some months ago, I saw some stones from the cliffs which appeared to be almost identical with a Portland stone (?) which I have seen used in fortifications in the south of England. There were many fossils, but I could neither spare time to examine the cliffs nor carry many specimens, being on an express journey by hammock up the coast. This was the only occasion that I have met with any fossils in Africa, and that in a part of the coast now well known through the work of the German Expedition. The quartz, micaceous, slate, shale, and sandstone rocks of this part of the continent are a poor field for palæontologic research.

I am very curious as to the geological formation of the Congo Valley between this point and the Stanley Falls, but at present have learned nothing. I should expect, however, to find the sandstone the only visible rock.

I wish that I could speak with better acquaintance with the names of the rocks, but often I feel sorely puzzled. On our first journey to Stanley Pool we mistook some strangely shaped hills near to Manyanga for granite, but have since ascertained them to be singular relics of the sandstone.

I need not enter into details of our work, which are so fully and constantly reported in the *Missionary Herald*. Regretting that the information I can supply is so meagre,

Believe me, dear Sir, yours very truly,

W. HOLMAN BENTLEY

Intelligence in Animals

1. I OBSERVE that Dr. Romanes, in his very interesting work on "Animal Intelligence," has been good enough to notice an account given by me in NATURE, vol. xi. p. 29, of an instance of a scorpion committing suicide under special excitement. It may be well to remention the fact that in this case the rays of the sun, focused on the back of the scorpion by means of a common lens, were the exciting cause of the self-inflicted fatal sting; and to set the matter at rest it may be remarked that two witnesses who saw the experiment can corroborate my statements. On reconsidering the whole affair, however, it occurred to me that in wounding its own back the scorpion may have merely been trying to get rid of an imaginary enemy. The concentrated rays of the sun no doubt caused pain, and the sting was probably directed towards the seat of this in an automatic manner, as a defensive act. This seems to me a more feasible explanation than to regard the action as due to an instinct detrimental to the individual and to the species.

2. While writing on the subject of "animal intelligence," it has occurred to me that the following remarkable example is worthy of being put on record:—Some years ago, while living in Western Mysore I occupied a house surrounded by several acres of fine pasture land. The superior grass in this preserve was a great temptation to the village cattle, and whenever the gates were open, trespass was common. My servants did their best to drive off the intruders, but one day they came to me rather troubled, stating that a *Brahminy bull* which they had beaten had fallen down dead. It may be remarked that these bulls are sacred and privileged animals, being allowed to roam at large and eat whatever they may fancy in the open shops of the bazaar-men.

On hearing that the trespasser was dead, I immediately went to view the body, and there sure enough it was lying exactly as if life were extinct. Being rather vexed about the occurrence, in case of getting into trouble with the natives, I did not stay to make any minute examination, but at once returned to the house with the view of reporting the affair to the district authorities. I had only been gone a short time, when a man, with joy in his face, came running to tell me that the bull was on his legs again and quietly grazing! Suffice it to say that the brute had acquired the trick of feigning death, which practically rendered its expulsion impossible, when it found itself in a desirable situation which it did not wish to quit. The ruse was practised frequently, with the object of enjoying my excellent grass, and although for a time amusing, it at length became tiresome, and resolving to get rid of him the sooner, I one day, when he had fallen down, sent to the kitchen for a supply of hot cinders, which we placed on his rump. At first he did not seem to mind this much, but as the application waxed hot, he gradually raised his head, took a steady look at the site of the cinders, and finally getting on his legs, went off at a racing pace, and cleared the fence like a deer. This was the last occasion on which we were favoured with a visit from our friend.

G. BIDIE

Ootacamund, June 5

The Mealy Odorous Spot in Lepidoptera

THE mealy spot on the base of the front margin of diurnal Lepidoptera, which emits an odour supposed to serve for sexual purposes, is present only in the male. It is therefore most interesting to observe that this spot is not always present in different individuals of the same species. Among the numerous varieties of *Papilio priamus* proved by rearing to belong to that species, the spot in question is present only in *P. priamus*, and is wanting in the male of all the varieties which have come under my observation. *Callidryas eubule* has the spot present only in specimens from Florida; it is wanting in all specimens from other localities of the United States, including a large number from Texas. In *Colias electra* and *edusa*, Keferstein (*Wien. Zool. Bot. Gesell.* 1882, p. 451) states that after an examination of a series of males he has found the mealy spot only exceptionally present, and the same is supposed by him to be the case in other species of *Colias*.

It would be interesting to know how this exceptional presence of so prominent a characteristic is to be explained.

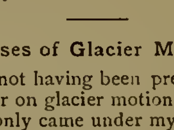
Cambridge, Mass., June 21

H. A. HAGEN

Causes of Glacier Motion

UNFORTUNATELY not having been present when Mr. W. R. Browne read his paper on glacier motion at the Royal Society on June 15, 1882, it only came under my notice when published in NATURE, vol. xxviii. p. 235. It is doubtless of little importance, but there is one sentence which does not seem to read exactly as I wrote it, namely, "It (a glacier) will get a series of cracks *in its longer axis*," should be "*across* (or transverse to) its longer axis," which I think makes the meaning more clear.

I may perhaps mention that when ice on lakes becomes from four to seven feet thick the effect of a sudden decrease of temperature does not, for obvious reasons, always cause a complete solution of continuity of the ice all the way through from its upper to its under surface, the crack being wedge-shaped, thus—



so that the water sometimes does not flow into the crack; the equable and higher temperature of the water counteracting at a certain point of the ice's thickness the penetration and consequent contracting influence of the colder air.

When the ice has acquired the great thickness above mentioned, the cracks by contraction are never so wide as when the ice is from one to three feet thick, but as far as I can remember they were more numerous, and when the water did not flow into them, were drifted full of snow by the first breeze of wind.

4, Addison Gardens, July 7

JOHN RAE

Sand

NOT having had the pleasure of perusing Mr. Waller's paper on sand, I gather from Mr. Gardner's notice of it that it is an attempt to distinguish by the aid of the microscope whether sand has been formed by the action of wind or of surf. Having a number of years ago become possessed with the idea that the form of the materials which make up the soils and subsoils found in any country might lead to a knowledge of the sources from which they had been derived, I had many soils and subsoils from Europe and Australasia looked at, but without being able to detect sufficient difference of shape or form as to lead to any definite result. Having been long familiar with the soils formed out of the boulder clay and drift of the south-east of Scotland, I had hoped to have seen a very marked difference in the form of the particles of sand existing in them from those of the interior of Victoria, New South Wales, and Queensland, large portions of the surface coverings of which countries are believed to have been deposited when covered with the sea. This difference exists certainly—that the soils of the boulder clays and drifts contain a far greater portion of fine and rough gravel, and rounder in shape than do those from Australia. Yet, so far as I could observe, the form of the sand was similar. It seems to me that both Messrs. Waller and Gardner are on the wrong track when searching solely for the typical forms of sand in the seashore or from torrents. The amount of sand found on the seashores of the world is large, no doubt, so is that from the rivers. What is that to the quantities contained in the surface coverings of the land? It is from this source the rivers obtain the supply they carry to sea or the shores, and make up the waste by friction. It has long seemed to me probable the sands, fine gravels, and silt formed by the passing of ice over the surface of the rocks would have a distinct form from the surface covering produced by other forces. The gravel or shingle of the rivers has a flatter shape than that of the seabeach when derived from the same rock. If such difference can be discovered in the silts, sands, and gravels derived from glacial action, it may be possible to assign limits to the extent to which ice has effected the present covering of the surface from the broken-up strata over which it has passed. Silt, sand, and shingle must all, however, be taken into account, and that from the deposits themselves, not from what has been subjected to littoral, fluviatile, or wind action.

Bonnington JAMES MELVIN

Garfish—Wild Fowl

WITH reference to Mr. Archer's note in NATURE last week (p. 226), may I remark that the beak of the garfish of southern waters (*Hemiramphus*, A.) is of rather too fragile a nature to be capable of making a slit of four inches in length in a hard felt hat? May not the fish in question have been more likely a young and small *Xiphias*—or, as is equally probable, a juvenile *Pristis* or sawfish—emulating with the thoughtless exuberance of youth the habits of *Exocætus?*

Any Australian can confirm the correctness of Dr. Rae's observations in the same page of NATURE *re* wild ducks and railways. Looking down upon the reedy waterholes on the south bank of the Yarra, from Princes Bridge in Melbourne, abundance of native waterfowl can any day be seen swimming about in conscious security and much less on the alert than they are in any swamp in the loneliest part of the bush. The constant roar of a great passing traffic, as well as the unceasing turmoil and unearthly noises of a large railway station within stone's throw of their haunts, is now quite unnoticed by these usually most watchful and wary of all birds.

But for the fear of trespassing on your space, I could give many more illustrations of the truth of Dr. Rae's remarks and of the quick and unerring instinct which so soon teaches both furred and feathered animals to dread less the roaring and shrieking ogre that is so swiftly tearing his way into their most secluded haunts in the uttermost parts of the earth than the silent, solitary biped who with gun in hand creeps stealthily upon them. ROBERT S. GOODSIR

Edinburgh, July 9

Glowworms

WHILE watching, last evening, some glowworms in a mossy stone wall, my attention was attracted to a firefly flying to and fro in the field beyond and approaching the wall where I stood. Arriving within two or three feet of the glowworm I was watching, he made several sharp zigzag flights, drawing nearer the light of the glowworm, and then, making a dash like that of a hawk at an object it has been watching, pitched directly on the glowworm, covering it in the fraction of a second. I had been noting the curious habit of this, which thus appeared to be the female insect, of standing with its abdomen erected in the air and quite motionless, except for a sort of pulsation, but on the contact of the male, the body fell to a normal position, and it was evident that coïtus was taking place. I watched them ten minutes until I was completely satisfied that this was the case, when I swept them both into a card box which I send with this for examination by a competent entomologist of the insects, which have not the slightest likeness to each other, the female resembling in general form the glowworm of England, but having an intenser light, and the light-emitting organs, beside the abdominal, which is the most luminous as well as the largest, being two glands (apparently) situated where the joints of wings might be expected if the insect were winged. The light is of an exquisite green, and so brilliant as to pale little at the proximity of a wax taper burning at six inches' distance.

This morning, on opening the box, I found the female apparently dead and collapsed; but the male, on the light returning to them, attempted to renew his embraces.

I remember a discussion at Cambridge (U.S.A.) some years ago, in which Agassiz conjectured that the light of the glowworm served as an amorous guide, but I had only a few weeks before noticed quite a different use for it. In one of the primitive forests of New York State, where twilight is normal from the density of the shade, I was attracted by the loud buzzing of a fly under a recumbent tree trunk. On looking for the cause of it I found a large, luminiferous insect resembling in general construction the common glowworm, but with powerful mandibles, which had built itself a little pit resembling that of an ant-lion, at the bottom of which it was lying, its light distinctly visible. The fly was in the clutch of the mandibles, helpless, though as large as a bluebottle, nor could I easily extricate him. There could be no more mistaking in his case that the light was a decoy than in this of the Pistoiese insect being a sexual invitation.

W. J. STILLMAN

Cutigliano, Pistoiese Apennines, June 25

[The name of the glowworm is *Lamprorhiza splendidula*, a common South European species.—ED.]

Mimicry

I HEARD what I fancy was rather a curious instance of mimicry last Wednesday evening (June 28) about 10 o'clock. I was walking with a friend across a field adjoining a meadow, in which was a landrail (*Rallus Crex*); we both noticed that the animal's cry, or crake as it is called here, was pitched in a higher and somewhat softer key than is usually the case, and my friend remarked that perhaps it was a young bird, but we were considerably surprised to hear him imitate the cry of the lapwing (*Tringa vanellus*). At first this cry was uttered only once alternately with several crakes, but we listened for about ten minutes, at which time, I suppose, he fancied that his note was perfected (which, however, it was not, being much less sharp than the pee-wit of the lapwing), and so he essayed it several times in succession. But he ultimately relapsed into his craking again.

Filston Hall, Shoreham, Kent, July 4 A. HALE

Indian Numeration

IN your review (p. 195) of "Field and Garden Crops of the North-Western Provinces and Oudh" you speak of the peculiar system of numeration used by the author, as in the instance 6,79,06,496, expressing sixty-seven millions, &c. Perhaps I may be allowed to point out that this marking is quite in accordance with the native Indian method of numeration, in which there is no word equivalent to "million." In India the series runs thus:—Thousands, tens of thousands, lakhs, tens of lakhs, krors (or crores). A lakh is a hundred thousand, a kror is ten millions.

It may be doubted whether it is advisable to adopt this system in an English book, for even native readers of it would easily enough follow our own; still it is not uncommon to see lakhs and krors made use of in English official papers.

Of three questions asked by the reviewer, the above remarks give an answer to one; as to the others I may say that a "seer" is two pounds avoirdupois, and a "maund" is forty seers.

Eton College, July 4 FREDERIC DREW

FUNERAL OF MR. SPOTTISWOODE

THE funeral of the late President of the Royal Society on Thursday last was impressive and solemn, and was a fitting end to the life that had passed away.

We take from the *Times* the following account of the general arrangements of the funeral :—

A large number of those who were present assembled in the Jerusalem Chamber, which the Dean had kindly placed at the disposal of the family. Those who by courtesy were styled pall-bearers met here—Dr. Evans, Vice-President and Treasurer of the Royal Society, the Marquis of Salisbury (Chancellor of the University of Oxford), Earl Granville (Chancellor of the University of London), Mr. Childers, Sir W. Siemens, the Duke of Northumberland, Sir Frederick Leighton, P.R.A., the Master of the Stationers' Company, Lord Aberdare, Sir John Lubbock, Mr. E. J. Stone, Sir Bartle Frere, Prof. Flower, Sir W. Armstrong, and, representing departments in the firms with which the name of Spottiswoode is connected, Mr. Shinn, Mr. Millwood, Mr. Carey, Mr. White, Mr. Howe, Mr. Wilson, Mr. Hamilton, and Mr. Straker. Others, who went first to the Jerusalem Chamber, were the Archbishop of York, the Bishop of Lincoln, Mr. Mundella, M.P., Mr. Shaw-Lefevre, M.P., the Dean of Christ Church, the Master of Balliol (Prof. Jowett), the Archdeacon of Maidstone, Sir Frederick Bramwell, Sir Richard Cross, M.P., Mr. Warren De La Rue, Sir Frederick Evans, Sir Joseph Fayrer, Sir James Caird, Lord Claud Hamilton, the Hon. George Brodrick (Warden of Merton), Mr. W. H. Smith, M.P., Mr. W. E. Forster, M.P., Sir Charles Dilke, General Sir H. Rawlinson, Sir James Paget, Mr. Irving, Prof. Huxley, Sir Joseph Hooker, Mr. Lecky, Sir Richard Temple, and Mr. J. Norman Lockyer. Some again, among whom were Lord O'Hagan, Sir Walter Stirling, Sir Henry Barkly, Sir James Cockle, the Dean of Wells, Mr. Philip Magnus, Director of the City and Guilds of London Institute, Mr. Trueman Wood, Secretary, and Mr. Wheatley, Assistant Secretary of the Society of Arts, Mr. Symons, F.R.S., of the Meteorological Society, and Sir John Kennaway, M.P., at once took their places in the choir or south transept, the seats in the north transept being reserved for *employés* of Messrs. Eyre and Spottiswoode and Messrs. Spottiswoode and Co.

Besides those who have already been named, the list of mourners invited to attend in the Jerusalem Chamber included the following gentlemen, of whom nearly all were present :—

Mr. Andrew Cockerell (representing his Royal Highness the Prince of Wales), the Lord Mayor, Mr. Gladstone, the American Minister, Count D'Aunay, Count Munster, the Lord Chancellor, the Earl of Northbrook, the Duke of Argyll, the Duke of Buccleuch, the Earl of Derby, the Earl of Ducie, the Earl of Dufferin, Earl Sydney, Sir Stafford Northcote, M.P., Lord Sherbrooke, Earl Spencer, Sir Frederick Abel, C.B., Capt. Abney, R.E., Prof. Acland, M.D., Prof. J. Adams, LL.D., Prof. W. Adams, M.A., Sir George Airy, K.C.B., Prof. Allman, M.D., Prof. C. Babington, M.A., Mr. John Ball, M.A., Mr. P. W. Barlow, F.G.S., the Earl of Rosse, Lord Chelmsford, Lord Eustace Cecil, Lord Lawrence, Lord Reay, the Marquis of Hartington, M.P., Lord Rayleigh, Lord Colin Campbell, Lord Carlingford, the Earl of Kimberley, Earl Amherst, Lord Houghton, the Bishop of London, the Dean of St. Paul's, the Bishop of Truro, the Dean of Salisbury, Mr. W. J. Farrar, Mr. W. H. Barlow, Mr. J. F. Bateman, F.G.S., Prof. Beale, M.D., Mr. I. L. Bell, F.C.S., Sir J. R. Bennett, M.D., Mr. George Bentham, F.L.S., Mr. Beresford-Hope, M.P., Sir Henry Bessemer, Mr. H. W. Blake, M.A., General Boileau, F.R.A.S., the Rev. T. G. Bonney, M.A., Mr. W. Bowman, LL.D., Mr. T. L. Brunton, M.D., Mr. G. B. Buckton, F.G.S., Sir C. J. Bunbury, Lord Cardwell, F.G.S., Dr. W. B. Carpenter, C.B., Mr. W. Carruthers, V.P.L.S., Prof. Cayley, V.P.R.A.S., Mr. Chamberlain, General Clark, R.A., Prof. R. B. Clifton, M.A., the Earl of Crawford and Balcarres, Prof. W. Crookes, F.C.S., Mr. T. B. Curling, F.R.C.S., Prof. G. H. Darwin, M.A., Prof. W. B. Dawkins, M.A., Prof. H. Debas, Ph.D., Prof. J. Dewar, M.A., Prof. Duncan, M.B., Mr. Edwin Dukin, F.R.A.S., Mr. W. T. Dyer, M.A., Sir W. Elliott, K.C.S.I., Mr. A. J. Ellis, B.A., Mr. Arthur Farre, M.D., Mr. Fawcett, M.P., Mr. James Fergusson, D.C.L., Prof. G. C. Foster, B.A., Dr. M. Foster, Prof. E. Frankland, D.C.L., Capt. Douglas Galton, C.B., Mr. Francis Galton, M.A., Dr. J. H. Gladstone, Mr. J. Glaisher, F.R.A.S., Mr. R. Godwin Austen, F.G.S., the Right Hon. J. G. Goschen, M.P., Lieut.-Col. J. Grant, C.B., the Right Hon. Sir W. H. Gregory, K.C.M.G., Sir W. Grove, Sir W. Gull, Mr. Albert Gunther, M.A., Prof. F. Guthrie, F.G.S., Mr. W. A. Guy, M.B., Sir W. V. Harcourt, M.P., Mr. A. G. Harcourt, V.P.C.S., Sir John Hawkshaw, Mr. Thomas Hawksley, M.I.C.E., Mr. R. B. Haywood, M.A., Mr. P. G. Hewett, F.R.C.S., Mr. James Heywood, F.G.S., Dr. T. A. Hirst, Prof. A. W. Hoffman, Ph.D., Mr. J. Hopkinson, M.A., Dr. W. Huggins, Mr. J. W. Hulke, F.R.C.S., Prof. Humphrey, M.D., Dr. J. H. Jackson, Dr. J. G. Jeffreys, Sir W. Jenner, K.C.B., Dr. J. C. Joule, Sir John and Lady Kennaway, Admiral Sir Astley Cooper Key, K.C.B., Prof. Ray Lankester, M.A., General Sir J. H. Lefroy, C.B., Mr. Joseph Lister, F.R.C.S., Admiral Sir F. L. M'Clintock, Sir H. Sumner Maine, LL.D., Prof. Marshall, V.P.R.C.S., Prof. N. S. Maskelyne, M.A., Mr. C. W. Merrifield, Mr. Alfred Newton, M.A., Prof. Odling, Mr. Daniel Oliver, F.L.S., Prof. Owen, C.B., Dr. John Percy, Mr. W. H. Perkin, C.S., Major-Gen. Pitt-Rivers, Sir Lyon Playfair, M.P., Dr. W. Pole, Mr. W. H. Preece, C.E., Prof. J. Prestwich, M.A., the Rev. Bartholomew Price, the Rev. Charles Pritchard, M.A., Dr. Quain, Sir A. C. Ramsay, LL.D., Prof. Osborne Reynolds, Admiral Sir G. H. Richards, C.B., Mr. G. J. Romanes, M.A., Prof. H. Roscoe, B.A., Mr. Osbert Salvin, M.A., Prof. Sanderson, Mr. P. L. Sclater, M.A., Mr. R. H. Scott, M.A., Mr. John Simon, C.B., Mr. W. W. Smyth, M.A., General W. J. Smythe, Mr. H. C. Sorby, LL.D., Mr. H. T. Stainton, F.L.S., Prof. Balfour Stewart, M.A., Prof. G. G. Stokes, M.A., General R. Strachey, R.E., Prof. J. J. Sylvester, M.A., Dr. Allen Thomson, Sir W. Thomson, LL.D., Mr. J. Todhunter, M.A., Prof. Tyndall, Dr. A. W. Williamson, Mr. W. H. Pollock, Mr. E. Bunbury, the Rev. B. Compton, Mr. Horace Davey, Q.C., M.P., the Head Master of Rugby, the Provost of Eton, the Head Master of Eton, the Hon. Ralph Dutton, the Hon. Robert Butler, Mr. Osborne Morgan, M.P., the Bishop of Exeter, Sir Louis Pelly, Sir Henry Thompson, Sir James Lacaita, Mr. J. Boehm, Mr. A. Milman, Mr. W. Hasseltine, Mr. F. Pollock, Mr. Pascoe Grenfell, Mr. T. Woolner, Mr. Lawrence T. Cave, Mr. T. H. Gordon, the Rev. W. H. Milman, Mr. T. Chenery, Mr. W. F. Burton, Mr. Douglas Freshfield, General Hutt, Dr. W. Grey, Dr. Priestly, Prof. Bryce, M.P., Mr. W. C. Cartwright, Sir Julian Goldsmid, Sir Louis Malet, Sir Rutherford Alcock, Sir Arthur Hobhouse, Sir Charles Mills, M.P., General Sir M. M'Murdo, General Sir Patrick Grant, Sir Charles Trevelyan, Sir James Stephen, Sir Charles Bowen, Lord G. Hamilton, M.P., the Recorder of London, Alderman Sir S. H. Waterlow, M.P., the Wardens of the Stationers' Company, Col. Donnelly, Prof. Ruskin, Cardinal Manning, a deputation from the Chemical Society, Mr. J. G. Dodson, M.P., Dr. Cumberbatch, Dr. Gibbons, Mr. Robert Browning, Mr. E. Chinnery, Mr. J. A. Froude, Sir R. Lingen, Mr. Herbert Spencer, the Dean of Llandaff, Sir Harry Verney, M.P., Lord Wolseley, Mr. Cyril Graham, Mr. Charles Eastlake, Mr. George Frere, F.R.S., Mr. Talfourd Ely, Sir Thomas Pears, Prof. Leone Levi, Prof. Max Müller, Mr. Frederick Verney, Mr. H. C. Hughes, and Major Gordon.

The body was borne from the house in Grosvenor Place to the Abbey on an open funeral car drawn by four horses. In the carriages immediately following the funeral car were Mrs. Spottiswoode, Mr. Hugh Spottiswoode, Mr. G. A. Spottiswoode, Mr. Cyril Spottiswoode, Mr. Rate, Mrs. George Spottiswoode, Miss Spottiswoode, Miss Augusta Spottiswoode, Mr. Arthur Brandreth, Mrs. Brandreth (sister of Mr. Spottiswoode), Mr. and Mrs. G. Noble Taylor, Mr. T. P. Beckwith, Miss Ellen Arbuthnot, Miss Mabel Spottiswoode, Mr. Adrian Spottiswoode, Mr. Norton Longman, Miss Longman, Miss Elizabeth Spottiswoode, Mr. Eyre, Mr. and Mrs. R. M. Bray, Mr. Frederick Arbuthnot, Miss Margaret and Master John Arbuthnot, Mrs. Beckwith, Mr. Sydney Beckwith, Mrs.

Jervoise, and Mr. Rate, jun. The servants came next in two carriages, and after the empty family carriage, the carriage of his Royal Highness the Prince of Wales, and then those of friends of the family.

At the entrance to the cloisters a company of the 2nd London Rifle Volunteer Regiment, formed of *employés* of the Spottiswoode establishments, stood with arms reversed as the procession passed through to the Abbey. At the West Cloister door, choristers, scholars, and clergy, the Rev. Flood Jones, precentor, the Rev. John Troutbeck, Minor Canon, Canon Rowsell, Canon Barry, Archdeacon Farrar, and the Dean, met the body. Immediately behind the chief mourners, and in front of those from the Jerusalem Chamber came, by special invitation of the family, Earl Stanhope, the Earl of Dalhousie, Sir F. W. Pollock, and Mr. J. F. Moulton; then Mr. Andrew Cockerell, representing the Prince of Wales, and among those from the Jerusalem Chamber, Mr. George Busk, Vice-President of the Royal Institution. The coffin, still covered with its lovely floral tributes, was placed under the lantern, while the 90th Psalm was sung to Purcell's Burial Chant, and the lesson was read by Canon Duckworth. As the notes of the anthem died away the body was borne to its last resting-place, near the grave of Archbishop Spottiswood. So great was the congregation of mourners, that not half the number could find standing room in the narrow aisle in which the grave is made. The Dean said the "Committal" and the prayers, and after the singing of Bishop Wordsworth's well-known hymn, "Hark, the sound of holy voices, chanting at the crystal sea," the Dean pronounced the blessing, and the mourners, casting into the grave the wreaths and bunches of flowers which many of them had carried, slowly dispersed. Dr. Bridge played the "Dead March" in "Saul" at the conclusion of the funeral service. The inscription on the plate of the coffin, which for sole decoration bore a Latin cross of brass, was—

WILLIAM SPOTTISWOODE,
Born January 11, 1825,
Died June 27, 1883.

A sermon *in memoriam* was preached in the Abbey by the Dean on Sunday afternoon.

THE ECLIPSE PARTY

LETTERS have been received from the English and American members of the above, giving some details which we think may prove of interest to our readers.

Leaving England on February 17 in the s.s. *Medway* the English observers made a calm passage to Colon. Here they met the American party, consisting of Prof. Holden, Dr. Hastings, Mr. Rockwell, Mr. Preston, Lieut Brown, and Mr. Upton, to which it will be remembered they were to be attached. The united party then proceeded to Panama, and took ship in the *Bolivia* for Callao, where they arrived on March 20. Early the following morning the instruments and baggage were removed to the U.S.S. *Hartford*, in which the voyage from thence was to be made, and the party left Callao about five o'clock on the evening of March 22, sighting Caroline Island, the spot selected for the observations, on April 20. Although named Caroline Island it is not a single island, but a low-lying chain of coral islets which enclose a central lagoon. The ring of islets is about seven and a half miles in length, and one and a half in breadth The island like most of its kind is of value on account of its stores of guano, and its cocoanut produce, being leased to Messrs. Houlder Brothers of 146, Leadenhall Street, whose agent at intervals visits this, and other Pacific coral islands leased to the firm. On the arrival of the *Hartford* a boat under the charge of Lieut. Qualtrough put off to make a tour of inspection, returning with the intelligence that there were two large empty frame houses, several smaller ones, and seven inhabitants—four men, one woman, and two children—who had come thither from Tahiti two months previously. A site having been selected by Prof. Holden for the erection of the observatory, the work of disembarkation commenced. This was a matter of great difficulty, the nature of the coast preventing even the small ship's boats approaching within several hundred yards of the shore. The boats had first to run in through a narrow opening in the reef, the boxes had then to be carried through fifty yards or so of water, varying in depth from two to three feet, next over about fifty yards of sharp irregular coral rock that cut the men's shoes to pieces, and finally to be carried up a soft sandy beach for upwards of a quarter of a mile. However, the landing was effected without accident, and the observers took possession of their various quarters.

The English observers report that the house in which they were located was a very comfortable one, containing a kitchen, dining-room, bed-room, bath-room, and store-room, and a large laboratory. Mr. Rockwell, one of the American observers, was fortunate enough to obtain the luxury of a bed. Mr. Upton, another of the party, had to be content with a table, whilst the rest swung their hammocks and cots in the verandah, an arrangement which, while possessing perhaps advantages of its own when the weather was fine, was not altogether the best when the nights were wet. Still the observers were not uncomfortable; and if they did not "fare sumptuously every day," yet, with abundance of fish and cocoanuts, they did not live altogether badly.

The weather, with the exception of one severe rain-storm, was pleasant during the sojourn of the observers, although nearly every day slight showers were brought to the island by flying clouds.

On the evening of the 22nd, just as the *Hartford* was casting off for Tahiti, *L'Éclaireur* came in with the French expedition, consisting of MM. Janssen, Trouvelot, Palisa, and Tacchini on board.

The preparations for the eclipse proceeded briskly, and by April 28 the siderostat, equatorial, and photoheliograph were erected and adjusted in position. The spectroscopes were next taken in hand, and the rating of the clocks proceeded with. This took some time; but matters had so far advanced by May 1 that from that date, with the exception of May 4, when the weather was wet, two rehearsals of the observations were made daily, one at 7, the other at 11.30 a.m. Messrs. Preston and Brown of the U.S. Coast and Geodetic Survey during this period made pendulum observations.

By the evening of May 3 the photographers were nearly ready to take trial plates, and these they hoped to obtain the following day. The hitherto fine weather, however changed, and before noon the next day five inches of rain had fallen, and the photographic dark room which had been erected was destroyed, all the dye being washed out of the ruby curtains and window. This damage being repaired, an attempt was made to obtain trial plates the next day, but the length of time occupied in rehearsing the observations, and the still unsettled state of the weather, prevented this being done. The early morning of the eclipse found the weather in the same unsettled state; about nine o'clock, however, the clouds began to disperse themselves, and by ten o'clock the sky was moderately clear. After the first contact the lenses were dusted, the slits of the spectroscopes cleaned, and the adjustments finally inspected.

With regard to the work of observation itself, this was done in accordance with the programme laid down before the observers left England, although the time-table of exposures was slightly departed from to meet the circumstances of the case, as, for instance, a greater length of totality than was expected, the duration being five minutes twenty-five seconds. During the eclipse the direction and velocity of the wind remained constant,

whilst the meteorological observations of Mr. Upton showed a slight rise in barometric pressure, a rise in humidity, and a fall of temperature, the latter reaching even the nightly values, whilst radiation thermometers showed that the heat received by the earth was almost *nil*.

The observations with the photoheliographs which the English observers took out being taken in hand by Lieut. Qualtrough of the American navy.

Perhaps some details as to the work itself may be of interest.

First with regard to the work of Mr. Woods. A red-end collodion plate was washed and placed by him in one of the prismatic camera slides five minutes before totality. Four minutes later he started the clockwork of the integrating spectroscopic slide. Forty seconds before totality exposures were made in the Rowland grating cameras, and at totality the prismatic camera and slit spectroscope were each opened.

In the spectroscopes which were under the care of Mr. Lawrence the exposures commenced ten minutes before totality, his work continuing until ten minutes after totality.

The photoheliographs as we have said were looked after by Lieut. Qualtrough, the plates which he exposed in these instruments being given to Mr. Woods after the eclipse. During the intervals in the exposures of the plates the observers found time to note the corona. In its general character it seems to have much resembled that seen last year in Egypt, but its light was of a more natural tone, the landscape lacking the weird colouring so marked a feature in the Egyptian eclipse

Mr. Lawrence examining the corona with the finder was able to detect much delicate detail, especially in those portions of it near the preceding limb of the moon. He also examined it with a small pocket spectroscope with lens. Taking out the prisms during mid totality he could see the green ring, and very faintly towards the end C and D_3. After totality he still saw the 1474 ring, as well as the red and yellow ones; these latter, however, being as before very faint. Replacing the prisms he could see then only the 1474 line, that examined by Prof. Hastings. The F line, for which he had specially searched, was not seen by him at all. Mr. Lawrence agrees in thinking that the coronal light was of a more natural tint than it was in the eclipse last year. Mr. Dixon of the American party made a careful sketch of the corona, showing five well defined streamers. Soon after totality the photoheliograph clock was stopped, and an endeavour made to obtain the run of the sun's crescent on the two cameras for the purpose of orientation, but, owing to the prevalence of clouds, the attempt was only successful with one, the smaller instrument, with which two exposures were obtained on one plate.

So much for the observations themselves. As to the results we learn that the photographs taken with the small photoheliograph are very good, that which had two minutes exposure showing as much as those which M. Janssen exposed during the whole of totality. The large photoheliograph has not given such good results, all the plates taken showing signs of slight shifts. Still it is believed that, by combining the photographs on each of the nine plates, the whole structure of the corona from the limb to its outmost limits will be obtained.

With the first order grating H and K were obtained as bright lines just before, and immediately after, totality, but with the second order grating no result seems to have been obtained; at least the observers could see nothing when they examined the plate on the island. The photographs taken with the dense prism spectroscope, like those obtained with the first order grating, show bright lines at the commencement and end of totality, particularly at the end, the photograph taken then showing H, K, *h*, *f*, and F very distinctly.

The integrating spectroscope also did useful work. Although no result was obtained during totality with this instrument, the flash of bright lines before and again after totality were successfully photographed by it. The more prominent lines in these photographs are those which belong to hydrogen and the lines H, K, and 1474.

The slit spectroscope was also successful, giving a good photograph from the ultra violet to the green. This spectrum, whilst being in the main a continuous one, is not the same on the two sides of the disk, nor are the lines so numerous as those obtained last year in Egypt. H and K are very strong in the present photograph, but in this respect also the spectrum differs from that obtained in Egypt, these lines then extending across the interval, which is not so in the present photograph. The hydrogen line near G, however, extends over nearly a solar diameter; and *h*, F, 1474, *b*, and other lines have also been obtained.

With regard to the gelatine red-end plates of the prismatic camera, although they gave good photographs, yet the almost entire absence of prominences will diminish their value. In the eclipse of last year, when many prominences were visible, these plates were used with good results. The Rowland grating, too, seems to have given no useful result, but this is probably due, like the small measure of success with the prismatic camera, to the comparative absence of prominences.

In developing the red-end plate immediately after totality Mr. Woods was unfortunate enough, owing to his having to manipulate it almost entirely in the dark, to get it torn, and nothing now remains but the gelatine edging.

The work now being complete, the things began to be repacked for the homeward journey. The *Hartford* returned to Caroline on the 8th, the work of reembarkation commenced, and on the 9th the expedition left.

The observers were almost sorry to leave the island, as their sojourn there had been a most pleasant one. Like most of its kind it is well wooded, the graceful outlines of the cocoanut palms being characteristic features in the pretty scenery which the island affords.

By day the smaller hermit crab swarmed the sandy beach, feeding on what decayed animal matter it could find, whilst at night the large red hermit crabs covered the same beach in their hundreds, they preferring dead vegetable matter. The lagoon too, around which the little islets arrange themselves, was a never-failing source of interest and amusement, and in boating there, and in the deeper water off the reef, or in hunting the shore in search of the brilliant-coloured shells and coral with which the island abounds, the observers found much amusement.

In deep water bivalve shells more than two feet across were observed, whilst the reefs at low water were covered with smaller representatives of the same or a similar species, which threw jets of water into the air. Several octopi were caught by the various members of the expedition, and many beautiful sea-urchins picked up by them in their daily walks. Thus did they spend their spare hours, and it was therefore with some regret that they saw the outlines of the island disappear on their horizon. The *Hartford* was bound for Honolulu in the Sandwich Islands. The voyage was however broken at Hilo, Hawaii, in order that the members of the expedition might visit the celebrated volcano of Kilauea. Honolulu was reached on May 30. Here Messrs. Preston and Brown, who were to continue their pendulum observations remained, the rest of the expedition proceeding in the *Zealandia* for San Francisco. The English observers left at Honolulu copies of the photographs they obtained, to be forwarded to England by the next mail. They left San Francisco on June 15, and may therefore be expected to arrive in England about the end of the present month.

THE ARCHÆOLOGY OF SOUTHERN CALIFORNIA[1]

A VALUABLE contribution to American anthropology has recently appeared, published under the auspices of the U.S. Government, forming the seventh volume of the "Reports of the U.S. Geographical Surveys West of the One Hundredth Meridian." It deals mainly with the remains of the Indians of Southern California, their implements, weapons, vessels, and ornaments.

The observers and collectors were those engaged upon the work of the survey, some of them detailed for work of a different character, but fortunately able to render valuable assistance in explorations for archæological finds.

The letterpress embodies the work of F. W. Putnam, the distinguished curator of the Peabody Museum, whose editorial revision and direction has moulded the whole, that of A. A. Abbott, the veteran explorer of the antiquities of New Jersey, H. C. Yarrow, S. S. Haldeman, A. S. Gatschet, H. W. Henshaw, and Lucien Carr, whose report upon the measurements of the crania from California is most suggestive and important. Besides their own contributions to the principal subject, these gentlemen have freely used the short descriptions of the personal visits of the officers of the army and others to the Pueblo villages of New Mexico and Arizona.

The present inhabitants of Central and Southern California are regarded as a degenerate race deteriorated from an ancestral people of superior parts, and they afford to-day a marked contrast with the more advanced and intelligent races of Northern California. This inferiority has been recognised by all observers, and was comprehended by the Jesuit missionaries, whose unfortunate system, however much its zealous propagation recommended their vigour and sincerity, only helped the natural tendency and hastened the course of a degradation already under way.

As early as 1534 the Spanish explorers invaded this region, and met in many instances a warlike and determined resistance. The priest and missal followed the sword and helmet, and completed the destruction of the people by processes more insidious than those of the warrior, but scarcely less fatal. Missions were established, the natives proselytised, not always by moral suasion, and brought under the control of the missions; they existed in a state of appanage, and became listless and degraded.

The natives of the immediate southern border of California show an improvement over those of Central California, approximating to the superior type in Northern California, a contrast which has so impressed the minds of students as to have started the assumption that the Central Californians belong to a different race, and are to be referred to Malay and Chinese origins. It is however with the description of the implements, utensils, ornaments, &c., of the southern Indians as exhumed from burial mounds, and the story told by such mortuary relics of the habits of their ancestors, that this volume is filled.

Attention had been directed by the Smithsonian Institution to the area upon the coast of California opposite the group of Santa Barbara Islands, and to these islands themselves, as a promising field for archæological search. The indications followed rewarded the Survey with many important objects, enough to permit a conception of the life of their makers.

These latter were in the stone age depending upon stone and bone implements as tools of war, chase, and industry. They seem to have been almost entirely without a knowledge of pottery, but this need may have been scarcely felt from their skill in the manufacture of stone vessels formed from steatite masses, and of all sizes, and adapted to the commonest domestic uses.

This series of objects affords a striking example of their patience and ingenuity. They are described under the designation of "Cooking pots and food vessels." They are in the main oblate spheroidal vessels of soapstone thickened over the base and sides exposed to the heat, and thinning towards the rim of the circular opening upon the top. The smaller specimens are frequently much finished in their smoothness, and vary enough in size and shape to suggest that they were the property of individuals, and prepared and kept for the personal use of their owners. These small vessels often show mending where fractured, a row of holes being perforated upon the two opposite sides of a crack, and the edges drawn together by sinews which are sunk in grooves, over which has been plastered asphaltum. Asphaltum figures in various ways, and was constantly resorted to as a convenient cement; it was employed to fasten their stone-bolts and arrow-heads to their shafts, to attach mouth-pieces to their pipes, the line to their fish-hooks, &c., it formed a surface over their objects upon which ornaments could be imbedded in rude decoration, and figures on their shell beads in spiral lines of black.

Besides the *ollas*, various dish-like utensils are figured with one or more holes for suspension after use, or for removal from the fire, being probably used as baking pans. Stone mortars of basalt and sandstone, small colour mixers, dishes of shell (*Haliotis*), and cups formed of fish vertebræ complete the list of serviceable vessels.

The smoking-pipes, which are carefully studied and described by Dr. Abbott, are long, straight, conical, and sub-cylindrical tubes of steatite, displaying no great variety of form and but inconspicuous attempts at ornamentation. The straight tube corresponding to the bowl of the common pipe is in line with the opening at the insertion of the mouthpiece, and it would seem that tubes of bone or reed inserted for stems must have been curved to permit of their use in any normal position.

The chipped flints are of striking beauty, and will be recognised by all who have examined specimens of ornamental spears and daggers from this region. They are shown of natural size upon two plates of considerable beauty, and vary from 4 inches to 8 or 10 inches in length, lenticular in section, and present ripple-like and corrugated surfaces of very delicate sculpture. The chapters upon perforated stones, miscellaneous objects made of stone, and textile fabrics are especially interesting.

The claim of any great age for these relics seems precluded by their association with glass beads, bronze cups and platters, iron swords, nails, knives, and pistol barrels, all pointing unmistakably to contact with the Spaniards. Yet there can be but little doubt that they perfectly represent the arts of life prevailing among the ancestors of their owners and makers for ages before the appearance of the white man, and that many are themselves heirlooms descended from a great antiquity.

The concluding chapter of Part I. is a suggestive summary of the results of cranial measurements, and the writer, Lucien Carr, indicates the past presence of two races whose intermingling remains are now found upon the Santa Barbara Islands, one—the dolichocephals or long heads—presenting a picture of subjugation and decadence; the other—the brachycephals or short heads—spread over the mainland, occupying the northern islands, and pressing upon the precarious remnant of their predecessors on the southern islands.

Part II. is a diversified compilation of a number of personal narratives of visits to the Pueblo villages, some chapters upon the implements and pottery of their occupants, which seem of a degraded type compared with the productions of their probable ancestors, and a shor

[1] "Report upon the U.S. Geographical Surveys West of the One Hundredth Meridian in charge of First Lieutenant Geo. M. Wheeler." Vol. vii. "Archæology." (Washington, 1879.)

review of cranial measurements. The material seems insufficient and fragmentary, and affords imperfect means for judging in a satisfactory way of the exact status and organisation of these people. A final contribution to the linguistics of the subject, by A. S. Gatschet, closes the volume, with a compendious statement of the relations of the tribes of the western coast with a list of forty vocabularies of western languages.

Finally, this handsome volume, in typography, paper, and illustrations, is of irreproachable beauty, and it treats of a field in archæological study of deep interest and wide import. L. P. GRATACAP

THE SIZE OF ATOMS[1]

II.

IN making brass, if we mix zinc and copper together we find no very manifest signs of chemical affinity at all; there is not a great deal of heat developed: the mixture does not become warm, *it does not explode*. Hence we can infer certainly that contact-electricity action ceases, or does not go on increasing according to the same law, when the metals are subdivided to something like 1/100,000,000 of a centimetre. Now this is an exceedingly important argument. I have more decided data as to the actual magnitude of atoms or molecules to bring before you presently, but I have nothing more decided in *giving for certain a limit to supposable smallness*. We cannot reduce zinc and copper beyond a certain thickness without putting them into a condition in which they lose their properties as wholes, and in which, if put together, we should *not* find the same attraction as we should calculate upon from the thicker plates. I think it is impossible consistently with the knowledge we have of chemical affinities and of the effect of melting zinc and copper together, to admit that a piece of copper or zinc could be divided to a thinness of much less, if at all less, than 1/100,000,000 of a centimetre without separating the atoms or dividing the molecules, or doing away with the composition which constitutes as a whole the solid metal. In short, the structure as it were of bricks, or molecules, or atoms, of which copper and zinc are built up; cannot be much, if at all, less than 1/100,000,000 of a centimetre in diameter, and may be considerably greater.

I will now read you a statement from an article which was published thirteen years ago in NATURE.[2]

"Now let a second plate of zinc be brought by a similar "process to the other side of the plate of copper; a second "plate of copper to the remote side of this second plate of "zinc, and so on till a pile is formed consisting of 50,001 "plates of zinc and 50,000 plates of copper, separated by "100,000 spaces, each plate and each space 1/100,000 of "a centimetre thick. The whole work done by electric "attraction in the formation of this pile is two centimetre-"grammes.

"The whole mass of metal is eight grammes. Hence "the amount of work is a quarter of a centimetre-gramme "per gramme of metal. Now 4030 centimetre-grammes of "work, according to Joule's dynamical equivalent of heat, "is the amount required to warm a gramme of zinc or "copper by one degree Centigrade. Hence the work done "by the electric attraction could warm the substance by "only 1/16,120 of a degree. But now let the thickness of "each piece of metal and of each intervening space be "1/100,000,000 of a centimetre instead of 1/100,000. "The work would be increased a millionfold unless "1/100,000,000 of a centimetre approaches the small-"ness of a molecule. The heat equivalent would therefore "be enough to raise the temperature of the material by "62°. This is barely, if at all, inadmissible, according to "our present knowledge, or, rather, want of knowledge, "regarding the heat of combination of zinc and copper. "But suppose the metal plates and intervening spaces to "be made yet four times thinner, that is to say, the thick-"ness of each to be 1/400,000,000 of a centimetre. The "work and its heat equivalent will be increased sixteen-"fold. It would therefore be 990 times as much as that "required to warm the mass by 1° C., which is very much "more than can possibly be produced by zinc and copper "in entering into molecular combination. Were there in "reality anything like so much heat of combination as this, "a mixture of zinc and copper powders would, if melted in "any one spot, run together, generating more than heat "enough to melt each throughout; just as a large quantity "of gunpowder if ignited in any one spot burns throughout "without fresh application of heat. Hence plates of zinc "and copper of 1/300,000,000 of a centimetre thick, "placed close together alternately, form a near approxima-"tion to a chemical combination, if indeed such thin plates "could be made without splitting atoms."

Similar conclusions result from that curious and most interesting phenomenon, the soap-bubble. Philosophers old and young who occupy themselves with soap-bubbles, have one of the most interesting subjects of physical science to admire. Blow a soap-bubble and look at it, —you may study all your life perhaps and still learn lessons in physical science from it. You will now see on the screen the image of a soap-film in a ring of metal. The light is reflected from the film filling that ring, and focused on the screen. It will show, as you see, colours analogous to those of Newton's rings. As you see the image it is upside down. The liquid streams down (up in the image) and thins away from the highest point of the film. First we see that brilliant green colour. It will become thinner and thinner there, and will pass through beautiful gradations of colour till you see, as now, a deep red, then much lighter, till it becomes a dusky, yellowish white, then green, and blue, and deep violet, and lastly black, but after you see the black spot it very soon bursts. The film itself seems to begin to lose its tension, when it gets considerably less than a quarter of the wave-length of yellow light, which is the thickness for the dusky white, preceding the final black. When you are washing your hands, you may make and deliberately observe a film like this, in a ring formed by the forefingers and thumbs of two hands, and watch the colours. Whenever you begin to see a black spot or several black spots, the film soon after breaks. The film retains its strength until we come to the black spot, where the thickness is clearly much less than 1/60,000 of a centimetre, which is the thickness of the dusky white.

Newton, in the following passage in his "Optics" (pp. 187 and 191 of edition 1721, Second Book, Part I.), tells more of this important phenomenon of the black spot, than is known to many of the best of modern observers.

"Obs. 17.—If a bubble be blown with water, first made "tenacious by dissolving a little soap in it, it is a common "observation that after a while it will appear tinged with a "variety of colours. To defend these bubbles from being "agitated by the external air (whereby their colours are "irregularly moved one among another so that no accurate "observation can be made of them), as soon as I had "blown any of them I covered it with a clear glass, and by "that means its colours emerged in a very regular order, "like so many concentric rings encompassing the top of "the bubble. And as the bubble grew thinner by the con-"tinual subsiding of the water, these rings dilated slowly "and overspread the whole bubble, descending in order to "the bottom of it, where they vanished successively. In "the meanwhile, after all the colours were emerged at the "top, there grew in the centre of the rings a small round "black spot like that in the first observation, which con-"tinually dilated itself, till it became sometimes more than

[1] A lecture delivered by Sir William Thomson at the Royal Institution, on Friday, February 2. Revised by the Author. Continued from p. 205.

[2] See article "On the Size of Atoms," published in NATURE, vol. i. p. 551; printed in Thomson and Tait's "Natural Philosophy," second edition, 1883, vol. i. part 2, Appendix F.

"one-half or three-quarters of an inch in breadth before the "bubble broke. At first I thought there had been no light "reflected from the water in that place, but observing it "more curiously I saw within it several smaller round "spots, which appeared much blacker and darker than the "rest, whereby I knew that there was some reflection at the "other places which were not so dark as those spots. And "by farther trial I found that I could see the images of "some things (as of a candle or the sun) very faintly re-"flected, not only from the great black spot, but also from "the little darker spots which were within it.

"Obs. 18.—If the water was not very tenacious, the "black spots would break forth in the white without any "sensible intervention of the blue. And sometimes they "would break forth within the precedent yellow, or red, "or perhaps within the blue of the second order, before "the intermediate colours had time to display themselves."

Now I have a reason, an irrefragable reason, for saying that the film cannot keep up its tensile strength to 1/100,000,000 of a centimetre, and that is, that the work which would be required to stretch the film a little more than that, would be enough to drive it into vapour.

The theory of capillary attraction shows, that when a bubble—a soap-bubble for instance—is blown larger and larger, work is done by the stretching of a film which resists extension, as if it were an elastic membrane with a constant contractile force. This contractile force is to be reckoned as a certain number of units of force per unit of breadth. Observation of the ascent of water in capillary tubes, shows that the contractile force of a thin film of water, is about sixteen milligrammes weight per millimetre of breadth. Hence the work done in stretching a water film to any degree of thinness, reckoned in millimetre-milligrammes, is equal to sixteen times the number of square millimetres by which the area is augmented, provided the film is not made so thin that there is any sensible diminution of its contractile force. In an article "On the Thermal Effect of Drawing out a Film of Liquid," published in the *Proceedings* of the Royal Society for April, 1858, I have proved from the second law of thermodynamics that about half as much more energy, in the shape of heat, must be given to the film, to prevent it from sinking in temperature while it is being drawn out. Hence the intrinsic energy of a mass of water in the shape of a film kept at constant temperature, increases by twenty-four milligramme-millimetres for every square millimetre added to its area.

Suppose then a film to be given with a thickness of a millimetre, and suppose its area to be augmented ten thousand and one fold: the work done per square millimetre of the original film, that is to say, per milligramme of the mass, would be 240,000 millimetre-milligrammes. The heat equivalent of this is more than half a degree Centigrade (0·57°) of elevation of temperature of the substance. The thickness to which the film is reduced on this supposition, is very approximately 1/10,000 of a millimetre The commonest observation on the soap-bubble, shows that there is no sensible diminution of contractile force, by reduction of the thickness to 1/10,000 of a millimetre; inasmuch as the thickness which gives the first maximum brightness, round the black spot seen where the bubble is thinnest, is only about 1/8,000 of a millimetre.

The very moderate amount of work shown in the preceding estimates, is quite consistent with this deduction. But suppose now the film to be farther stretched, until its thickness is reduced to 1/10,000,000 of a millimetre (1/100,000,000 of a centimetre). The work spent in doing this is two thousand times more than that which we have just calculated. The heat equivalent is 280 times the quantity required to raise the temperature of the liquid by one degree Centigrade. This is far more than we can admit as a possible amount of work done in the extension of a liquid film. It is more than half the amount of work, which if spent on the liquid, would convert it into vapour at ordinary atmospheric pressure. The conclusion is unavoidable, that a water-film falls off greatly in its contractile force, before it is reduced to a thickness of 1/10,000,000 of a millimetre. It is scarcely possible, upon any conceivable molecular theory, that there can be any considerable falling off in the contractile force, as long as there are several molecules in the thickness. It is therefore probable that there are not several molecules in a thickness of 1/10,000,000 of a millimetre of water.

Now when we are considering the subdivision of matter, look at those beautiful colours which you see in this little casket, left, I believe, by Prof. Brand to the Royal Institution. It contains polished steel bars, coloured by having been raised to different degrees of heat, as in the process of annealing hard-tempered steel. These colours, produced by heat on other polished metals besides steel, are due to thin films of transparent oxide, and their tints, as those of the soap-bubble and of the thin space of air in "Newton's rings," depend on the thickness of the film, which, in the case of oxidisable metals, forms by combination with the oxygen of the air, under the influence of heat—a true surface-burning.

You are all familiar with the brilliant and beautifully distributed fringes of heat-colours on polished steel grates and fire-irons, escaping that unhappy rule of domestic æsthetics, which too often keeps those articles glittering and cold and useless, instead of letting them show the exquisite play of warm colouring, naturally and inevitably brought out, when they are used in the work which is their reason for existence. The thickness of the film of oxide which gives the first perceptible colour, a very pale orange or buff tint, due to the enfeeblement or extinction of violet light and enfeeblement of blue, and less enfeeblement of the other colours in order, by interference of the reflections from the two surfaces of the film, is about 1/100,000 of a centimetre, being something less than a quarter wave-length of violet light in the oxide.

The exceedingly searching and detective efficacy of electricity comes to our aid here, and by the force as it were spread through such a film, proves to us the existence of the film when it is considerably thinner than that 1/100,000 of a centimetre, when in fact it is so very thin as to produce absolutely no perceptible effect on the reflected light, that is to say, so thin as to be absolutely invisible. If in the apparatus for measuring contact electricity, of which the drawing is before you (NATURE, vol. xxiii. p. 567), two plates of freshly polished copper be placed in the Volta condenser, a very perfect zero of effect is obtained. If, then, one of the plates be taken out, heated slightly by laying it on a piece of hot iron, and then allowed to cool again and replaced in the Volta condenser, it is found that negative electricity becomes condensed on the surface thus treated, and positive electricity on the bright copper surface facing it, when the two are in metallic connection. If the same process be repeated with somewhat higher temperatures, or somewhat longer times of exposure to it, the electrical difference is augmented. These effects are very sensible before any perceptible tint appears on the copper surface as modified by heat. The effect goes on increasing with higher and higher temperatures of the heating influence, until oxide-tints begin to appear, commencing with buff, and going on through a ruddier colour to a dark-blue slate colour, when no farther heating seems to augment the effect. The greatest contact-electricity effect which I thus obtained between a bright freshly polished copper surface and an opposing face of copper, rendered almost black by oxidation, was such as to require for the neutralising potential in my mode of experimenting[1] about one-half of the potential of a Daniell's cell.

[1] First described in a letter to Joule, published in the *Proceedings* of the Literary and Philosophical Society of Manchester of Jan. 21, 1862, where also I first pointed out the demonstration of a limit to the size of molecules from measurements of contact electricity. The mode of measurement is more fully described in the article of NATURE (vol. xxiii. p. 567), referred to above.

Some not hitherto published experiments with polished silver plates, which I made fifteen years ago, showed me very startlingly, an electric influence from a quite infinitesimal whiff of iodine vapour. The effect on the contact-electricity quality of the surface, seems to go on continuously from the first lodgment, to all other tests quite imperceptible, of a few atoms or molecules of the attacking substance (oxygen, or iodine, or sulphur, or chlorine, for example), and to go on increasing until some such thickness as 1/30,000 or 1/40,000 of a centimetre is reached by the film of oxide or iodide, or whatever it may be that is formed.

The subject is one that deserves much more of careful experimental work and measurement than has hitherto been devoted to it. I allude to it at present to point out to you how it is that by this electric action, we are enabled as it were to sound the depth, of the ocean of molecules attracted to the metallic surface, by the vapour or gas entering into combination with it.

When we come to thicknesses of considerably less than a wave-length, we find solid metals becoming transparent. Through the kindness of Prof. Dewar, I am able to show you some exceedingly thin films, of measured thicknesses of platinum, gold, and silver, placed on glass plates. The platinum is of $1\cdot9 \times 10$ thickness, and is quite opaque;

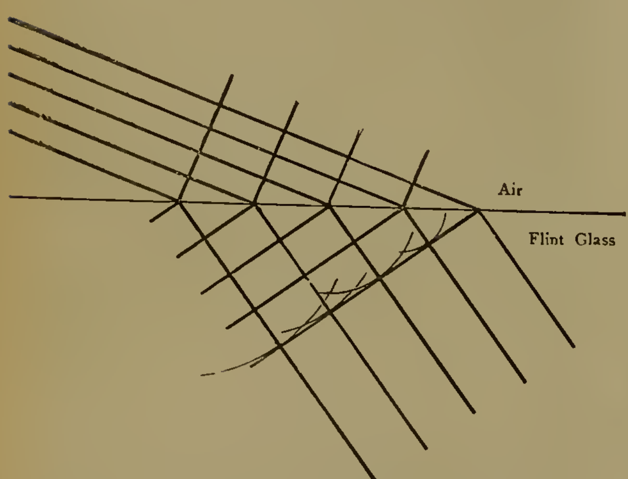


Fig. 3.—Diagram of Huyghen's construction for wave front of refracted light. Drawn for light passing from air to flint glass.

but here is a gold film of about the same thickness, which is transparent to the electric light, as you see, and transmits the beautiful green colour, which you see on the screen. The thickness of this gold (1·9, or nearly 2) is just half the wave-length of violet light in air. This transparent gold, transmitting green light to the screen as you see, at the same time reflects yellow light to the ceiling. Now I will show you the silver. It is thinner, being only $1\cdot5 \times 10^{-5}$ of a centimetre thick, or 3/8 of the air-wave-length of violet light. It is quite opaque to the electric light, so far as our eyes allow us to judge, and reflects all the light up to the ceiling. It is not wonderful that it should be opaque; we might wonder if it were otherwise; but there is an invisible ultra-violet light of a small range of wave-lengths, including a zinc-line of air wave-length $3\cdot4 \times 10^{-5}$, which this silver film transmits. For that particular light the silver film of $1\cdot5 \times 10^{-5}$ thickness is transparent. The image which you now see on the screen, is a magic lantern representation of the self-photographed spectrum, of light that actually came through that silver. You see the zinc-line very clear across it near its middle. Here then we have gold and silver transparent. The silver is opaque for all except that very definite light of wave-lengths from about 3·07 to 3·32.

The different refrangibility of different colours, is a result of observation, of vital importance in the question of the size of atoms. You now see on the screen before you a prismatic spectrum; a well-known phenomenon produced by the differences of the refractions of the different colours, in traversing the prism. The explanation of it in the undulatory theory of light, has taxed the powers of mathematicians to the utmost. Look first, however, to what is easy, and made clear by that diagram (Fig. 3) before you; and you will easily understand that refraction depends on difference of velocity of propagation of light, in the two transparent mediums concerned. The

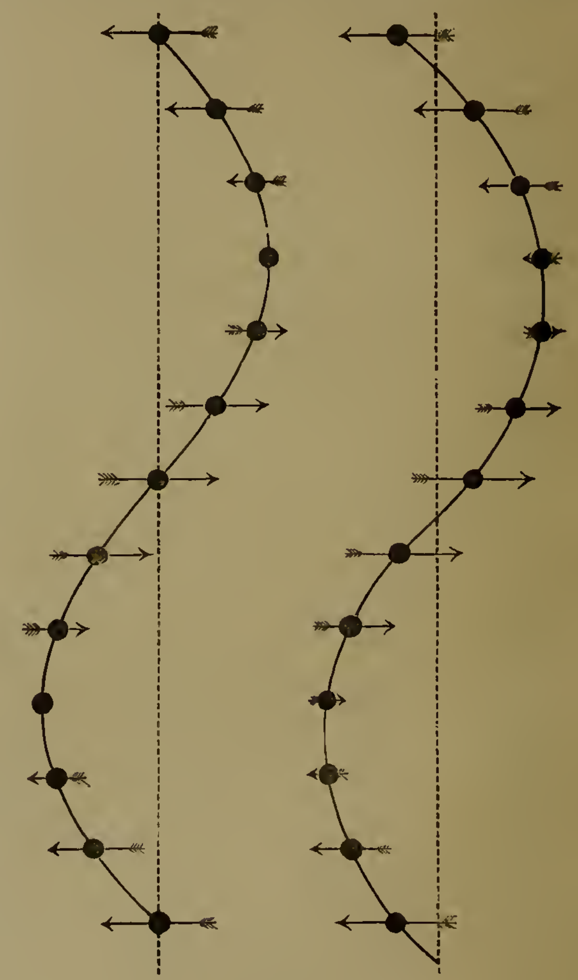


Fig. 4. Fig. 5.
Twelve particles in Wave-Length.

angles in the diagram are approximately correct, for refraction at an interface between air or vacuum and flint glass; and you see that in this case, the velocity of propagation is less in the denser medium. The more refractive medium (not always the denser) of the two, has the less velocity for light transmitted through it. The "refractive index" of any transparent medium, is the ratio of the velocity of propagation in the ether, to the velocity of propagation in the transparent substance.

Now, that the velocity of the propagation of light should be different in different mediums, and should in most cases be smaller in the denser than in the less dense medium, is quite what we should, according to dynamical principles, expect from any conceivable constitution of the

luminiferous ether and of palpable transparent substance. But that the velocity of propagation in any one transparent substance, should be different for light of different colours, that is to say, of different periods of vibration, is not what we should expect; and could not possibly be the fact if the medium is homogeneous, without any limit as to the smallness of the parts of which the qualities are compared. The fact that the velocity of propagation *does* depend on the period, gives what I believe to be irrefragable proof, that the substance of palpable transparent matter, such as water, or glass, or the bisulphuret of carbon of this prism whose spectrum is before you, is not infinitely homogeneous; but that on the contrary, if contiguous portions of any such medium, any medium in fact which can give the prismatic colours, be examined at intervals not incomparably small in comparison with the wave-lengths, utterly heterogeneous quality will be discovered; such heterogeneousness as that which we understand in palpable matter, as the difference between solid and fluid; or between substances differing enormously in density; or such heterogeneousness as differences of velocity and direction of motion, in different positions of a vortex ring in an homogeneous liquid; or such differences of material occupying the space examined, as we find in a great mass of brick building when we pass from brick to brick through mortar (or through *void*, as we too often find in Scotch-built domestic brick chimneys).

Cauchy was I believe the first of mathematicians or naturalists, to allow himself to be driven to the conclusion, that the refractive dispersion of light can only be accounted for, by a finite degree of molecular coarse-grainedness, in the structure of the transparent refracting matter; and as, however we view the question, and however much we may feel compelled to differ, from the details of molecular structure and molecular inter-action assumed by Cauchy, we remain more and more surely fortified in his conclusion, that finite grainedness of transparent palpable matter, is the cause of the difference of the velocity of different colours of light propagated through it, we must regard Cauchy as the discoverer of the dynamical theory of the prismatic colours.

But now we come to the grand difficulty of Cauchy's theory;[1] look at this little table (Table II.), and you will see

TABLE II.—*Velocity* (V) *according to Number* (N) *of Particles in Wave Length*

N	$V\left(=100\,\frac{\sin(\pi/N)}{\pi/N}\right)$
2	63·64
4	90·03
8	97·45
12	98·86
16	99·36
20	99·59
∞	100·00

in the heading, the formula which gives the velocity, in terms of the number of particles to the wave-length, supposing the medium to consist of equal particles arranged in cubic order, and each particle to attract its six nearest neighbours, with a force varying directly as the excess of the distance between them, above a certain constant line (the length of which is to be chosen, according to the degree of compressibility possessed by the elastic solid, which we desire to represent by a crowd of mutually interacting molecules). If you suppose particles of real matter arranged in the cubic order, and six steel wire spiral springs or elastic indiarubber bands, to be hooked on to each particle and stretched between it and its six nearest neighbours, the postulated force may be produced in a model with all needful accuracy; and if we could but successfully wish the theatre of the Royal Institution, conveyed to the centre of the earth, and kept there for five minutes, I should have great pleasure in showing you a model of an elastic solid thus constituted, and showing you waves propagated through it, as are waves of light in the luminiferous ether. Gravity is the inconvenient accident of our actual position, which prevents my showing it to you here just now. But instead, you have these two wave-models (see Fig. 2 above), each of which shows you the displacements and motions of a

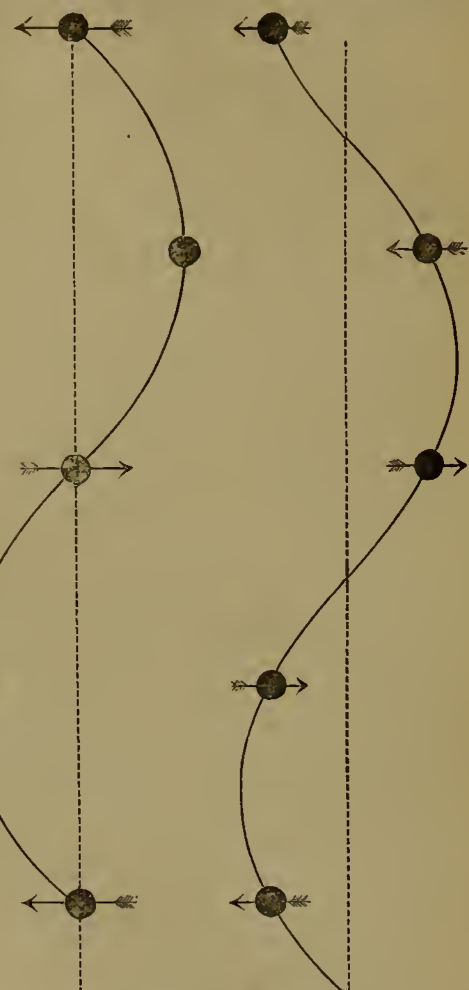


FIG. 6. FIG. 7.
Four particles in Wave-Length.

line of particles, in the propagation of a wave through our imaginary three-dimensional solid; the line of molecules chosen being those which, in equilibrium, are in one direct straight line of the cubic arrangement, and the supposed wave having its wave front perpendicular to this line, and the direction of its vibration, the direction of one of the other two direct lines of the cubic arrangement.

You have also before you this series of diagrams (Figs. 4 to 9) of waves in a molecularly-constituted elastic solid. These two diagrams (Figs. 4 and 5) illustrate a wave in which there are twelve molecules in the wave-length; this one (Fig. 4) showing (by the length and position of the arrows) the magnitude and direction of velocity of each molecule, at the instant when one of the

[1] For an account of the dynamical theory of the "Dispersion of Light," see "View of the Undulatory Theory as Applied to the Dispersion of Light," by the Rev. Baden Powell, M.A., &c. (London: 1841)

molecules is on the crest of the wave, or has reached its maximum displacement; that one (Fig. 5) showing the magnitude and direction of the velocities, after the wave has advanced such a distance, as (in this case equal to one-twenty-fourth of the wave-length) to bring the crest of the wave to midway between two molecules. This pair of diagrams (Figs. 6 and 7) shows the same for waves having four molecules in the wave-length, and this pair (Figs. 8 and 9) for a wave having two molecules in the wave-length.

The more nearly this critical case is approached, that is to say the shorter the wave-length, down to the limit of twice the distance from molecule to molecule, the less becomes the difference between the two configurations of

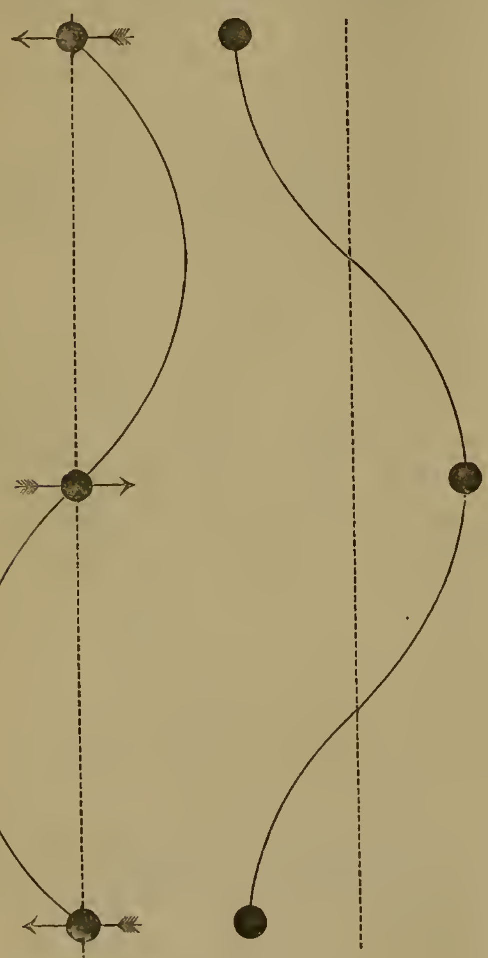


FIG. 8. FIG. 9.
Two particles in Wave-Length.

motion, constituted by waves travelling in opposite directions. In the extreme or critical case, the difference is annulled, and the motion is not a wave-motion, but a case of what is often called "standing vibration." Before I conclude this evening, I hope to explain in detail the kind of motion which we find instead of wave-motion (become mathematically imaginary), when the vibrational period of the exciter is anything less than the critical value; because this case is of extreme importance and interest in physical optics, according to Stokes' hitherto unpublished explanation of phosphorescence.

This supposition of each molecule acting with direct force only on its nearest neighbour, is not exactly the postulate on which Cauchy works. He supposes each molecule to act on all around it, according to some law of rapid decrease as the distance increases; but this must make the influence of coarse-grainedness on the velocity of propagation smaller than it is on the simple assumption, realised in the models and diagrams before you, which therefore represents the extreme limit of the efficacy of Cauchy's unmodified theory to explain dispersion.

Now, by looking at the little table (Table II.) of calculated results, you will see that with as few as 20 molecules in the wave-length, the velocity of propagation is 99½ per cent. of what it would be with an infinite number of molecules; hence the extreme difference of propagational velocity, accountable for by Cauchy's unmodified theory in its idealised extreme of mutual action limited to nearest neighbours, amounts to 1/200. Now look at this table (Table III.) of refractive indices, and you see that the difference of velocity of red light (A), and of violet light (H), amounts in carbon disulphide to 1/17; in dense flint glass to nearly 1/30; in hard crown glass to 1/73; and in water and alcohol to rather more than 1/100. Hence, none of these substances can have so many as 20 molecules in the wave-length, if dispersion is to be accounted for by Cauchy's unmodified theory, and by looking back to the little table of calculated results (Table II.), you will

TABLE III.—*Table of Refractive Indices.*

Line of Spectrum.	Material.				
	Hard Crown Glass.	Extra dense Flint Glass.	Water at 15° C.	Carbon Disulphide at 11° C.	Alcohol at 15° C.
A	1·5118	1·6391	1·3284	1·6142	1·3600
B	1·5136	1·6429	1·3300	1·6207	1·3612
C	1·5146	1·6449	1·3307	1·6240	1·3621
D	1·5171	1·6504	1·3324	1·6333	1·3638
E	1·5203	1·6576	1 3347	1·6465	1·3661
b	1·5210	1·6591	—	—	—
F	1·5231	1·6442	1·3366	1·6584	1·3683
G	1·5283	1·6770	1·3402	1·6836	1·3720
h	1·5310	1·6836	—	—	—
H	1·5328	1·6886	1·3431	1·7090	1·3751

The numbers in the first two columns were determined by Dr. Hopkinson, those in the last three by Messrs. Gladstone and Dale. The index of refraction of air for light near the line E is 1·000294.

see that there could not be more than twelve molecules in the wave-length of violet light in water or alcohol; say ten in hard crown glass; eight in flint glass; and in carbon disulphide actually not more than four molecules in the wave-length, if we are to depend upon Cauchy's unmodified theory for the explanation of dispersion. So large coarse-grainedness of ordinary transparent bodies, solid or fluid, is quite untenable. Before I conclude I intend to show you, from the kinetic theory of gases, a *superior limit* to the size of molecules, according to which, in glass or in water, there is probably something like 600 molecules to the wave-length; and almost certainly *not fewer* than two, or three, or four hundred. But even without any such definite estimate of a superior limit to the size of molecules, there are many reasons against the admission that it is probable or possible. there can be only four, or five, or six to the wave-length. The very drawing by Nobert of 4,000 lines on a breadth of a millimetre, or at the rate of 40,000 to the centimetre, or about two to the ether wave-length of blue (F) light,[1] seems quite to negative the idea of any such possibility, of only five or six molecules to the wave-length, even if we were not to declare against it from theory and observation of the reflection of light from polished surfaces.

(*To be continued.*)

[1] Loschmidt, "quoting from the Zollvereins department of the London International Exhibition of 1862, page 83, and from Harting 'On the Microscope,' page 881," *Sitzungsberichte der Wiener Akademie Math. Phys.* 1865 vol. lii.

STELLAR PHOTOGRAPHY AT HARVARD

AT the meeting of the Astronomical Society which was held on June 8 last, Prof. Pickering of Harvard College Observatory, so well known for his stellar observations, and who is a Foreign Associate of the Society, attended and gave an interesting account of the work which has been done during the past few years at his observatory.

Some few years ago Prof. Pickering took up the work of determining the intensity of the light of the principal stars by eye observation, without taking the question of colour into consideration, work which has been already dwelt upon in this journal. For this purpose he used a photometer, completing his observations, which number some 90,000, about a year ago, and a large part of his results are already in print. The published results of the more important investigators of star magnitudes, from the time of Almagest and Lûfi, have also been reduced. Sir W. Herschel's observations, which appeared almost a century ago in the *Philosophical Transactions*, have likewise been taken in hand at Harvard Observatory and completely discussed. Sir John Herschel's works, the "Uranometria Nova," the "Durchmusterung," as well as many other works in the same field, have also been made use of in preparing the Harvard Catalogue, which therefore shows those cases in which the photometric observations carried out by Prof. Pickering differ from the results obtained by other observers, when their observations are reduced to the same system. These eye observations of stars having been completed, Prof. Pickering, in conjunction with his brother, Mr. W. H. Pickering, has taken up stellar photography from the same point of view. By this means a comparison is obtained between the brightness of the star as seen by the eye, and its brightness as determined by its greater or less action upon the photographic plate; and by a comparison of photographs taken on different nights any variation in brightness may be detected; whilst the exact positions of stars may of course be more accurately and permanently recorded than by eye observations. Mr. A. A. Common recently, by taking photographs of the nebula in Orion on different nights and comparing them, has thus been able to detect a probable variation in one of the stars in the nebula, and in 1858 Professor George P. Bond, by measuring the diameters of stars in photographs was able to determine the relative brightness of the two stars which form the double ζ Ursæ Majoris.

But the work at Harvard University was to do more than this. The stars which Prof. Bond examined were close together. Prof. Pickering wished to compare stars far removed from each other. For this purpose the ordinary method of stellar photography, by which photographs are taken at the foci of large telescopes, would not suffice. These photographs only comprise a small region of but one or two degrees in diameter. A different method was therefore employed in the Harvard observations. A wholly different form to the ordinary equatorial telescope was used. It is not unusual to construct photographic cameras to take pictures of buildings which subtend an angle of 60° or even 90°. But when applied to the stars, however, the images at the edges are very poor, and only very small apertures can be used. It has, however, been found that some of the best lenses for pictures can be obtained covering a circle of 20° diameter without serious distortion, and at the same time large apertures can be used, thus reducing the time of exposure. In order to still further this work, Mr. W. H. Pickering investigated the sensitiveness of various photographic plates, and obtained some so sensitive that stars of the fifth and sixth magnitude have been photographed without using clockwork, they forming dots or making lines, as their images pass across the photographic plate, the length of these lines depending of course upon the time during which the plate is exposed. If the plate be exposed during ten seconds a distinct dot is obtained, whilst an exposure of thirty seconds causes a short line to be formed. The plates used at Harvard Observatory are six by eight inches. They are divided into six equal parts, each part being in turn exposed. By this means six regions of the heavens, each about 15° square, may be photographed on one plate; and by a variation in the dot and line system employed, sometimes taking the dot and sometimes the line first, three pictures may be taken on a single division of one of the plates without giving rise to any confusion. Instead of simply six, therefore, eighteen photographs are taken on one of these plates, so that on a single plate a portion of the heavens of more than three hours' right ascension, and extending from 30° S. to 60° N., may be included. Since each portion of the plate covers a region of about 15°, the camera mounting has a series of notches or stops, by which it may be instantly moved through that amount either of right ascension or declination.

When photographing the following is the exact method employed. The first exposure takes the region between 30° and 15° south declination, and between one hour and a half and half an hour west of the meridian. First the plate is exposed for ten seconds, and each star records itself by a dot. The plate is then covered for ten seconds; next it is exposed for a period of thirty seconds, and each star makes a line on the plate. By means of the clamping arrangement to which we have referred the plate is then moved through one hour in right ascension. This takes up the remaining few seconds of the minute, so that the taking of the next photograph begins with the first second of another minute. The camera is then on the meridian. The same part of the plate is again exposed, and in order to distinguish this series of stars from those first photographed, this time the plate is exposed first during thirty seconds, and then during ten, so that the result is a line followed by a dot. This gives the second series. But the same portion of the plate may be again used. The remaining ten seconds of the second minute, like those of the first, are spent in moving the camera through another hour of right ascension. Then a fresh exposure is made for thirty seconds, a line simply being obtained without a dot, and this completes the series. The first class of images is in dots and lines, the second in lines and dots, the third is recognised by the presence of lines alone. The thirty seconds which remain of the third minute are employed in exposing a second portion of the plate, and changing the position of the camera, which now takes in the region from 15° S. to the equator. The same process is then gone through again, three exposures as before being made in three different positions of right ascension. By continuing this process, taking three photographs on each of the six portions into which the plate is divided, the whole region included between the declinations of $-30°$ and $+60°$, and between three hours of right ascension, $1\frac{1}{2}$ hours on each side of the meridian, being one eighth of the whole heavens, excluding the circumpolar stars, will be photographed on one plate, the whole operation occupying but eighteen minutes. With regard to those stars in the vicinity of the Pole, some other method will have to be adopted. Thus much for one branch of the work—and an important branch—carried on at Harvard Observatory.

Another portion of their work consists in the preparation of a photographic map of the entire heavens. The method just described, in which clockwork is dispensed with, only enables those stars whose magnitude is not less than five or six to be photographed, and stars of a less magnitude than this must of course be included in a map of the heavens. The camera in this work, therefore, is driven by clockwork. By this means stars of the eighth magnitude record their images on the photographic plate, and as many as 200 are visible in the paper print within a

circle of 5° in diameter. A photograph taken in this way of a portion of the constellation of Orion, besides showing the three stars of the Belt and the Sword-Handle, gives an interesting picture of the nebula.

With reference to the question of the colours of stars it is interesting to note the faintness of α Orionis in the photographs. To the eye its brilliancy is almost as great as that of β, whilst in the photograph it is not more prominent than λ. The reason is to be found in the colour of α. It is a red star, and consequently makes but little impression on the photographic plate.

Again, in the constellation Cetus the three stars which are brightest to the eye are α, γ, and δ. A, which is the brightest of the three, has close to it a very faint companion, scarcely visible to the naked eye, its magnitude being given as 6·3, whilst that of α is 2·7. This is the appearance of this part of that constellation as seen by the eye. A photograph of this region was taken at Harvard with the result that the small star is seen in the photograph nearly as bright as α, it being only three-tenths of a magnitude less. The colour of these stars again explains this, α being of a reddish tint, whilst the small star is of a deep blue colour, and being so the rays which flow from it have a greater influence on the photographic plate. A comparison of the number of stars seen in the photograph of Orion with the number in the photometric catalogue, further illustrates this effect of colour. In that part of this constellation included between 5° north and 5° south declination, and 75° to 90° of right ascension, sixteen stars were common to photograph and catalogue; a like number, being either too small in magnitude or too red in colour, although catalogued, remain unrecorded on the photographic plate; whilst five others seen in the photograph are not given in the catalogue. A reduction has been made of the results given by the plates of different makers, with the view of ascertaining the value of the deviation. In two of such plates the average deviation was 0·21 of a magnitude, and in two measurements of the same plate it was found to be 0·07 of a magnitude.

It is obvious from this account of the work at Harvard that star photography is entering into a new phase, and one which will entirely replace the present system of eye observations, for the reason that, whilst the eye is so variable, photographic plates may now be obtained, doing their work with almost definite wave-lengths of light. The constant record of the plate, must in time therefore be preferred to observation by the variable eye. At the same time as photography advances, if it be considered necessary to obtain photographic star maps to record the observations of the average eye, there will be no difficulty in this being done.

NOTES

IN accordance with the provisions of the Statutes, the Council of the Royal Society met last Thursday to elect one from among themselves to fill the office of president until the annual election on November 30. The choice, as had been anticipated, fell upon Prof. Huxley. We believe that this *ad interim* election has given the greatest satisfaction to all the Fellows of the Society.

WE have received from the Johns Hopkins University, Baltimore, the circular giving the programme for the next academic year and a report on the work of the year that is past. Not only are a great number of subjects included in the programme of this University, but provision is made that the work in each section shall be thoroughly done, and we think the Trustees are to be entirely congratulated upon the progress that is being made. Among the scientific subjects we find physics, chemistry, geology, mineralogy, and biology in all its branches. With the other subjects which the programme sets forth we have here of course little to do, but we must add that we are glad to note under the heading "Philosophy" that the study of psychology is well provided for. Not only are there courses of lectures, but a limited number of the students are provided with seats in the physiological laboratory, where they may prosecute original research. It is so in all the scientific subjects. The work of the advanced student is arranged with a view of initiating him into the methods of original investigation, which, when he has finished his course of instruction, he is encouraged to carry on. Thus in the physical laboratory, which is under the direction of Prof. Rowland and Dr. Hastings, during the past year original investigations have been carried on in many parts of the subject; for instance, to name one or two, the concave grating has been used in an attempt to photograph the spectrum, and with it an endeavour has also been made to ascertain the wave-lengths of the lines. The unit of electrical resistance has also been investigated during the past year, and during the coming session an attempt will be made to establish an international unit for such resistance. We notice too, as a feature of the advanced course in physics conducted by Prof. Rowland, that besides the lectures and laboratory work there are weekly meetings for the discussion of the current literature of the subject. The courses in chemistry, which are under the sole control of Prof. Remsen, are likewise excellent. Besides the ordinary courses in general and analytical chemistry, the programme states that arrangements will soon be made by which the study of applied chemistry—for example, metallurgy, the chemistry of iron and steel, of dye stuffs, of soils and fertilisers—may be taken up by the students. Original research has been a prominent feature in this laboratory also, the results appearing in the *American Chemical Journal*. With regard to mineralogy and geology we notice that they are included in the courses on chemistry. The courses on biology are most excellent, general biology, embryology, osteology, and plant analysis being included in the first year's work. In the second year the student takes up mammalian anatomy, animal physiology and histology, and animal morphology. Then when the student desires to take up the study of marine animals, the University provides him with a laboratory by the sea itself. This laboratory was open last year from May 1 until September 29, and during that time the development of Thallassema was investigated, studies were made with regard to the origin of the oyster-shell, the parthenogenesis of the Echini, the development of Tubularia, and other subjects, which want of space alone prevents our mentioning. The results of these investigations are published in "Studies from the Biological Laboratory"; abstracts of two of these researches have also been printed in the *Proceedings of the Royal Society*, and Dr. E. B. Wilson's paper on the Development of Renilla will appear in the *Philosophical Transactions*. We might add much more to what we have said concerning the excellent character of the work done at this University, as we do not doubt that the other courses are as well provided for as the more purely scientific subjects to which alone we have referred. The Johns Hopkins University, in fact, although but a new institution, has been founded on a broad basis, giving to the student those opportunities for original work which it is so difficult to obtain elsewhere. We should much like to see such an account of original research done and to be done issuing each year from the laboratories of Oxford and Cambridge.

THE Berlin Academy of Sciences has elected Prof. Simon Newcomb (Washington) and Prof. B. Apthorp Gould (director of Cordova Observatory) as corresponding members.

IN our review of the life of Sir Edward Sabine, which appeared in our issue of last week (p. 219), we stated that he accompanied the expedition which under the command of Capt. James Ross was sent to make a magnetical survey of the Antarctic regions. This was an error, as although all the observations

were investigated and discussed by him he was not with the expedition, but had the observations forwarded to him at regular intervals.

THE whale which was found by a fisherman in Selsea Bay some six weeks since, and presented to the Brighton Aquarium, is a valuable addition to that establishment. Although undoubtedly belonging to the whale family, competent authorities have pronounced it to be a bottle-nosed dolphin, a creature rarely to be seen alive in an aquarium. It has been placed in a tank which holds 100,000 gallons of water, and is 110 feet in length, so that the animal, which is ten feet long, has some amount of freedom. It seems to be doing quite well, for not only has it not lost in bulk since its capture, but has even gained, weighing now more than eight hundredweight. It is very tame, taking its food from the attendant. At present it subsists upon mackerel, that being the food most easily obtained just now. Of these it takes five meals each day, and manages to eat some 400 of them during a week. The mackerel season is, however, almost over, and some other diet must be found for the animal, perhaps herrings. When first placed in the tank it retreated to one end. After a week's sojourn there, it sought the other end of the tank. Here it remains, swimming in circles. When swimming it keeps close to the surface of the water, moving through it with a graceful undulating movement, coming now and again to the surface, and taking in a fresh supply of air about every third or fourth time it thus rises. The animal is certainly an interesting acquisition to the Aquarium.

THE balloon of the Paris Observatory has been in working order for some weeks. Its capacity being only sixty cubic metres, it was found difficult to use it except in calm weather. The motions of the registering apparatus are an obstacle to correct readings. The experiments, conducted by Admiral Mouchez, are stated to be only preliminary to further aërostatical experiments. The subject is quite new, scientific ballooning being only in its infancy, and it is only by gradual investigation that the extent of the services it can render to science can be ascertained.

A CORRESPONDENT of the *North China Herald* describes a journey from Hankow on the Yangtsze to Chunching in Szechuen, a distance of 720 geographical miles. After passing Ichang, the highest port on the great river opened to foreign trade, the first of the celebrated gorges is entered, and the mountainous country which extends up to and beyond Chunching begins. Through these ranges, which mostly run in a north and south direction, the Yangtsze, here called the Ch'uan Ho, or river of Szechuen, forces its way. Leaving the wild, little-inhabited country of the gorges behind, the traveller, on reaching Wan-hsien, 160 geographical miles above Ichang, emerges into a country of picturesque sandstone hills, at this season covered from base to summit with poppy gardens, with not a vacant spot except where perpendicular cliffs prevent all access. He emerges, too, among a people remarkable for their polished manners and especial politeness to Europeans. While Hupeh province was suffering from floods, the traveller found Eastern Szechuen, from Kweichow to Wan-hsien, praying for rain. The drought here had extended over six months, the south gates of the cities were closed (as facing the *yang* or fire-element), and all slaughtering of animals was forbidden. From Wan-hsien to Chunching, a distance of 200 miles, the aspect of the river remained the same—a succession of winding reaches, nearly all, owing to the peculiar sandstone formation, running at right angles to each other, and united by the customary rapid. Cliffs were frequent, and the sites of the towns and cities, built on steep projecting knolls, their walls and battlements crowning the precipices, are admirable. At length, two months from Shanghai, the traveller reached Chunching, the commercial metropolis of Szechuen, in which, by the Chefoo Convention, the English Government is authorised to maintain a Resident, who watches the commercial prospects and movements of the great provinces of Szechuen and Yunnan.

THE Paris *Figaro* recently published a special supplement on Tonkin, and if the writer is to be credited, that country is one of the richest in the world. Its gold mines, he says, can rival those of California and Australia. The natives use that metal for exchange; the females of the Muongs of the Black River, on their way to and from market, gamble with thousands of francs worth of it, without caring whether they win or lose. The mines of Talan, near Yuen-kiang, on the Red River, were visited by the Commission of the Meikong, who found gold there in bars as well as dust. Still higher, near the source of the Red River, the precious metal is obtained in large quantities. Silver also is not rare, and copper is found everywhere, all the domestic utensils of the people being made of this metal. The tin mines are not worked for want of capital, although those worked near Mong-tsze, in Yunnan, near the Red River, are the most valuable known to exist. Zinc, lead, iron, and bismuth are also known. The coal mines, however, are the most important of all. Tonkin produces also musk, tortoise-shell, mother-of-pearl, wax, silk, peacocks' feathers, as well as those of the blue pheasant, and other birds of brilliant plumage. "In short," concludes the *Figaro*, "it is a rich country, and worth the trouble of occupying it."

ANOTHER trial has been made in Paris of the electric tramcar in which Faure-Sellon-Volckmar accumulators were employed. The experiment was preceded by a lecture given by M. Philippart, tending to show the great economic superiority of electricity over the employment of horses. On this occasion the route chosen was not, as formerly, from the Place des Nations to La Muette and Trocadéro, but from Trocadéro to the Louvre and thence to the Place des Nations by the Bastille, an alteration made to show the capacity of the electric tramcar for ascending slopes on the common roads.

DR. OBACH has lately perfected his tangent galvanometer with a swinging coil. In the present form the coil is compound, being in reality one for measuring quantity and another for measuring electromotive force. The coil is movable on a horizontal axis, and therefore can be inclined at any angle. It has the advantage over a tangent galvanometer in having a suspended needle which can be rendered dead beat; the coils are also balanced so that the deflection corresponding to one volt with the high resistance coil is that which corresponds with one ampere with the low resistance coil. This instrument promises well for practical testing if made in a convenient portable form.

THE last number of the *Zeitschrift der Gesellschaft für Erdkunde* of Berlin contains a paper by Dr. F. Boas on the former distribution of the Eskimo in the Arctic-American archipelago. After referring to the discovery by Arctic travellers, in places where no human foot appears now to tread, of traces of habitations, graves, weapons, &c., he says that two theories have been broached to account for these remains. One is that the ice has encroached more and more on the sea, and driven away the people; the other that there has been a migration from the west across the archipelago. Dr. Boas rejects both of these explanations. He points out that, judging by the remains, the former inhabitants led precisely the same life as the Eskimo that we know to-day. He comes to the conclusion, after an examination of the various islands, of the distribution of traces of previous inhabitants and of the present tribes, that for numerous reasons we must abandon the theory that there was an earlier extension of inhabitants towards the north. He thinks that the remains found are those of the present tribes who have been driven from place to place by the necessity of obtaining subsistence, and refers to the

custom of several tribes to abandon huts in which death has taken place and to leave them standing. The hunting-grounds too would change from time to time according to the severity of the winter. A hard and fast boundary line cannot be laid down for inhabitants of the Arctic regions any more than for the flora. In favourable years plants are carried north and grow until a succession of severe winters again destroys them, and their remains might also lead, in the same way, to the incorrect conclusion that there had been a change in the climate of the region. Similarly with human settlements. The presence of traces of these latter in a given place show, not that the climate has become more severe, but that the place lies in that debatable land between districts favourable and unfavourable to the existence of man. Before any really satisfactory conclusion can be reached, however, he thinks we must have a thorough examination of the migration of the Eskimo ; before it is possible to account for the presence of traces of the people in the far north on coasts where they do not now live, we must recollect how their wanderings depend on the physical conditions of life, on the nature of the ground, of the hunting, and the influence of the neighbouring tribes. But on all these points we lack material for a complete explanation of the facts. With respect to the comparatively great age claimed for some of these remains which have been brought by Arctic travellers to Europe, Dr. Boas suggests that all estimates as to the age of objects such as these coming from the Arctic regions must be taken with great care, owing to the different effects of the climate. He instances the remains of Parry's camp at Point Nias in Hecla Bay, which were found looking quite fresh in 1854, more than thirty years after Parry's expedition ; while the cairn erected at the same time (1820) on Cape Providence was covered with lichen and moss, and looked quite ancient in 1854.

WE have received the Administration Report of the Meteorological Reporter to the Government of the North-West Provinces and Oudh for the years 1882-83. At the beginning of the present year the observatories reporting to the Allahabad Office were twenty in number, and great activity seems to have been displayed in all of them. The question of the construction of a first class observatory for these provinces has advanced during the present year, but only very slightly. It will in all probability be built at Allahabad. In addition to the ordinary observations, special observations of soil temperatures have been carried on at Allahabad and Jeypore. At Jeypore, where the observatory has practically become one of the first class, all records being made automatically, a sixth soil thermometer has been added to the five which the observatory already possesses to record the temperature at a depth of twenty feet. It is evident from the report that Mr. S. A. Hill, the meteorological reporter, is doing his level best with the means at his command. Unfortunately, however, the native observers still make mistakes, and some of the monthly means require a considerable amount of overhauling.

DR. HENRY MACAULAY, M.D., of Belfast, has recently made a suggestion which, if followed in tropical countries, will turn the tables on the sun with a vengeance. He suggests that Mouchot's sun-engine should be used to pump cold air into dwellings, factories, &c., pointing out that the temperature can in this way be reduced from 100° or more to 60°. He points out that not only will this reduce the temperature especially at night, thus rendering sleep possible, but fresh air will be guaranteed during the day, and the plague of flies and insects would be excluded. The weak point about this arrangement is that it requires ice. We think, however, that sooner or later in America, where the heat in summer is more distressing than in any other part of the world, and ice is everywhere, this arrangement, or one like it, is certain to be adopted.

THE last number of the *Proceedings of the Royal Society of Tasmania* contains several papers on the botany and zoology of Tasmania. In a presidential address the Governor, Sir J. Lefroy, remarks on the omission of any reference to the Botanic Gardens of Hobart Town by Prof. Thiselton Dyer, in a review of the botanical enterprise of the Empire, and demands more public support for these gardens. He notices also a fact which will be of some interest in England just now, viz. that of over ten thousand visitors to the Museum in six months more than half were Sunday visitors. Among the chief papers are :—Notes on a species of Eucalyptus (*E. hæmastoma*), by Mr. Stephens ; type species of Tasmanian shells, by Prof. Tate ; the magnetic variation of Hobart, by Sir J. Lefroy ; notes on *Leontopodium catipes*, by Baron von Müller, &c. With respect to the Sunday opening of the Museum, the Council of the Society report that it is open only between the hours of half-past two and five, "and this arrangement, as will be seen by the number availing themselves of the opportunity, may be pronounced to be no longer an experiment, and to be fully justified by the quiet and orderly demeanour of the visitors."

THE voyage round the world of the Swedish frigate *Vanadis*, which we recently announced, will be shared by the Duke of Gotland, King Oscar's youngest son. The journey, which will be of about eighteen months' duration, will chiefly be a scientific one, several eminent Swedish *savants* participating in the same. From the Straits of Magellan the ship will proceed to the Sandwich Islands, Japan, China, India, and thence home.

THE steamers *Obe* and *Nordenskjöld* left Tromsö for Novaya Zemlya on the 3rd inst. Norwegian fishermen report that the state of the ice in the Arctic Sea east of the North Cape is very favourable this spring.

M. PASTEUR has been appointed head of the Sanitary Commission formed in Paris in view of the dreaded visitation of cholera.

A FRENCH scientific periodical puts forward the idea of a joint occupation of Mecca by the several European powers for the purpose of stopping pilgrimages thither and thereby preventing the further dissemination of cholera through the crowding of people in so pestilential a city, especially when the Ramadan falls in summer.

WE are asked to say that possessors of the eighth edition of Prof. Babington's "Manual of British Botany" may, by application to Mr. Van Voorst, 1, Paternoster Row, obtain gratis two pages of additions and corrections which have been prepared by the author.

LOCUSTS are reported from the south of Russia, but the very energetic measures taken by the Governors for the destruction of the eggs and larvæ will, it is believed, arrest their ravages.

THE additions to the Zoological Society's Gardens during the past week include a Tennant's Squirrel (*Sciurus tennanti*) from Ceylon, presented by Mr. A. Ross ; two Rufous Tinamous (*Rhynchotus rufescens*), three Spotted Tinamous (*Nothura maculosa*) from the Argentine Republic, presented by Mr. E. M. Longworthy ; two Common Buzzards (*Buteo vulgaris*), British, presented by Mr. James S. Cookson ; a Land Rail (*Crex pratensis*), British, presented by Mr. J. W. Merison ; a Jackdaw (*Corvus monedula*), British, presented by Mr. J. Baldwin ; two Cockateels (*Calopsitta novæ-hollandiæ*) from Australia, presented by Mrs. Day ; three Angulated Tortoises (*Testudo angulata*), a Geometric Tortoise (*Testudo geometrica*), an Areolated Tortoise (*Testudo areolatus*), a Robben Island Snake (*Coronella phocarum*), a Laland's Ground Snake (*Typhlops lalandii*) from South Africa, presented by the Rev. G. H. R. Fisk, C.M.Z.S. ; a Margined

Land Tortoise (*Testudo marginata*), South European, presented by Lord Arthur Russell, M.P.; an Indian Badger (*Arctonyx collaris*) from Assam, a Rough-billed Pelican (*Pelecanus trachyrhynchus*) from Mexico, purchased; two Red-crested Whistling Ducks (*Fuligula rufina*), a Variegated Sheldrake (*Tadorna variegata*), five Summer Ducks (*Aix sponsa*), five Chilian Pintails (*Dafila spinicauda*), bred in the Gardens.

OUR ASTRONOMICAL COLUMN

THE CONSTANT OF ABERRATION.—M. Otto Struve presented to the Imperial Academy of Sciences of St. Petersburg, in February last, a memoir by M. Nyren, of the Observatory at Pulkowa, on the aberration of the fixed stars. He states it is the result of researches made by M. Nyren during many years, with the view to determine the value of the constant of aberration, with the highest degree of accuracy which the most perfect means of observation allow. The value 20″·445, deduced by W. Struve, has been so far generally accepted by astronomers as the most exact, and has been employed in all astronomical calculations. This is the value given in his memoir upon the subject, but in 1852, by a new combination of his measures, the constant was altered to 20″·463, and with respect to this value he remarked: "Elle me paraît le vrai résultat pour l'aberration, qui doit être tiré de mes observations du premier vertical." (Preface to "Recueil de Mémoires présentés à l'Académie des Sciences par les Astronomes de Poulkova," t. i.) Notwithstanding this statement, Struve's first value was retained in our ephemerides, &c.; we have a suspicion that his correction, as he appears to have considered it, was very generally overlooked. M. Nyren was charged with the execution of a new series of observations at Pulkowa, with the same instrument employed by the elder Struve, and every endeavour was made to free the new series from all objection that it was possible to bring against the earlier one. Further, M. Nyren discussed a long series of excellent observations made by M. Wagner with the great meridian telescope in the years 1861–72, on the three stars, Polaris, δ Ursæ Minoris, and 51 (Hev.) Cephei. M. O. Struve remarks that with these two new determinations we now possess seven separate series of observations executed with the three great instruments of the Observatory of Pulkowa, and he gives the values of the constant of aberration resulting therefrom as follow:—

W. Struve, prime-vertical instrument ...	20″·463 ± 0″·017
Schweizer, meridian telescope	20·498 ± 0·012
Peters, vertical circle	20·507 ± 0·021
Gyldén, ,, ...	20·469 ± 0·026
Wagner, meridian telescope	20·483 ± 0·012
Nyren, vertical circle	20·495 ± 0·021
Nyren, prime-vertical instrument	20·517 ± 0·014

M. O. Struve considers that these values sufficiently prove that the constant of aberration is now known with a degree of accuracy which it will be difficult to surpass; it appears certain that the mean of the seven determinations deduced by M. Nyren, 20″·492, will not be liable to an error of a hundredth of a second.

If this mean value for the constant of aberration is combined with the velocity of light determined by M. Cornu and Mr. Michelson, the solar parallax is found to be 8″·784, which, M. Struve adds, only differs by a very few hundredths of a second from the most reliable determinations lately obtained by the geometrical process.

With regard to W. Struve's alteration of the constant of aberration assigned in his memoir, it may be remarked that his result depended upon observations made with the prime-vertical instrument upon seven stars, and the separate values accorded well. But, as he subsequently pointed out, this agreement of different determinations, obtained with the same instrument, only guaranteed the accuracy of the final result under the condition that there existed no common source of error. He examined all possible sources of constant error, and convinced himself that none existed which could exercise an appreciable influence. Nevertheless he said it must be admitted that there existed an agent which possibly might prejudice the exactness of his determination. Considering that the observations of the maximum of aberration fall at a time of year when the star passes the meridian near 6 p.m., while the observations of the minimum of the aberration take place at 6 a.m., it is seen that the first are made during a decreasing temperature and the last during an increasing one. "The zenith-distance of the star being determined from the time between the two corresponding transits indicated by the clock, it follows, if the clock has a defect of compensation and if its effective rate during the interval differs from the mean daily rate obtained by observations of consecutive days, that the error produced acts in the same sense upon the results obtained by different stars." It is the same if between the two corresponding passages the azimuth of the axis of rotation changes. Fortunately these two perturbing causes only exercise a minute influence upon the zenith distances to be determined. Yet, as Struve asks: "Comment prouver que cette influence n'ait point altéré la valeur trouvée de l'aberration de quelques centièmes de seconde?" He considered he had direct proof that there was no azimuthal change, but with regard to change of clock rate, as already stated, he was induced to rediscuss his series of observations with the result above given.

ON THE FUNCTION OF THE SOUND-POST, AND ON THE PROPORTIONAL THICKNESS OF THE STRINGS OF THE VIOLIN[1]

SIR JOHN HERSCHEL says: "It (the bridge) sets the wood of the upper face in a state of regular vibration, and this is communicated to the back through a peg set up in the middle of the fiddle and through its sides, called the 'soul' of the fiddle, or its sounding-post."[2]

Savart says: "L'âme a pour usage de transmettre au fond les vibrations de la table . . . son diamètre est déterminé par la qualité du son qu'on veut avoir; il est maigre quand elle est trop mince, et sourd quand elle est trop grosse."[3]

Daguin, in his "Traité de Physique," devotes a whole page to the discussion of the functions of the sound-post. The most important sentences are the following:—" . . . l'âme n'agit pas comme conducteur du son. . . Il nous semble que l'on doit expliquer l'effet de l'âme de la manière qui suit. L'âme, ou les pressions extérieures par lesquelles on la remplace, a pour effet de donner au pied du chevalet un point d'appui autour duquel il vibre en battant sur la table de son autre pied. Si l'un des pieds n'était appuyé sur un point fixe, il se releverait pendant que l'autre s'abaisserait, parceque les cordes n'agissent pas normalement à la table, puisque l'archet les ébranle très obliquement, ce qui entraîne le chevalet dans un mouvement transversal quand il n'a pas de point d'appui fixe. Lorsque l'archet est dirigé normalement aux tables, cet inconvénient n'existe plus, et l'âme n'est plus nécessaire."[4]

Helmholtz says: "The vibrating strings of the violin, in the first place, agitate the bridge over which they are stretched. This stands on two feet over the most mobile part of the belly between the two '*f*' holes. One foot of the bridge rests upon a comparatively firm support, namely, the sound-post, which is a solid rod inserted between the two plates, back and belly, of the instrument. It is only the other leg which agitates the elastic wooden plates, and through them the included mass of air.[5]

The experiments[6] which follow have been made for the purpose of ascertaining whether it be any part of the function of the sound-post to convey vibrations to the back, or whether this post acts solely as a prop supporting the belly, so that its elasticity is not injured by the pressure from the strings, and also, as Daguin states, affords the firm basis which he considers necessary for one foot of the bridge.

Mr. Hill and other practical men maintain that the quality of the wood of which the sound-post is made affects the tone of the violin, as undoubtedly do very minute differences of position. If the quality of the wood is important, we must admit that vibrations are conveyed by the post.

Whether or not the sound-post exercises the function of transmitting vibrations, it is obvious (1) that it performs the important duty of contributing to the support of the belly; (2) that the nodal arrangement of the belly and also that of the back are

[1] Paper read at the Royal Society, May 24, by William Huggins, D.C.L., LL.D., F.R.S.

[2] "Encyclopædia Metropolitana," Article "Sound," p. 804.

[3] "Mémoire sur la Construction des Instruments à Cordes et à Archet," 8vo, Paris, 1819. Also Biot's "Report," *Ann. de Chimie*, tome 12, pp. 225-255.

[4] "Traité de Physique, Acoustique," tome 1, p. 575.

[5] "Sensations of Tone," translated by Ellis, p. 137. In the 4th German edition this passage remains unaltered.

[6] I wish to express my indebtedness to Mr. A. J. Ellis for some suggestions in connection with these experiments.

influenced by the pressure of the ends of the post against the upper and lower plates; (3) that Helmholtz is right, at least so far that the leg of the bridge under the fourth or G string has much more power than the other in setting the belly into vibration.

The usual way of investigating vibrations by the scattering of sand over the surface of the agitated body is difficult of application to the violin, on account of the curved form of the upper and lower plates. I found a convenient method to be by the use of what I may call a touch-rod. It consists of a small round stick of straight-grained deal a few inches long; the forefinger is placed on one end, and the other end is put lightly in contact with the vibrating surface. The finger soons becomes very sensitive to small differences of agitation transmitted by the rod.

The experiments were made on a strongly made modern violin, and in some cases repeated on a fine violin by Stradiuarius in the possession of the writer.

The sand method, and also the touch-rod, showed that the position of maximum vibration of the belly is close to the foot of the bridge under the fourth string. The place of least vibration is exactly over the top of the sound-post behind the other foot of the bridge. The back is strongly agitated, the vibrations being least powerfully felt where the sound-post rests, which is at nearly the thickest part of the back. These effects were very satisfactorily observed on a violoncello, where the phenomena are on a larger scale.

When the sound-post was removed from the violin the large difference of the amount of vibration on the two sides of the belly was no longer present, the belly was about equally strongly agitated on both sides, making allowance for the string which was bowed. The tone became very poor and thin, as is well known to be the case when the sound-post is removed. The vibration of the back was now very feeble as compared with its vibration when the sound-post was present, a circumstance in favour of the view that the sound-post conveys vibrations to the back.

A clamp of wood was prepared which could be so placed on the violin as to connect by an arch of wood outside the violin the place of the belly behind the bridge where the top of the sound-post presses with the place of the back where it rests. It was expected that the wooden arch would restore to some extent the connection of belly and back which was broken by the removal of the post, and carry, though imperfectly, vibrations from the upper plate to the back.

When this clamp was put on, the poor and thin sound was altered to the fuller character of tone which belongs to the violin when the sound-post is in its place. On testing the condition of the back its normal state of vibration was found to be in a large degree restored. If, while the strings were being bowed, the clamp was suddenly removed, the tone at the same moment fell to its poor character, and the vibration of the back as instantly diminished.

It was further observed that, if the upper part of the clamp pressed upon the belly without the lower part coming into contact with the back, the tone is altered in the direction as when the sound-post was present, but it was not until the lower part of the clamp was in contact with the back that the normal character of the tone was fully restored. A similar effect to that resulting from the pressing of one end of the clamp only was produced by firmly placing one end of a wooden rod at this part of the belly. This effect may be due to the setting-up in the belly, by pressure at this part, of the peculiar nodal arrangement which the post produces when in its place.[1]

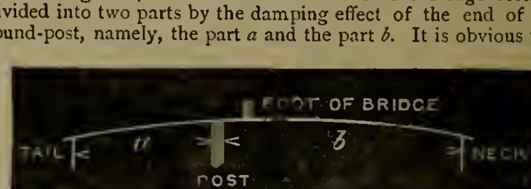
There could be no doubt that vibrations were carried by the clamp, for the lower end was powerfully agitated when the upper end rested upon the belly. If the sole function of the sound-post is to serve as a firm prop for the foot of the bridge, it should fulfil this condition most fully when placed under the foot of the bridge. In this position of the sound-post, however, as is well known, the tone is much injured.

In order to separate that part of the function of the sound-post which serves as a support from the further function it may possess as a transmitter of vibrations, it was desirable to introduce such alterations in the structure of the sound-post as would enable it to retain its supporting power, and yet greatly modify and, if possible, stop its power of transmitting vibrations. A sound-post was made in which about half an inch of the middle was cut out, and a piece of lead inserted, also a sound-post in which instead of lead sealing-wax was put in. The effect of these compound posts which retained uninjured their prop power was to modify greatly the quality of the tone, but not to diminish its quantity in any marked degree, a result in favour of the view that the character or the wood of which the post is made does influence the tone, and that vibration is transmitted by the post. As these compound posts could transmit vibrations freely, it was desirable to contrive a post which would not carry vibrations and yet form a firm prop. A post was made with a piece of hard indiarubber inserted in the middle, but this post was found by experiment with a tuning-fork to transmit vibrations to some extent. Other materials were tried without success. A post capped at each end with pieces of sheet vulcanised rubber stopped almost completely the sound of a tuning-fork when the foot of the fork rested on the rubber over one end of the post, while the other end equally protected with rubber rested on a body capable of reinforcing the sound of the fork. This rubber-capped post was firmly fixed in position in the violin, so that it would be able to support fairly well the belly and foot of the bridge, and yet not be able to carry vibration; unfortunately it does not seem possible, from the nature of things, to have a *rigid* prop which does not transmit vibrations, but this post, with thin sheet rubber at the ends firmly forced into position, must have been fairly efficient in its supporting power. The effect on the tone was about the same as when the sound-post was removed. When the wooden clamp was put on, then the normal tone returned, and the back vibrated strongly.

These experiments appear to show that the sound-post is more than a prop, and that, besides its other functions, it does transmit vibrations to the back in addition to those which are conveyed through the sides.

Experiments with sand and the touch-rod appear to me to show that Helmholtz's statement is too absolute when he says "it is only the other leg of the bridge which agitates the elastic wooden plates." Undoubtedly it is the fourth string foot of the bridge which is the more powerful in agitating the upper plate, but the other foot appears to me also to have an influence. When the post is placed exactly under the foot of the bridge, then the belly on this side is almost without vibration; if the post is absent, then this foot appears to agitate its own side of the belly as strongly as the other foot. As there is no post on the fourth string side of the fiddle, that foot stands in a position most favourable for setting up vibrations in the belly, being nearly half way between the supports of the belly at the tail and the

neck end of the violin. The other side of the belly, on the first or E string side, where the other foot of the bridge rests, is divided into two parts by the damping effect of the end of the sound-post, namely, the part *a* and the part *b*. It is obvious that



this foot of the bridge is unfavourably placed for setting the part of the belly, *b*, into vibration, since it is so far from its central mobile part. On the other hand, its position is favourable for a portion of its energy of vibration to be transmitted through the post to the back.

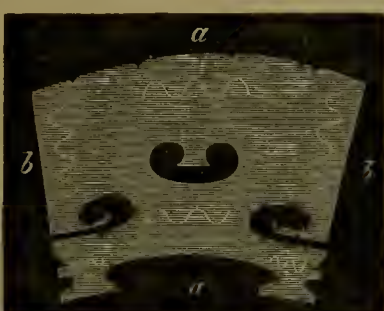
Practically very small differences of position of the top of the post behind the foot of the bridge are found to alter largely the character of the tone of the fiddle, and in the case of fine instruments the setting of the post is an operation demanding much care and judgment. The explanation lies probably in the circumstance that a small difference in the position of the post will

[1] According to Daguin some similar experiments were made by Savart, but I have failed to find them in those of his papers to which I have had access. "On peut la (l'âme) mettre en dehors, en l'appuyant à une espèce d'arcade dont on colle les pieds de chaque côté du violin. . . . On peut la remplacer par la pression d'un poids convenable appuyé sur la table supérieure." "Savart a conclu de là que l'âme a pour effet de rendre normales les vibrations de la table. . . ."—"Traité de Physique," tome i. p. 575.

alter greatly the proportion of energy passing through the post to that which is absorbed into vibrations of this side of the belly. At the same time it must also alter slightly the nodal arrangement of the belly, which must have an influence on the tone. If from the form of construction, or relative quality of the wood of the upper plate as compared with the under plate, the conditions of a violin are such that the highest quality of tone of which it is capable requires a relatively larger amplitude of vibration of the back, the position of the sound-post should be nearer the bridge. In a contrary condition of things the sound-post should be farther from the bridge. The extreme range needed in different violins is about a quarter of an inch. Any shift of the post must affect the relative mobility of the two sides of the belly.

If the sound-post transmits vibrations, these will be in addition to those received from the sides of the violin. It may be, therefore, that one condition which determines the best position of the post is the degree in which from their form and material these fulfil this duty. All the sides must share in this duty, but the touch-rod shows that a large part of this action is borne by the parts of the sides which curve inwards under where the strings are bowed. It is in harmony with this view that Mr. Hill states that if the inside blocks at the corners, which are put to strengthen these parts, extend in a small degree into these curved portions, the tone is injured.

The plane of the vibrations of the strings is that in which they are bowed, which is more or less oblique to the bridge. The vibrations may be considered divided into two sets at right angles to each other, *a* and *b*.



The touch-rod shows that these vibrations exist strongly in the upper part of the bridge. I venture to suggest that the use of the peculiar cutting of the bridge, which was finally fixed from trials, by Stradiuarius, is to sift the vibrations communicated by the strings and to allow those only or mainly to pass to the feet which would be efficient in setting the body of the instrument into vibration, the other vibrations which would be injurious in tending to give a transverse rocking motion to the bridge, being for the most part absorbed by the greater elasticity given to the upper part of the bridge by the cutting. Below the two large lateral cuts, the touch-rod shows a very great falling off of the vibrations *b*. In the case of a violoncello these vibrations were also very greatly reduced below the side openings of the bridge.

The violin on which the experiments were made was without a bass bar, which is a piece of pine glued to the under side of the belly on the fourth string side. This bar is regarded as strengthening the belly and also enabling it to respond better to the lower notes. The touch-rod showed no difference in the general behaviour of this violin from a fine one by Stradiuarius containing a bass bar.[1]

On the Proportional Thickness of the Strings.—As the lengths of the strings are the same, we have only the two conditions of weight and tension on which their pitch depends. It is obvious that for equal pressure on the feet of the bridge, as well as for more convenient fingering and bowing, the strings should be at the same tension. They should therefore differ in weight, so as to give fifths when brought to the same tension. The weights of the strings are inversely as the squares of the number of vibrations, which in the case of fifths is as 3 to 2, namely, as 9 to 4. As the first three strings are of the same material, it is more convenient to take their diameters, which must be as 3 to 2, that is, each string in advancing from the first string must be half as thick again as the string next to it. In the case of the fourth string, covered with wire, we must find the weight of the third string of gut, and take a fourth string of which the weight is 9 to 4 for the third string.

A good average thickness of 2nd (A) string = 0·0355 inch.

Then the strings should be— 1st = 0·0237 ,,
2nd = 0·0355 ,,
3rd = 0·0532 ,,

A gut string 0·0532 inch in diameter weighs, when of the same length as a fourth string, 0·98 grm., then the fourth = 2·20 grms.

Ruffini sells sets of strings in sealed boxes, and these were found to be in about the same relative proportion to each other as the sizes indicated on the gauges sold by several makers.

The measures of a set of Ruffini's strings were found to be:—

1st = 0·0265 inch.
2nd = 0·0355 ,,
3rd = 0·0460 ,,
4th = 1·4100 grm.

It will be seen that the first string is thicker, and the third thinner, and the fourth much lighter than the theoretical values. Therefore the tension of the first string would be greater, and that of the third and fourth strings less than they should be in relation to that of the second string. The greater flexural rigidity of the fourth string will have a small effect in the direction of making the vibrations quicker, and therefore of making the tension required less.

By means of a mechanical contrivance I found the weights necessary to deflect the strings to the same amount when the violin was in tune. The results agreed with the tensions which the sizes of the strings showed they would require to give fifths.

A violin strung with strings of the theoretical size was very unsatisfactory in tone.

The explanation of this departure of the sizes of the strings which long experience has shown to be practically most suitable, from the values they should have from theory, lies probably in the circumstance that the height of the bridge is different for the different strings. It is obvious, where the bridge is high, there is a greater downward pressure. By this modification of the sizes of the strings there is not the greater pressure on the fourth string side of the bridge which would otherwise be the case. On the contrary, the pressure is less, which may assist the setting of the belly into vibration. There is also the circumstance that the strings which go over a high part of the bridge stand farther from the finger-board, and have therefore to be pressed through a greater distance, which would require more force than is required for the other strings, if the tension were not less.

[1] In the "Early History of the Violin Family," Engel, speaking of the Crwth, says:—"Furthermore, the contrivance of placing one foot of the bridge through the sound-hole, in order to cause the pressure of the strings to be resisted by the back of the instrument, instead of by the belly, is not so extraordinary and peculiar to the Crwth as most writers on Welsh music maintain. It may be seen on certain Oriental instruments of the fiddle kind which are not provided with a sound-post. For instance, the bridge is thus placed on the three-stringed fiddle of the modern Greek, which is only a variety of the ordinary rabâh, but which the Greeks call lyra. Inappropriate as the latter designation may appear, it is suggestive, inasmuch [as it points to the ancient lyra as the progenitor of the fiddle."—P. 28.

UNIVERSITY AND EDUCATIONAL INTELLIGENCE

CAMBRIDGE.—The next examination for Minor Scholarships and Open Exhibitions at St. John's College will take place in December, 1883. There will be open for competition, besides certain Exhibitions, two Minor Scholarships of 50*l*. per annum and two of 75*l*.; also such Foundation Scholarships as shall be vacant, two of which may, after the commencement of residence, be increased in value to 100*l*. each.

Candidates may offer themselves for examination in any of the following subjects:—Classics, Mathematics, Natural Science, Hebrew, or Sanskrit.

The Examinations will begin on Tuesday, December 11.

Successful candidates will be required to commence residence not later than October, 1884. Further particulars of the Scholarships and Exhibitions may be obtained in October, 1883, on application to one of the tutors.

SCIENTIFIC SERIALS

Bulletins de la Société d'Anthropologie de Paris, tom. v. fasc. iv. 1882.—Discussion on M. Ball's case of cretinism, in which the axiom advanced by M. Lunier was generally accepted, that, while idiocy

is hereditary and congenital, cretinism is endemic.—M. Gustave Le Bon, in defending the accuracy of his determinations of the comparative weight of the brain of boys and girls against the charges advanced by MM. Budin and Manouvrier, explains his methods of determination, which, in his opinion, confirm the conclusions contained in his earliest memoir on the subject: viz. that (1) while male and female children differ very little in weight at their birth, when, if the weight of boys be taken at 100, that of girls will be 94·28, the difference between the sexes in adult life may be at least three times greater; (2) that at the same age, with equal stature and weight, the female brain will be found notably inferior in weight to that of the man.—On the cranial dimensions of the savage Stiengs, or Moïs, of Cochin China, by Dr. P. Neis, who finds that this people exhibits the low mean cranial capacity of 1400, with a cephalic index of only about 75.—M. Capitan records the results of his experiments on the methods of trepanning employed in prehistoric times. He has experimented both on the living and dead subject, using a flint instrument, with which he reproduced perforations similar to those observed in prehistoric crania. This was effected by boring and incision, as well as by scraping, and in both cases the animals operated on recovered rapidly and completely from the operation, although Broca had maintained that the removal of any part of the cranial surface could not possibly have been effected on the living subject by such instruments as were used by primitive man.—Dr. Collignon describes the nature of the human remains found at Cumières, Meuse, belonging to the Neolithic age, among which are seven well preserved skulls, and various long bones, including two platycnemic tibiæ.—Dr. Heurot's report of the ossuary of the polished stone period, discovered in 1881 at Liry, in the Ardennes, was laid before the Society by M. Mortillet, who drew attention to the extraordinary projection of the lower jaw observable in one of the crania, which in this particular seems to foreshadow the present and future evolutionary change, rather than to accord with the ordinary type of the receding anthropoidal chin of the prehistoric ages. In the course of the discussion arising out of Dr. Heurot's communication, M. Legnay described similar burial places examined by himself at Le Grand Compant, near Luzarches, and at Vaureal, Pontoise, where, as at Liry, a passage composed of upright stones, and covered in with wood, gave admittance to the true sepulchral chambers.—M. Topinard reports on his examination of *Le Questionnaire de Sociologie et d'Ethnographie*, issued by the commission appointed by the Society for its elaboration; and while he approves generally of the plan followed, which is that suggested by M. Letourneau, he has drawn attention to numerous points omitted by the latter, who, by his mode of defending the proceedings of the Society, and attacking M. Dally, gave an aggressively personal character to the discussion, very unusual in meetings of the Society. Owing to want of unanimity among the members, the method to be followed for the French system of instructions for travellers still remains undecided.—The Galibis of Cayenne, who have long been established in the Jardin d'Acclimatation of Paris have been made the subject of an exhaustive study by M. Manouvrier, whose detailed communications of the numerous observations and determinations in regard to the sociology, language, and ethnology of these tribes led to a somewhat lengthy discussion on the *rationale* and early extension of the practice of the *couvade*, which has been observed among the Galibis of French Guiana, as well as among the Basques, and appears to have prevailed under various modifications among several ancient peoples.

Rendiconti of the *Reale Istituto Lombardo*, May 31.—A comparative study of the fauna of the various Pliocene deposits in Lombardy, by Dr. C. F. Parona. As many as 275 species were examined, 248 in the Pliocene of the Northern Apennines, and 187 in the Upper Miocene, of which 117 still survive in the neighbouring seas.—On Paff's method of integration of partial differential equations of the first order, by Prof. E. Beltrami.—A contribution to the history of the adulteration of food from the earliest times, by C. L. Gabba.—On the mortality of infants during the first and second years of their lives in the various provinces of Italy, by Prof. G. Sormani. For the decade ending 1880 the average rate of mortality in the first year throughout Italy was 214·9 per 1000, and in the second 114·6 per 1000. Compared with the rest of Europe, these figures show that Italy occupies the lowest position in the scale, the death-rate being in excess even of Croatia and Slavonia (107·4) and of Russia (102·7). In the general comparative table, Ireland stands first (34·5), England occupying a medium position with an average of 59·1 per 1000.—The career of David Lazzeretti, founder of the new sect of Lazzerettists, studied in the light of documents recently discovered, by G. Barzellotti.—The telephone in its legal aspect (continued), by C. Norsa.

Bulletin de la Société d'Encouragement pour l'Industrie Nationale, June, 1883.—Report on M. Lavanchy-Clarke's workshops for the blind, by M. Legentil.—Report on M. Latry's tinted papers, by M. Ern. Dumas.—Colouring elements of madder and their metamorphoses, by M. A. Rosenstiehl.—On the saline tracts in the south-east of France, by M. P. de Gasparin.—On sewing-machines and sewing-machine industries of all sorts, as shown at the Paris Universal Exhibition of 1878, by M. Emile Bariquand.

THE number for June 15 of the *Archives des Sciences Physiques et Naturelles* contains researches on the absorption of ultra-violet rays by different substances (seven plates), by M. J. L. Soret (fourth memoir).—A new contribution concerning the family of Tintinodea, by Dr. Herman Fol (one plate).—On the magnifying power and strength of dioptric arrangements, by Dr. Adrien Guébhard.—Meteorological observations at the Geneva Observatory for the month of May.

SOCIETIES AND ACADEMIES

LONDON

Geological Society, June 6.—Mr. J. W. Hulke, F.R.S., president, in the chair.—George Paul was elected a Fellow of the Society.—The following communications were read:—The estuaries of the Severn and its tributaries, an inquiry into the nature and origin of their tidal sediment and alluvial flats, by Prof. W. J. Sollas, M.A., F.R.S.E., F.G.S.—Notes on a collection of fossils and rock-specimens from West Australia, north of the Gascoyne River, by W. H. Hudleston, M.A., F.G.S.—Notes on the geology of the Troad, by J. S. Diller. Communicated by W. Topley, F.G.S. This paper gave a brief account of the results obtained by the author whilst attached to the United States Assos Expedition. Together with a geological map (scale 1 : 100,000) this was sent to Mr. Topley for the service of the new geological map of Europe (and its borders), which is now being prepared by a Committee of the International Geological Congress. The country described is that lying south and west of the River Menderé (Scamander). The sedimentary rocks may be divided into three great groups:—1. An old, possibly Archæan, highly crystalline series, forming the mountainous lands of the Ida range (5750 feet), but also appearing in smaller detached areas to the west and north-west. Probably these have existed as islands from early times, and around these the later rocks have accumulated. Mount Ida itself is almost a dome, the lowest rocks (talc schists) occupying the summit. On the northern slopes there is true gneiss. No igneous rocks enter into the structure of this mountain. At different horizons there are bands of coarsely crystalline limestone, and as far as can be seen this series is conformable throughout. 2. Resting on these old rocks and in part made up of their remains is a series of partially crystalline rocks, chiefly limestone. It is probable that this series is in large part of Cretaceous age; but it contains rocks which are older, possibly Palæozoic. Eocene fossils have lately been discovered by Mr. Frank Calvert, which also may have come from this series. The rocks in the south of the Troad, hitherto supposed to be Lower Tertiary, are now known to be of later date. Sharply marked off from these older rocks are the Upper Tertiaries; these are of two ages, occurring in two distinct areas. 3. The *Upper Miocene*, which fringes the western shores of the Troad, and forms a broader band at the north-west corner in the lower course of the Menderé. Hissarlik is built on this. These beds are marine, and belong to the *Sarmatian Stage*. The Troad is the most south-westerly point at which the *Mactra kalk* is yet known. 4. Freshwater beds, which occur in force in the interior of the country, between the Menderé and the south coast, and in patches near the coast. These are *Upper Miocene* or *Lowest Pliocene*. Later than these are the *Pliocene beds* of the great plain of Edsemet. The igneous rocks are of various ages, but most are of Tertiary date. The oldest is a *granite* which intrudes through and alters the oldest (?Archæan) crystalline rocks. This is invaded by dykes of *Quartz-porphyry*. *Quartz-diorite* invades and alters the group of partially crystalline rocks. The oldest rocks in the newer series are the *Andesites* and *Liparites*. These, in part, are older than the Sarmatian stage, as the conglomerate at its base contains fragments of these rocks. But they are also in part of later date. Where they can be studied together the Liparite is the later of the two, as it flows through and carries up fragments

of the Andesite. The Andesite (unlike the Liparite) seems to have reached the surface, in some cases, through volcanic vents. *Basalts* and *Nepheline-basalts* are of late Tertiary date; possibly they are the latest volcanic rocks of the district, but their relation to the other eruptive rocks of the Troad cannot be definitely determined. The volcanic rocks in the isolated area between Alimadja and Lyalar are interesting because their relative ages are here well seen. The earliest was melaphyre; this was followed by mica-andesite, horneblende-andesite, augite-andesite, basalt, and late (if not last) by liparite. Mr. Topley, who in the absence of the author read the paper, explained the objects of the Assos Expedition and the geological results obtained by Mr. Diller. He gave a short account of previous literature, and mentioned some of the main points in which our knowledge of the Troad is now advanced. Mr. Topley briefly described the physical geography and general structure of the country, illustrating this by means of a section which he had prepared from Mr. Diller's map and paper.

Zoological Society, June 19.—Prof. Flower, F.R.S., president, in the chair.—The Secretary read an extract from a letter received from Mr. Albert A. C. Le Souëf, containing observations on the colouration of the plumage of the Satin Bower-bird (*Ptilonorhynchus holosericeus*).—Prof E. Ray Lankester, F.R.S., read a memoir on the muscular and endoskeletal systems of *Limulus* and *Scorpio*, drawn up by himself with the assistance of his two pupils, Mr. W. J. Barham and Miss E. M. Beck These investigations seemed to confirm Prof. Lankester's previously expressed views as to the near affinity of these two forms, hitherto usually referred to different classes of the animal kingdom, and to justify the association of *Limulus* with the Arachnida.—A paper was read by Dr. Gwyn Jeffreys, F.R.S., F.Z.S., on the Mollusca procured during the cruise of H.M.S. *Triton* between the Hebrides and Faroes in 1882. Ten new species of Gastropoda were described, and another species (*Fusus sabini*) was fully diagnosed. The chief interest of the paper consisted in the distinction of the Mollusca inhabiting the "warm" and "cold" areas of that sea-bed, in accordance with the views of Dr. Carpenter and the late Sir Wyville Thomson.—A communication was read from Mr. Martin Jacobi, containing descriptions of some new species of Beetles belonging to the family Galerucidæ.—Prof. P. Martin Duncan, F.R.S., read a paper on the Madreporarian genus *Phymastræa* of Milne-Edwards and Jules Haime, and gave the description of a new species obtained on the west coast of India, which he proposed to call *Phymastræa irregularis*.—Dr. J. S. Garson, F.Z.S., read a paper on the anatomy of the Pygmy Hog of Nepaul (*Porcula salvania* of Hodgson), as exhibited in a female specimen of this animal which had lately died in the Society's Gardens. Dr. Garson came to the conclusion that this animal was not sufficiently different from the true Pigs (*Sus*) to warrant its generic separation.—A communication was read from Mr. Osbert Salvin, F.R.S., containing an account of a series of birds collected by Capt. A. H. Markham, R.N., at various points of the western shores of the Pacific, from Esquimault on the north, to the Straits of Magellan on the south, including some from the Galapagos Islands and from the island of Juan Fernandez.—Mr. E. W. White, F.Z.S., read some notes on the birds of the Argentine Republic, being a supplement to two former papers read before the Society on the same subject.—A communication was read from Mr. A. Boucard, C.M.Z.S., containing an account of a collection of birds made in Yucatan by Mr. Gaumer.

SYDNEY

Royal Society of New South Wales, May 2.—Annual meeting.—The number of new members elected during the year was forty-one, making the total number of ordinary members upon the roll to date 486. At the Council meeting held on December 13 it was unanimously resolved to award the Clarke Memorial Medal for the year 1883 to Baron Ferdinand von Müller, K.C.M.G., F.R.S., Government Botanist, Melbourne; and at the same meeting the Council awarded the prize of 25*l*., which had been offered for the best communication on the "Influence of Australian Climates and Pastures upon the Growth of Wool," to Dr. Ross, M.L.A., Molong; and the prize for the one upon "The Aborigines of New South Wales" to Mr. John Fraser, B.A., West Maitland. During the year the Society held ten meetings, at which the following papers were read:—Annual address by H. C. Russell, F.R.A.S.—On the geology of the Hawkesbury sandstone, by Rev. J. E. Tenison-Woods, F.G.S.—On tropical rains, by H. C. Russell, F.R.A.S.—On the orbit of the late comet, by G. Butterfield.—On a method of determining the true south, by J. S. Chard.—Notes on the progress of New South Wales during the years 1872 to 1881, by Christopher Rolleston, C.M.G.—On some marine fossils of the coal-formation of New South Wales, by Rev. J. E. Tenison-Woods, F.G.S., F.L.S.—On some Mesozoic fossils from the Palmer River, Queensland, by Rev. J. E. Tenison-Woods, F.G.S., F.L.S.—On French geographical societies and the colonies, by E. M. de la Meslee.—Notes on the aborigines of New Holland, by James Manning.—On the ashes of some Epiphytic ferns, by W. A. Dixon, F.C.S.—On a fossil plant formation in Central Queensland, by Rev. J. E. Tenison-Woods, F.G.S., F.L.S.—The Medical and Microscopical Sections held regular monthly meetings. The sum expended upon the library during the year was 422*l*. 12*s*. 10*d*. At the annual meeting M. Louis Pasteur, M.D., was unanimously elected an Honorary Member of the Society, to fill the vacancy caused by the death of the late Dr. Charles Darwin, M.A., F.R.S., and Dr. Ottokar Feistmantel of Calcutta was elected a Corresponding Member.—Names of the new Council:—President, Hon. J. Smith, C.M.G. Vice-Presidents: Charles Moore, F.L.S., W. A. Dixon, F.C.S. Hon. Treasurer, H. G. A. Wright, M.R.C.S.E. Hon. Secretaries: Prof. Liversidge, F.R.S., F.G.S., Dr. Leibius, F.C.S. Members of Council: Robert Hunt, F.G.S., Dr. W. Morris, P. R. Pedley, Frederick Poolman, Chr. Rolleston, C.M.G., H. C. Russell, F.R.A.S.

PARIS

Academy of Sciences, July 2.—M. Blanchard, president, in the chair.—Obituary notices of M. Maillard de la Gournerie, by M. Bertrand; of Mr. William Spottiswoode, by M. Dumas; and of General Sabine, by M. d'Abbadie.—On the condensation and liquefaction of gases, by M. J. Jamin.—On the tornadoes that swept over Kansas, United States, on May 30, 1879, by M. Faye. Although every tornado almost invariably takes place in the south-west quadrant of an area of comparatively low pressure (Finlay's "Report of 600 Tornadoes"), this meteorological condition is not to be regarded as their true cause. The author shows on the contrary that, like other storms and hurricanes, they are due to whirlwinds descending with vertical axis, and originating, not in the lower atmospheric strata, but in the upper currents whose direction is entirely independent of the light winds previously prevailing near the surface of the earth.—Remarks and observations on MM. Carl Vogt and Emile Yung's treatise on practical comparative anatomy, by M. de Quatrefages. For Darwin's biological tree representing all life past, present, and even future on the globe, Vogt and Yung substitute a grove composed of many distinct trees, the number and species of which still remain to be determined. But while this conception deprives the Darwinian theory of much of its seductive grandeur, evolution itself can lose nothing by abandoning an absolute system in which mere hypothesis plays far too large a part.—On a complete system of the combinations of two biquadratic binary forms, by M. C. Stephanos.—On a class of lineal equations of the fourth order, by M. E. Goursat.—On surfaces of the third order, by M. C. Le Page. A method is proposed of constructing a surface of the third order determined by nineteen points.—On the application of Ampère's method to the determination of the elementary law of electrical induction by displacement, by M. Quet.—Electrodynamic actions involving arbitrary functions; hypotheses determining these functions, by M. P. Le Cordier.—Method of unmagnetising timepieces which have become magnetised by the vicinity of a powerful magnetic field, by M. Deprez.—Action of chlorhydric acid on the protosulphuret of tin, by M. A. Ditte.—On the fusibility of salts, by M. E. Maumené.—On a new process of making a quantitative analysis of urea, by M. L. Hungounenq.—An examination of the corpuscles held in suspension in water, by M. Eug. Marchand.—Deposits of barytine, celestine, and anhydrite, their association and probable mode of formation, by M. Dieulafait. The experiences of M. Gorgeu are shown to be inadequate to explain the formation of these substances in lodes and in saline lands. At the same time they are not to be absolutely rejected, and may prove to be of great value when the chemical aspect of volcanic phenomena is taken seriously in hand.—Influence exercised by the elements contained in sea water on the development of fresh-water animals, by M. H. de Varigny. From experiments made with the spawn of frogs and other organisms, it appears that chloride of sodium (kitchen salt) is the substance most noxious to the development of fresh-water animals.—Application of heat to the preservation of wines in common use, the blends known as "vins de coupage,"

by M. E. Houdart. By this process all danger of fermentation is avoided, while the quality and appearance of the wines so treated remain unimpaired.

BERLIN

Physiological Society, June 15.—In continuation of the experiments upon the influence of temperature upon the time occupied by reflex actions, which Prof. Kronecker described at the last meeting, Prof. Ewald communicated observations which he had made upon patients who were suffering from rabies. These patients responded with a reflex jerk quicker in a temperature between 0° C. and 5° C. than in temperatures between 40°—50°, and at higher temperatures the times occupied by a reflex action again became shorter.—Dr. B. Baginsky spoke about the results of experiments which he had instituted in order to determine the function of the cochlea. It is well known that anatomical research has determined that the membrana basilaris of the cochlea, in which the terminal filaments of the auditory nerve are distributed, increases in breadth from the bottom towards the upper part; and Herr von Helmholtz had founded an hypothesis upon this to explain the differentiating perception of certain higher tones, viz. that the sound-waves that penetrate into the cochlea occasion a synchronous vibration either in the broader upper half or in the narrower lower half of the membrana basilaris, so that the higher tones would excite the fibres of the auditory nerve distributed in the lower part, and the deeper notes the fibres distributed in the upper part. In animals which are low in the scale of development there is a similar arrangement, which consists of auditory cilia of different lengths, which have the same function, as the shorter ones are intended for the higher notes, and the longer ones for the deeper notes and noises, and are set into synchronous vibration by them. This hypothesis has been experimentally confirmed in the case of the auditory cilia of the lower animals, and it had actually turned out true that deep notes produced vibrations in the long hairs, and high notes in the short ones. Herr Baginsky now undertook to test the hypothesis of Herr von Hetmholtz experimentally on the cochlea of higher animals. After he had succeeded in overcoming the great practical difficulties, he wounded the top of the cochlea of the healthy ear in dogs which had been made absolutely deaf of their other ear, and then observed their hearing powers by means of the different notes of organ-pipes between *c* and *c''''*. On the third day after the immediate consequences of the operative interference had disappeared, it was found that the dogs responded perfectly to the notes *c''''*, *c''''*, *c'''*, *c''*, but were deaf to the deeper notes. This condition remained unaltered for weeks, and when the animal that had been the subject of experiment was killed, the post-mortem examination showed that the top only of the cochlea had been wounded, and that the filaments of the auditory nerve that were distributed to that portion were destroyed. Less precise were the results of the experiments in which the lower part of the cochlea was destroyed; in these cases absolute deafness occurred in a succession of cases; in other cases, again, the dogs responded to high as well as to low notes, to the latter, perhaps, a little better; and again, in other cases, on the other hand, the dogs only responded to the notes *c*, *c'*, *c''*, while they were deaf to the higher notes. But this condition only lasted some fourteen days; then hearing power for the higher notes set in again, and soon reached the same sensitiveness as that for the deep notes. Post-mortem examination showed in these various cases different degrees of distinction occasioned by the operation. Herr Baginsky believes that he has by his experiments, in particular by the results of lesion of the top of the cochlea, verified experimentally for the mammalian cochlea the hypothesis of von Helmholtz.—Dr. B. Fränkel spoke concerning the different views of authors as to the behaviour of the pharyngo-nasal isthmus during the phonation of vowels, and about the attempts which have been made, up to the present inconclusive, to prove the closure or the patency of the isthmus. He himself has become convinced by his observations that in the phonation of all vowels the communication between the pharynx and nasal cavity remains patent, although more or less narrowed, and he demonstrated this partly by means of a spirit-manometer, one of whose limbs was brought into connection with one nostril, at the same time closing the other nostril while he was sounding the letter, or by means of a flame towards which he directed an elastic tube which was in connection with both nostrils. Not only on pronouncing A, but also with E, O, I, and U, a current of air was seen to issue from the nose. Dr. Fränkel then discussed the various varieties of nasal speech, of which he distinguishes three anatomical varieties, and finally gave his view as to the function of the uvula, which occurs only in man and in some of the higher apes, viz. that it has got nothing to do with the shutting off the isthmus nasopharyngeus or any connection at all with speech; it serves rather to protect the larynx in swallowing by dividing mouthfuls of solid food and drink into two portions, and thus compelling them to slip down on either side of the larynx; it likewise forms an elongation of the epiglottis.—Prof. Kronecker gave a short exposition of a demonstration which Dr. Openschewsky gave to the Society. In experimenting on the influence of the vagus and upon the gastric movements, it was observed that when the peripheral end of the gastric branch of the vagus is stimulated by single currents, the contraction of the cardia does not occur until after the cessation of the stimulations, although during the continuance of these no contraction of the stomach is observed, when a certain frequency of repetition of the stimuli has been attained. This induced Dr. Openschewsky to examine more closely the inhibitory action of the vagus. It has now been known for a good many years that in high degrees of anæmia the cardia executes spontaneous rhythmical contractions; by ligature of the coronary artery this anæmia could be artificially produced and the rhythmical contractions could be produced in the cardia. If the peripheral ends of the gastric branches of the vagus were now stimulated, an inhibition of these movements of the cardia regularly occurred, which lasted as long as the stimulation of the vagus. It is thus proved that the gastric branches of the vagus contain inhibitory as well as excitant fibres, exactly as its cardiac branches; and Dr. Openschewsky proposes to work out this part of the physiology of the vagus still further.

VIENNA

Imperial Academy of Sciences, April 12.—M. Abeles, on secretion from the living kidney if floated through with blood.—E. Hussak, on cordierite in volcanic outcasts.—E. Zuckerkandl, on the communications between the arteries of the human lung.—J. Wroblewski and K. Olszewski, on the liquefaction of oxygen and solidification of carbon disulphide and alcohol.—G. Goldschmidt, on pyrene-quinone.—T. von Oppolzer, tables to determine eclipses of the moon.—J. Liznar, a note on the theory of Lamart's variation apparatus for horizontal intensity.

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THURSDAY, JULY 19, 1883

CHOLERA PROSPECTS

THE early history of cholera is involved in a good deal of obscurity, and it was not until 1817, when the disease caused a terrible mortality amongst our troops in India, and subsequently spread into different parts of the Asiatic continent, that any noteworthy attention was given to it by European observers. It is very possible that even previous to the present century cholera had made its way into Europe, but the first trustworthy record of its course westwards was in 1831, when it travelled by way of Russia and the Baltic, and, as far as we know, made its appearance for the first time in England. In the following year it became widely prevalent in this country. In the years 1848-49, and again in 1853-54, cholera travelled to Europe and England from the East, taking much the same route as it did in 1831-32. The last outbreak from which we have suffered was in 1865-66, the disease being imported into Southampton in 1865, and reappearing both in the metropolis and in several other parts of the United Kingdom in the following year. But on this occasion the infection for the first time reached us through Egypt, having travelled there in the track of the Mohammedan pilgrims, who were on their return from Mecca, and being then distributed along the lines of steamboat traffic which, starting from Alexandria as a centre, radiate towards ports in the Mediterranean and on our own shores. In 1866 the disease became epidemic in the metropolis, and its special incidence in the East End was shown to be in the main due to the polluted character of the water delivered to that part of London.

The disease is once more prevalent in Egypt; it has already caused over 2000 deaths in a few towns in the delta of the Nile, and the prospect of its spread to the several ports of Europe is regarded with universal concern.

The etiology of cholera, in so far as relates to its influence in this country, does not admit of much doubt. The infection must be actually imported into our midst; it has never yet been imported except through human agency, and the poison appears to be all but, if not entirely, limited to the discharges from the bowels and to the matter vomited by the patients. Where these go the poison goes; hence sewers and drains receiving them tend to become channels for conveying the disease; soil fouled by them may, by leading to the pollution of well and other waters, as also by aërial emanations, favour its diffusion; and, to a less extent probably, the bed-linen and personal clothing of the sick may become vehicles of infection. In all essential respects the disease appears to spread under much the same conditions as favour the spread of enteric or typhoid fever, and, like that disease, it has in this country mainly been associated with the use of water supplies, which have been subjected to the risk of receiving the specific infection. What that infection consists in is not yet known, but judging from analogy it is a definite organism capable of reproducing its own kind under those conditions of filth which we have adverted to as being associated with the spread of the disease. In the case of anthrax, which causes the so-called wool-sorter's disease in man, and in the case of relapsing or famine fever, the microscope has succeeded in showing the organisms which lead to the production of those specific affections; but in the case of cholera no such results have as yet been attained, and this notwithstanding the laborious microscopic and other researches which have been made in India and elsewhere.

Having regard to the fact that cholera is as yet confined to Egypt, and that any spread may be expected to follow on the lines of human intercourse, the most obvious means of staying its spread to this country would at first sight appear to consist in quarantine measures. Such measures are already in force all along the Mediterranean, and even on the Atlantic coasts of Portugal, Spain, and France, but England has decided to adopt no such course, and our Government have acted wisely in arriving at this decision. Quarantine, in order to be efficient, must exclude all the healthy as well as the sick who arrive in our ports after having passed through the infected area, and if there be really reason to believe that vessels so arriving contain within them the germs of infection, and that those on board are liable to contract cholera, the result of detaining suspected fleets of merchant and passenger ships at the entrance of our ports until the last of those who are susceptible have suffered from the disease can be more readily imagined than described. In point of cruelty and selfishness such a practice could probably not find its equal. But, as a matter of fact, quarantine invariably fails to effect its intended purpose; those countries which practise it most rigidly are those to which cholera has almost invariably spread, and the line of loaded rifles and fixed bayonets by which quarantine measures have surrounded Damietta and Mansurah, the two first towns infected in Egypt, have certainly not succeeded in preventing extension of the disease along the lines of railway in the direction of Cairo and of Alexandria. For some thirty years we in England have trusted to a different system; and that system, which is known as one of "medical inspection," has received the formal assent of the delegates of the Cholera Conference which met at Vienna in 1866. Instead of herding the healthy together with the sick, we endeavour to deal with the sick and their infected things in such a way as to prevent the spread of infection to the healthy. To take an example. A ship arrives from Port Said in the Thames. Off Gravesend it is boarded by a Customs officer, to whom written statements as to the health of all present or previous passengers must be made. If any case either of cholera or of suspicious diarrhœa has occurred, the vessel is detained for a period sufficient to allow of a medical examination of all passengers and the crew by an official of the Port Sanitary Authority, who in turn have power to remove to their hospital ship all infectious patients, to detain for a period of probation all suspicious cases of sickness, and to disinfect the vessel and all infected articles. The really healthy are however permitted to land, and the vessel itself is detained no longer than is needed in the interests of health.

So far as the importation of the disease is concerned, our national system tends to greater security than a process of rigid quarantine, which would certainly be evaded

as it has been heretofore. It remains for all who are concerned to see that our water sources, whether public or private, shall be free from all risk of contamination, and so to arrange our means of house and of public drainage as to secure all dwellings against the entrance of sewer air into them. Much has been done in these directions since cholera last threatened our shores, but more remains to be done if we are to rid ourselves of all the conditions which will tend to favour the spread of that disease should it succeed in finding an entrance into our country.

MODERN PERSIA

The Land of the Lion and Sun: or, Modern Persia. By C. J. Wills, M.D. (London: Macmillan and Co., 1883.)

ONE of the "Fathers," the great Austin we believe of Hippo, when asked which was the first Christian virtue, replied, Humility! And the second? Humility! And the third? Still Humility! So Dr. Wills would seem consciously or unconsciously to think that of travellers the first, second, and third virtue is *anecdote!* The result of this belief is one of the most graphic and entertaining books of travel ever published. With anecdote it begins, with anecdote it ends, and its substance is anecdote, and all these endless anecdotes are themselves distinguished by three cardinal virtues. They are characteristic, they are well told, and they are infinitely varied. By way of experiment we have opened the book at haphazard at twelve different places, and at every place there was an anecdote, some pithy story or other illustrating the social customs and habits of the Persians and even of the very plants and animals of the Iranian world, where the author's lot was cast for the space of fifteen years (1866–1881) as "one of the medical officers of Her Majesty's Telegraph Department in Persia." On one of the pages thus exposed occurs the subjoined incident bearing directly on the "scorpion controversy" recently carried on in the correspondence columns of NATURE:—

"A story was told me by the late Dr. Fagergren, a Swede who had been twenty-five years in Shiraz, to the effect that scorpions, when they see no chance of escape, commit suicide; and he told me that when one was surrounded by a circle of live coals, it ran round three times and then stung itself to death. I did not credit this, supposing that the insect was probably scorched and so died. I happened one day to catch an enormous scorpion of the black variety, and to try the accuracy of what I supposed to be a popular superstition, I prepared in my courtyard a circle of live charcoal a yard in diameter. I cooled the bricks with water, so that the scorpion could not be scorched, and tilted him into the centre of the open space. He stood still for a moment, then to my astonishment ran rapidly round the circle three times, came back to the centre, turned up his tail where the sting is, and deliberately by three blows stabbed or stung himself in the head; he was dead in an instant. Of this curious scene I was an eye-witness, and I have seen it repeated by a friend in exactly the same way since, on my telling the thing, and with exactly the same result. For the truth of this statement I am prepared to vouch" (p. 249).

More startling is the account at p. 307 of the "house-snake and sparrow."

"One morning I heard a great twittering of birds, and on looking out I saw some thirty sparrows on the top of a half-wall. They were all jumping about in a very excited manner, and opening their beaks as if enraged, screaming and chattering. Presently I saw a pale-yellow coloured snake deliberately advancing towards them from the ornamented wooden window from which he hung. They appeared *all* quite fascinated, and none attempted to fly away. The snake did not take the nearest, but deliberately chose one and swallowed him. I got my gun, and notwithstanding the entreaties of my servants, some of whom wept, assuring me that the reptile was inhabited by the late master of the house, I gave him a dose of duckshot. He was a big snake, some four feet long. I cut him open and extracted the sparrow. After some ten minutes' exposure to the sun, the bird got up, and after half an hour flew away apparently unhurt. The snake was not a venomous one, nor do we find venomous ones in houses in Persia."

Suitable also for the columns of a *scientific* journal may be the subjoined about the "transit of Venus":—

"On the high road to the capital from the Caspian the members of the expedition sent by the German Government to observe the transit of Venus met a lovely vision in habit and hat on a prancing steed. They halted, saluted, and declared their errand.

"'To observe the transit of Venus, ah, well, you can go home now, gentlemen, *your duty is done*, good bye;' and the pretty vision disappears at a smart canter 'away in the ewigkeit,' as Hans Breitmann says. *That* joke dawned on those Germans after some hours" (p. 331).

Dr. Wills has naturally a good deal to say about the Persian system of medicine, which "has its advantages in its delightful simplicity. All diseases are cold or hot. All remedies are hot or cold. A hot disease requires a cold remedy, and *vice versâ*. Now if the Persian doctor is called in, and has any doubt as to the nature of the disorder, he prescribes a hot treatment, let us say. If the patient gets better, he was right; if worse, then he prescribes a cold remedy, and sticks to it. He thus gets over all need for diagnosis, all physiological treatment, and he cannot, according to his own lights, be wrong. . . . His fee is a few pence, or more generally he undertakes the case on speculation: *so much*, of which he is lucky if he gets half, if the patient gets well; nothing if he doesn't. . . . Remedies and contrivances of a barbarous nature, such as putting the patient in fresh horse-dung, or sowing him up in a raw hide, are the rule rather than the exception" (p. 34).

Talismans, spells, and charms of all sorts are also much relied upon, in connection with which a characteristic story is told:—

"During the cholera in Shiraz I was attending the daughter of the high priest, who was sitting surrounded by a crowd of friends, petitioners, and parasites. He was writing charms against the cholera. I, out of curiosity, asked him for one; it was simply a strip of paper on which was written a mere scribble, which meant nothing at all. I took it and carefully put it away. He told me that when attacked by cholera I had but to swallow it and it would prove an effectual remedy. I thanked him very seriously, and went my way. That day he called and presented me with two sheep and a huge cake of sugar-candy weighing thirty pounds! I did not quite see why he gave me the present, but he laughingly told me that my *serious* reception of his talisman had convinced the many bystanders of its great value, and a charm desired by an unbelieving European doctor must be potent indeed. 'You see, you might have laughed at my

beard; you did not. I am grateful. But if I could only say that you had *eaten* my charm, ah—then!' 'Well,' I replied, '*say so* if you like,' and our interview ended" (p. 291).

Like most Europeans who have lived long amongst them, our author learnt to regard with very kindly feelings the simple-minded natives who with all their faults are endowed with many noble qualities of head and heart. The Persian is here described as "hospitable and obliging, as honest as the general run of mankind, and especially well disposed towards the foreigner. Home virtues amongst the Persians are many. He is very kind and indulgent to his children, and as a son his respect for both parents is excessive. But the full stream of his love and reverence is reserved for his mother; and an undutiful son or daughter is hardly ever known in the country" (p. 314). Here of course follows a flood of anecdotes, some of which serve also to illustrate the character of the Armenians, of whom he has little good to say. "I will not trust myself," he writes, "to give my opinion of the Armenians. Of course I have known brilliant exceptions; but when I say that I indorse all that Morier, Malcolm, Lady Shiel, and the standard writers on Persia have said of these people, I need not add that my impression is unfavourable in the extreme. They possess one good quality, however, thrift" (p. 316).

In a work professing to give little more than personal experiences, valuable because derived from a lengthy residence in every part of the country, it would be unfair to look for any systematic information regarding the physical features, products, or natural resources of the land. Nevertheless, many useful details connected with these points occur here and there, and the statements made regarding the abundance and extraordinary cheapness of good provisions in all the fertile provinces would seem to justify the conclusion that Persia is not yet quite "played out." Cheese and butter at twopence a pound, flour and bread at a penny in the towns and much less in villages, eggs at ninepence per four or five dozen, quails and partridges at fourpence a brace, hares at fourpence each, lamb and mutton at proportionately low rates, make Persia "the poor man's paradise, in fact, *to live in, the cheapest country in the world*" (p. 298).

The work is furnished with a convenient glossary and an index, which contains some rather amusing entries; but there are neither maps nor illustrations beyond a solitary *chupper-khana* (posthouse) facing the title-page. But no such attractions were needed to render the "Land of the Lion and Sun" a far more entertaining book than most of our fashionable three-volume novels.

A. H. KEANE

CHLOROPHYLL CORPUSCLES AND PIGMENT BODIES IN PLANTS

Ueber die Entwickelung der Chlorophyllkörner und Farbkörper. By A. W. F. Schimper. (*Bot. Zeitung*, 1883.)

Ueber Chlorophyllkörner, Stärkebildner und Farbkörper. By A. Meyer. (*Bot. Centralblatt*, 1882.)

CONTRIBUTIONS to a more exact knowledge of the contents of the vegetable cell have increased of late to an extent which justifies the hope that some generalisation of the facts may before long be possible; meanwhile botanists must have experienced a feeling akin to dismay at the scattered condition of much of the literature, and the apparent hopelessness of collating the facts dealing with normal and abnormal cell contents. The works of Strasburger, Schmitz, Schimper, and others have already cleared the way to a better comprehension of many details, especially with regard to the cell nucleus and starch grains; but with each step it has been felt that the pushing back of the phenomena towards a common cause has raised other difficulties hitherto unforeseen.

In the isolated position of such structures as chlorophyll grains and pigment corpuscles as unexplained cell contents, we have an illustration of wide significance in this connection, and the attempt to bring all such bodies as these and the "starch-forming corpuscles" of Schimper into definite relationship one with another must be welcomed as promising much simplification of nomenclature and discussion, the more so, since these relationships are now shown to be genetic, and therefore real. Schimper in Bonn, and A. Meyer in Strasburg, proceeding independently, have arrived at the conclusion that the chlorophyll corpuscles, "starch-forming corpuscles," and pigment bodies of the higher plants are simply the more or less modified and mature conditions of certain minute protoplasmic structures found together with the nucleus in the youngest cells of any meristem.

Whereas botanists have assumed that chlorophyll grains, starch-formers, nuclei, &c., are produced free in the protoplasm of the cell, we are now called upon to note that such is not the case; but that these bodies arise from distinct structures present in the young cell from its earliest existence, and that any pigment (green or otherwise), starch grains (directly assimilated or not), &c., found in connection with the structures named, arise by later changes in the substance of the protoplasmic corpuscles produced by continuous growth and division of the few, minute "plastids" found in the young cell.

Meyer and Schimper agree in all essential points regarding the relationship and development of these bodies, and the slight differences in details and nomenclature between the two investigators in no way affect the main question.

To quote an example, we may take Schimper's description of the development of the pigment bodies occurring in the flower of *Hemerocallis fulva*. The cells of the perigone contain brick-red crystalline needles or three-pointed tablets, which arise as follows:—

In the very young flower bud, the cells contain, besides the nucleus and cell-protoplasm, minute bodies which Schimper names *plastidia*—a general term for these bodies in all meristems, and independent of any function afterwards performed by them. When the flower bud is already green the *plastidia* nearest the light have acquired a distinct green colour, and function, no doubt, as chlorophyll corpuscles; all such green *plastidia* are called by Schimper *chloroplastidia*. The *plastidia* in the cells more deeply situated, however, remain pale, and may be called *leukoplastidia*. All stages intermediate between *leukoplastidia* and *chloroplastidia* occur. The small lenticular *chloroplastidia* increase in size, become flatter, and divide as the cell grows. They then become narrower and pointed, some becoming needle- or spindle-shaped

a few remain broad, and finally acquire a triangular form with sharply pointed corners.

Meanwhile, the colour passes through intermediate dirty shades from green to brick-red; and, some time before the flower bud opens the ultimate shape and colour are attained, and the bodies are now called *chromoplastidia*. Many similar instances have established the connection between the three kinds of *plastidia*,[1] *e.g.* petals of *Senecio, Bellis, Tropæolum*, fruits of *Sorbus, Rosa, Lonicera*, &c.

The primitive *plastidia* are universally present in the meristems of the higher plants, and have now been found in so many seeds and embryos, that Schimper suggests that they no doubt exist in the embryo-sac and oosphere from the first. All the *chloroplasts* of the plumule and stem-axis, &c., arise by division of the plastidia in the *punctum vegetationis* of the young stem; these may be green from a very early stage, or acquire their green colour later, or remain colourless (*leukoplasts*). In cases where the *leukoplasts* form large starch grains, we have the *Stärkebildner* discovered by Schimper in 1880; all the kinds of *plastidia*, however, may be found in connection with starch grains, which often become resorbed later.

In the same way, all the *chloroplasts*, *leukoplasts*, and *chromoplasts* of the roots arise by division and differentiation of the few primitive *plastidia* in the *punctum vegetationis* of the radicle.

Since *chloroplasts* or *leukoplasts* are found at a very early age in the embryos of *Crucifers, Leguminosæ, Geraniaceæ*, and many others, Schimper considers it probable that they arise from primitive *plastidia* in the oosphere. *Chloroplasts* and *leukoplasts* (as starch-forming corpuscles) are visible in the embryo of *Linum austriacum* when it consists of eight cells only, and the minute starch-grains observed in the embryo sac and oosphere of that plant are no doubt contained in *leukoplasts*—which become green afterwards and are then visible. Schimper finds that the primitive *plastidia* may remain colourless as *leukoplasts*—which, if they form starch grains, are the *Stärkebildner* of his earlier papers—or may become *chloroplasts*, as is usual (but by no means universal) in cells exposed to light, which remain green, or pass over into *chromoplasts* (most flowers and fruits). Nevertheless, the order of change is not fixed, and no sharp lines can be drawn—thus, a *leukoplast* may become green, and function as a *chloroplast* for a time, and finally lose its colour again, and become a *leukoplast*.

The *Characeæ* seem to be the earliest plants in which all three forms of these bodies occur; the apical cells containing *leukoplasts*, and the antheridia red *chromoplasts*. Schimper suggests that if the oosphere is proved to contain already formed *plastidia*, it will support the view that the higher green plants owe their origin to symbiosis of green and colourless organisms. The author enters into no particulars, however, concerning this hypothesis, which appears by no means obvious in the light of other considerations.

[1] A. Meyer terms the bodies ana-plasts (= leucoplastidia), auto-plasts (= chloroplastidia), and chromo-plasts (= chromoplastidia) respectively. He uses the generic term *trophoplasts* to embrace all collectively. We may call them leuko-, chloro-, and chromo-plasts, since these names imply no functional peculiarities.

The following may be selected as further illustrations of Schimper's work:—

1. *Leucoplasts* arise from colourless *plastidia* (roots, &c.) or, more rarely, from *chloroplasts* (*e.g.* fruit of *Symphoricarpus*). They may become green *chloroplasts* (many embryos), or function as *Stärkebildner* (*e.g.* deeply-situated cells), or remain apparently without function (*e.g.* epidermis cells). In many flowers they become *chromoplasts*.

2. *Chloroplasts* (*i.e.* chlorophyll corpuscles) arise from the growth and division of primitive *plastidia* which are already green, or by the development of green colouring-matter in *leukoplasts* exposed to light. They often become *chromoplasts* later.

3. *Chromoplasts.*—All shades occur between pure carmine-red and greenish-yellow—never blue—the earlier statements being based on errors of observation.[1]

The development of the colouring matter is frequently attended by a disappearance of the starch grains on or in the *leukoplast* or *chloroplast* from which the *chromoplast* arises. As sometimes occurs with other bodies, the spindles, needles, and tablets produced as the ultimate forms of the *chromoplasts* appear to proceed from a process of crystallisation of certain of the proteid contents of the *chromoplast* from a formless matrix of living protoplasm. In these cases the pigmented tablets, needles, rods, &c., must be regarded as crystalloids. More rarely the proteids of the *leukoplasts* and *chromoplasts* separate in the same crystalline form.

Schimper distinguishes three types of *chromoplasts*:—

1. The spherical type, found in the arillus of *Taxus*, fruit of *Solanum*, &c.

2. Two or more pointed needles, tablets, &c., of *Hemerocallis, Lilium, Tropæolum*, and other flowers. In the fruits of *Rosa, Lonicera*, &c., both these types occur together.

3. In this type the *chromoplasts* are rod-shaped—*e.g.* flowers of *Tulipa*, root of *Daucus*, &c.

No relations can be discovered between the form, &c., of any of these bodies and the natural groups in which they occur.

H. MARSHALL WARD

OUR BOOK SHELF

The Forests of England and the Management of them in Bygone Times. By John Croumbie Brown, LL.D. (Edinburgh: Oliver and Boyd, 1883.)

French Forest Ordinance of 1669, *with Historical Sketch of Previous Treatment of Forests in France.* Compiled and Translated by John Croumbie Brown, LL.D. (Edinburgh: Oliver and Boyd, 1883.)

THESE two little books, published almost simultaneously but in the order in which their titles are given above, have been written, as Dr. Brown tells us, "as a small contribution to the literature of Britain on subjects pertaining to forest science." The author has shown in previous writings on kindred subjects the scarcity of English literature on forestry as compared with that of France and Germany, and he again draws attention to this fact by copious extracts in "The Forests of England" from a little work of his on "The Schools of Forestry in Europe," published in 1877.

The forests of England, exclusive of their practical utility, have played a not unimportant part in the history

[1] Schimper points out how easily such bodies as these are altered by processes hurtful to the cell: they must be observed in perfectly fresh, uncut, and uninjured cells.

of our country, and consequently any records or facts connected with them have a charm both for the forester as well as for the general reader. Dr. Brown's book on "The Forests of England" is therefore far from dry reading, treating as it does of such well-known forests and parks as Sherwood, Epping, Dean, and the New Forests, Woolmer, Whitlebury, Windsor, Malvern, Cannock, and Hatfield Chases, &c.

A good deal of attention is being directed at the present time to the preservation of our forests in their natural beauty, and we should hope that Dr. Brown's books will at least have the effect of sharpening the interest of those who have hitherto been indifferent about the works of draining and planting that are always ready to be put forward as improvements, but which are for the most part of a character that should not be allowed to be carried out without deep and serious consideration by those qualified to advise.

"French Forest Ordinance" is a book of a more practical character than the preceding, inasmuch as it deals more with forest treatment and legislation in France, nevertheless it contains much of interest. The following extract from Chapter III. will explain: "It has been mentioned that the forests were exploited at that time [middle of seventeenth century] on a system of exploitation known as *jardinage* or *furetage*. The method of exploitation so designated is that which is generally followed in the management of woods in England, and of forests in our colonies—felling a tree here and there, and leaving the others standing—and is called in French forest economy *jardinage*, or gardening, from its similarity to the procedure of a gardener gathering leeks, onions, turnips, carrots, cabbages, or cauliflowers—taking one here and there, not at haphazard, but with some principle for his guidance—it may be to thin them—it may be to gather in the mature, and leave the others to grow; and called *furetage*, or ferreting, from the similarity of the woodman's procedure in seeking out what trees to fell—to what is called, from the conduct of a ferret, ferreting out what is wanted when it does not at once appear."

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications.*

[*The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to insure the appearance even of communications containing interesting and novel facts.*]

"Waterspouts" on the Little Bahama Bank—Whirlwind at Grand Cayman

WE have received the following communication, by an officer of H.M. surveying vessel *Sparrowhawk*, employed in the West Indies, from the Hydrographer to the Admiralty :—

Being much interested in the subject of waterspouts and their formation, and having failed to find anything about them in the works of recognised authorities, I venture to record some personal experiences together with what information I have been able to collect from the inhabitants of Abaco and the adjacent bays.

During the summer months waterspouts are common on the Little Bahama Bank. I have seen seven at once in water varying from ten feet to over a hundred fathoms, and I am informed that fifteen have been observed.

I have noticed that the first movement which eventually produces a waterspout is a whirlwind on the surface of the water gradually increasing in velocity of rotation and decreasing its diameter as it travels along before the prevailing wind. The spray is lifted to a height of from five to ten feet, and then gradually melts away, assuming the appearance of hot air, which is visible (still rotating) to a similar height above the spray. A motion amongst the clouds soon becomes apparent, a tongue is protruded, and the spout becomes visible from the top downwards.

On one occasion a portion of a spout appeared for a moment in mid air above the disturbance on the surface of the water.

Although these appearances are commonly called "waterspouts," I am informed by men who have been caught in them that they contain no water, and should be properly called "windspouts;" the small fore-and-aft-rigged schooners that ply on the bank do not fear them, although a prudent captain would probably shorten sail to one. I have been unable to hear of an accident having occurred through a vessel being caught in a waterspout.

They frequently cross the land, but no water falls; they take up any light articles, such as clothes spread out to dry, straw, &c., that happen to be in their course, but have never been known to carry anything along with them for a distance.

At Grand Cayman Island I noticed a whirlwind on the water, of somewhat similar appearance to those of the Little Bahama Bank just mentioned, though there was no cloud above it; the place where it appeared was a sheet of shoal water between the fringing reef and the shore, about one cable in breadth and three to ten feet deep. The whirlwind passed about fifty yards from where I stood; its estimated diameter was fifteen feet, and it whirled rapidly from left to right; the spray was lifted from the surface in a revolving sheet to a height of ten feet, but appeared to get thinner towards the top, and gradually melted away till it looked like the air over a boiling cauldron visible to a height of ten feet above the spray. I estimated its rate of progression at five knots; the wind was light (force 2). The whirling spray made a continuous hissing noise like a fast boat under sail passing close; it caused no particular wave on the beach and left no wake; its character was unchanged for half a mile, when I lost sight of it by its passing a point.

The inhabitants informed me that in their memory several whirlwinds had passed, but none had been known to cross the land.

MORRIS H. SMYTH

A Remarkable Meteor

A METEOR was seen at Hendon on the 6th inst., at 8.53 p.m., in a clear sky, and broad daylight. The course by compass was from north-east to east, at an altitude of about 27° above the horizon when first seen, and 22° when it disappeared, after being visible six or seven seconds. I drew the attention of a friend, in whose garden we were standing, to it. He saw it about three seconds, and compared it to a stream of fire. I learnt later that it was also seen by parties boating on the waters at the Welsh Harp, but could not get any particulars beyond the fact that it was seen. Its passage appeared attended by intense combustion. It first appeared as a circular ball of fire, but speedily lost a spherical shape, and became pointed, resembling somewhat a spear head, as though the change in appearance were due to the resistance of the atmosphere. From a deep red at first it became of a decided golden colour, to change to a brilliant white just before or as it disappeared. There was nothing special about the disappearance.

P. F. D.

London, W.

The Function of the Sound-Post in the Violin

I READ with much interest the part of Dr. Huggins's paper which relates to the above subject, having myself tried numerous experiments in the same direction. The conclusions I arrived at do not so much differ from those set forth in the paper, as that I venture to think they go a step further. It is on this plea that I ask for the acceptance of the following observations :—

It is undoubtedly true that the sound-post of a violin does communicate the vibrations from the belly of the instrument to the back; but, as will be hereafter seen, these vibrations are not of an order to reinforce the *sound* except to a limited extent. By far the most important function of the sound-post is that it acts as a prop to the belly in such a position and in such a manner as to enable the latter to give out a more resonant *order* of waves. The back may, and does, give out a modicum of sound, but it is especially the belly which becomes more resonant under the influence of the prop than without it.

In the first place, when the sound-post is removed, the belly of the violin is then an uninterrupted elastic table with a vibration rate of its own, its greatest elasticity being just at the part where the bridge is situated. Now it may safely be predicted, without resorting to experiment, that this specific rate of vibration of the belly itself will interfere with the varying rates of vibration communicated to it by the strings. That it is so, however, I have conclusively proved by actual experiments in great

variety, and when such interference takes place the tone is always meagre, as described by Dr. Huggins.

But it will naturally occur that there must be *one* note in the scale of the instrument which will coincide in its vibrations with those of the belly when in this un upported condition, and that this note ought to be exceptionally loud. It is so in fact, but not to the extent that might at first be supposed. This is because in reality, as I shall try to explain, the injurious effect of interference does not include the whole question. When a tuning-fork is struck and held out of contact with a resonant body, it gives out a very feeble sound. The cause of this, as is well known, is that each half-wave is compensated and partly annulled by the succeeding half taking place in the opposite direction. A string stretched between two non-resonant supports does the same when plucked or bowed in the middle. In like manner, the belly of a fiddle, when unsupported by a sound-post, is under conditions which are very similar to those of the string. The most yielding part is immediately beneath the bridge, under the impulses of which every point of every longitudinal fibre moves up and down in the *same phase*, and every half-vibration cancels the effect of the half immediately preceding. The sound is correspondingly feeble. The wave, in fact, is not a true one. It is a to-and-fro, self-compensating motion all along the line. If the bridge were placed near one end of the instrument, the ca e would be different. Its nearness to a support or fulcrum on one side would cause the free part on the other side to break into a wave of progression, which is the true dynamic sound-giving wave. The office of the sound-post is precisely this. It forms a node at a particular part under the influence of which the wave is converted into one of *contrary phases* all over the surface. Such a wave travels in wood at amazing rapidity, and the consequence is that every half vibration reaches its limit and strikes the air almost before the other half has commenced its career, and therefore before it has had time to interfere with its dynamic effect. The best position of the node is found to be just behind the E string, because the higher the note the greater is the firmness required. The G string is further removed from the support, because the lower notes require greater freedom of motion, but it still partakes of its advantages.

I have never met with a satisfactory explanation of the cause of resonance in sound-boards. It cannot be due to extended surface in the sense that there are more extended vibrations or more numerous ones, because the greater the quantity of matter put in motion the more is the motion diluted. The investigation is practically a difficult one, owing to the extreme minuteness of the oscillations which have to be traced, but so far as the experiments indicate which I have been able to devise, the true cause does seem to be what I have been endeavouring to explain. A resonant wave is a *travelling* wave—the crest is always in advance of the depression, and expends itself dynamically before the latter has time to neutralise it. On the other hand, the depression succeeds in due order and produces a similar effect. It is in this sense only that an extended surface is useful and necessary.

If we need confirmation of the principle thus advanced, we have it in every wind instrument without exception. The type of all such instruments is the reed, the only difference being that in some it is aërial, and in others substantial. Take therefore an ordinary harmonium reed, and vibrate it with the finger. However elastic it may be, the sound is of the feeblest character. The double vibration is a compensated one—but let a current of air traverse the point of disturbance, the reed then speaks, or rather the current of air speaks. The half vibration has proceeded so far from its origin that it expends its dynamic force before the succeeding half is able to reach and neutralise it—the crest of the wave, as it were, has smitten the shore, before the depression has had time to overtake it. The depression then succeeds and does its own work.

R. HOWSON

Middlesbrough

Waking Impressions

THE accompanying experience may be of interest to some of your readers; and that it may be the more genuine in the recounting of it, I copy the little entry I made in my notebook some few hours only after the occurrence, as it was so distinctly impressed on my mind that I could not but be struck by it as being worth taking note of.

I have not unfrequently been on the point of noting down similar visual impressions between sleeping and waking time, but have hitherto always found that they were really of so fugitive a nature, or the mind so little sensitive as not to be retentive, that the mere effort to recall them and put them into uttered words (whether audibly or only mentally uttered) was quite sufficient to dispel the impression totally; though by a long directing of the memory I could sometimes *nearly* recover it, not perfectly enough, however, to feel confident that imagination had not added somewhat to the picture. But the present case has been so vividly impressed on my mind that it has been fairly caught, to my own satisfaction at any rate, and I hope that it may be not unworthy of a corner in your valuable paper.

"*Reigate, July* 13

"This morning I woke up suddenly with the end of a dream and found myself reading, as if from a printed book, only there was no book, merely printed words, thus: '*So while he was enjoying himself at . . . . she was in deep depression at Kaÿ-ro.*' The 'Kaÿ-ro' looked quite right, and I quite naturally pronounced it Cairo, and knew I meant that town. I was so struck by the clearness of the visual impression that, for fear of losing it, as one generally does, I instantly recounted the thing to my husband; but in the uttering of it when wide awake I could not at the moment, *even so soon* after the dream, recollect the name of the other locality (marked here by ' . . . . '), though I knew that it had been printed and read by myself in the dream. But about four minutes later, as we were talking it over, I said, 'It is so strange, for I'm sure I've not been talking or thinking either of *Beloochistan* or Cairo!' and at once it flashed upon me that Beloochistan had been the other name, and I had then and there reseen the impression after an interval of total oblivion of it.

"There had been no idea of book or sheet to carry the printing, nor, I fancy, even *solidity* of any kind in the letters; but that the whole phrase was conveyed to my mind through a printed form and by a process of reading I am quite certain. We were on a visit, and the night before had been greatly entertained by the conversation of our host, who had been a great traveller, and we had certainly talked much of India, Cashmere, and Assam; but as far as I can now, or could then, recollect, we had most certainly *not* mentioned either Beloochistan or Cairo, nor had I been reading a novel before going to sleep or during the previous day."

Collingwood, Hawkhurst, July 14

J. MACLEAR

Tertiary Corals

I SHALL be obliged if you or any of your readers would kindly inform me the best authority to consult on the tertiary corals of Piedmont and Liguria; also the age of the beds in the lower part of Val d'Andona.

W. E. BALSTON

Bearsted House, Maidstone, July 15

Wild Fowl and Railways—Instinct and Intelligence

I AM happy to find that my experience of "ducks and railways" is confirmed by so high an authority as Mr. Goodsir from observations made on the other side of the world. Agreeing so far, we differ as to the cause by which the birds are influenced, Mr. Goodsir attributing it to "quick and unerring instinct," whilst I credit the ducks with "quick intelligence" or reasoning powers. If caused by the "teaching" of instinct, the ducks should show no alarm on the sudden and first appearance of a smoking, roaring train in their midst. They certainly do at first show alarm, but as they receive no injury, their intelligence teaches them, after a brief experience, that there is no danger.

I may perhaps be permitted to give one of many instances known to me of the quickness of birds in acquiring a knowledge of danger. Golden plover, when coming from their breeding-places in high latitudes, visit the islands north of Scotland in large numbers, and keep together in great packs. At first they are easily approached, but after a very few shots being fired at them, they become not only much more shy, but seem to measure with great accuracy the distance at which they are safe from harm; the sportsman, however, not unfrequently takes an unfair advantage of them by loading with a wire cartridge, which adds twenty yards or so to the distance at which the gun will kill when charged in the ordinary way.

It would be easy to adduce many cases of what may be considered pure and true instinct, of which the following is perhaps not a bad example, and not unworthy of mention, if it has not already appeared in the columns of NATURE or elsewhere:—

If the eggs of a wild duck are placed with those of a tame one under a hen [1] to be hatched, the ducklings from the former, on the very day they leave the egg, will immediately endeavour to hide themselves, or take to the water if there is any near, should any person approach, whilst the young from the tame duck's eggs will show little or no alarm, indicating in both cases a clear instance of instinct or "inherited memory."

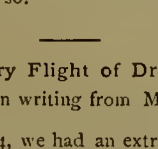
4, Addison Gardens, July 16 JOHN RAE

Clouds

THE following notes of a cloud action, which, so far as I am aware, is not common, may be considered worthy of record.

The occurrence took place at Chatham at about 1 p.m. on Sunday the 1st inst., and attracted attention more particularly from its following a week of strong electrical disturbance in the neighbourhood, accompanied by two fatal results.

At the hour named above, and apparently at a considerable height, certain semi-transparent clouds arranged themselves in thin columns at right angles to each other, some of the columns giving off shoots throughout their length, in shape somewhat resembling blades of grass. Whenever fleecy clouds passed between the foregoing formation and the earth, they were quickly



broken up into small, attenuated components which gradually reunited on getting out of the influence; but on one occasion a very small cloud thus acted upon set itself in the form of a right angle also and remained so. R. Y. ARMSTRONG

July 7

Extraordinary Flight of Dragon-Flies

AN English gentleman writing from Malmö, in Sweden, on July 3, says:—

"On Sunday, June 24, we had an extraordinary flight of the *Trollslända* (*Libellula quadrimaculata*, Linn.), . . . a brown dragon-fly an inch and five-eighths long and three inches from tip to tip of the wings. . . . They passed over or through the town and neighbourhood for about half an hour in the afternoon. The next day about 1 o'clock they reappeared for more than an hour, but on Tuesday the 26th, at 7.30 a.m., they again began in millions, and notwithstanding the wind had shifted to the south during the night, they held the same course from north-west by west, heading south-east by east. The streets, shipping, and every place were full of them. They did not fly very high, and seemed to avoid going into open doors and windows. Some hundred or so alighted on the gooseberry bushes, apple and pear trees in this garden, but never touched the fruit. I observed one sitting on the dead tip of an apple-twig, and I pushed it off with my stick *thirteen* times, the insect returning each time after flying away about five or six yards. . . . The flight ended that night about 8 p.m., having been incessant for more than twelve hours. On the 27th they appeared again about noon, flying the same course, but in much reduced forces. Each day since I have seen a few, but very few. . . . The papers say they were observed in all southern and Central Sweden, and in many places in Denmark, and they swarmed about the ships in the Sound. With their disappearance came the hot weather."

The foregoing extracts seem to me worthy of record in the pages of NATURE, and I accordingly forward them with that view. ALFRED NEWTON

Magdalene College, Cambridge, July 11

Sheet Lightning

WE had here last night a violent rain and lightning storm without thunder. The lightning was very vivid and incessant, and seemed nearly overhead, but there was no sound but that of rain. We are near the crest of the Apennines, and the storm seemed to have gathered along that crest, having been preceded by a furious sirocco suddenly supervening on a north-west wind.

I have twice before witnessed the same phenomenon of electrical storms with vivid lightning overhead and no thunder. Both instances occurred on the abrupt edge of the Montenegrin highlands, where they fall off into the low, wide plains of the Scutari district, and where thunderstorms are more common than in any other country I have ever visited. On these nights we were encamped on the edge of the hill country, on broken rocky land, with much low scrubby vegetation, but the lightning was so incessant and vivid that we were able to walk about, choosing our way amongst the stones and shrubs as readily as by daylight, the intervals between the flashes being, I should judge, never more than a minute, while much of the time they seemed absolutely continuous, the landscape being visible in all details under a diffused violet light. Looking overhead the movements of the lightning were easily discernible, the locality of the discharges varying from one part of the vault to another in a manner which it was impossible to confound with the reflection of lightning from a distance. Like the storm of last night those were followed by copious rain, but not a single peal of thunder was heard during the whole night. W. G. STILLMAN

Cutigliano, Pistoiese Apennines, July 11

ALGÆ [1]

DR. BERTHOLD tells us in his preface that he was induced by his discovery of the processes of fructification in *Erythrotrichia obscura* to study the small but interesting group of the Bangiaceæ, in the knowledge of which so many gaps still existed. The Zoological Station at Naples afforded him every facility for carrying on his researches on these algæ, not only in what may be called their wild state, but also under cultivation. To these advantages may be added, although in an inferior degree, that of the use of a great number of dried specimens. The results of his two years' study are embodied in the work mentioned at the head of this notice.

The small group of algæ, now included by Dr. Berthold under the general name of Bangiaceæ, consists of the three genera, Bangia, Porphyra, and Erythrotrichia; under the last genus are included *Bangia ciliaris*, and *B. ceramicola* of Harvey ("Phyc. Brit.," Pls. cccxxii. and cccxvii.). To these genera may probably be added Goniotrichum.

The exact systematic position of these algæ has, from the fact that little was known of their fructification, been hitherto uncertain. While their red colour induced Cohn, Thuret, and Bornet to place them with the Florideæ; other algologists, among whom may be mentioned J. Agardh, Kützing, Harvey, and Zanardini, grounding their opinion on the structure of the vegetative thallus, have classed them with the Chlorosperms.

For the first information relative to the fructification of the Bangiaceæ, we are indebted to Derbès and Solier, who had discovered in *Bangia fusco-purpurea* and *B. lutea* two different kinds of fructification, namely, the "common spores" and antheridia. Then followed the researches of Nägeli, Thuret, and Janczewski on Porphyra. Janczewski had actually discovered and described the carpospores of Porphyra, to which he gave the name of

[1] I mention a hen as foster-mother because the ducklings can have no instinctive knowledge of any note of alarm or warning she may give.

[1] "Die Bangiaceen des Golfes von Neapel." Eine Monographie von Dr. G. Berthold. Fauna und Flora des Golfes von Neapel. (Leipzig: Wilhelm Engelmann, 1882.)

octospores; but he failed to interpret their true significance as reproductive organs, and laid down his pen under the firm conviction that the cystocarpic fruit was entirely absent in Porphyra. Thuret's representation of this kind of fruit proved that Janczewski was mistaken. Dr. Berthold mentions that he was fortunate enough to obtain by his researches at the Zoological Station at Naples satisfactory proof that the reproductive processes in the Bangiaceæ correspond exactly with those of the other Florideæ; he further states (p. 21) that they are true Florideæ, but that they undoubtedly occupy the very lowest position in the class.

The first part of the work describes at some length the structure of the vegetative thallus of each of the three genera. A minute description follows of the organs of fructification, namely, the tetraspores, cystocarps, and antheridia, and of the mode in which the cystocarps are fertilised. The fructification of all the genera is illustrated by a plate containing twenty-five figures. We have then an account of the germination of the spores and of their development into plants, followed by observations on the systematic position occupied by the Bangiaceæ and their relation to the Chlorosperms. To these are added descriptions of the genera and species, with a notice of the habitat and time of appearance of the several species.

This very interesting work concludes with some remarks on Goniotrichum, and short descriptions of the two species *G. elegans* (*Bangia elegans* of the "Phyc. Brit.," Pl. ccxlvi.) and *G. dichotomum*. MARY P. MERRIFIELD

GAUSS AND THE LATE PROFESSOR SMITH

IN the centenary notice of Gauss (NATURE, vol. xv. pp. 533-537) I more than once refer to notes placed in my hands by the late Prof. Henry Smith. These took the form of two MSS. (*A*), (*B*). The former of these I used in its entirety (p. 537), the latter I withheld, with Prof. Smith's sanction, on account of the length to which the article had already extended. Many mathematicians may now like to read these further criticisms on Gauss by such a kindred genius. R. TUCKER

We proceed to give brief references to some of the most important points which have caused a new epoch in certain branches of analysis to date from the publication of the "Disquisitiones Arithmeticæ," and from the researches with which, some years later, Gauss supplemented or further developed the theories contained in that work. It may be proper to premise that Gauss found the theory of numbers as Euler and Lagrange had left it. Of these the former had enriched it with a multitude of results, relating to diophantine problems, to the theory of the residues of powers, and to binary quadratic forms; the latter had given the character of a general theory to some at least of these results, by his discovery of the reduction of quadratic forms, and of the true principles of the solution of indeterminate equations of the second degree. Legendre (with many additions of his own) had endeavoured to arrange as much as possible of these scattered fragments of the science into a systematic whole in his "Essai sur La Théorie des Nombres." But the "Disquisitiones Arithmeticæ" was in the press when this important treatise appeared, and what in it was new to others was already known to Gauss.

The first section of the "Disquisitiones," "De Numerorum Augmentiâ in genere," occupies hardly more than four pages of the quarto edition, and is of the most elementary character. Nevertheless, the definition and the elementary properties of a congruence, which were for the first time given in it, have exercised an immense influence over all the branches of the higher arithmetic; an influence which is perhaps surprising when we remember that it is a question of notation only, and that (as Gauss has said himself in a letter to Schumacher) nothing can be done with this notation which cannot (though less conveniently) be done without it.

The second section, "De Congruentiis Primi Gradus," contains applications of the definition and of the elementary properties of congruences to linear congruences, and to systems of such congruences. The problems solved in it are of an elementary kind, and may be regarded as either well known, or as lying within the scope of what was well known, at the time of the publication of the "Disquisitiones Arithmeticæ."

The same remark applies to the third section, "De Residuis Potestatum," which, notwithstanding the immense advantage of clearness and simplicity obtained by the use of the congruential notation, may be said to lie almost wholly within the aid of ideas to be found in Euler's memoirs. The demonstration of the existence of primitive roots (a demonstration which Euler had failed in rendering rigorous), is, however, a very noticeable exception.

The fourth section—"De Congruentiis Secundi Gradus"—opens with an exposition of the elementary theorems relating to quadratic residues and non-residues; and so far we are still entirely within the ground already occupied by Euler. But the greater part of this section is occupied with a research which of itself alone would have placed Gauss in the first rank of mathematicians. "If p and q are positive uneven prime numbers, p has the same quadratic character with regard to p that q has with regard to p, except when p and q are both of the form $4n + 3$, in which case the two characters are always opposite instead of identical." This is the celebrated Fundamental Theorem of Gauss, known also as the law of quadratic reciprocity of Legendre. Gauss discovered it (by induction) in March, 1795, before he was eighteen; the proof given of it in this section he discovered in April of the year following. He cannot at the earlier date have been aware that the theorem had been already enunciated (though in a somewhat complex form) by Euler; and that Legendre had attempted, though unsuccessfully, to prove it in the *Mémoires of the Academy of Paris* for 1784. But the question to whom the priority of the enunciation is due is of even less moment than questions of priority usually are; for the discovery of the theorem by induction was easy, whereas any rigorous demonstration of it involved apparently insuperable difficulties. Gauss was not content with vanquishing these difficulties once for all in the fourth section. In the fifth section he returns to it again, and obtains another demonstration reposing on entirely different, but perhaps still less elementary, principles. In January of the year 1808 he submitted a third demonstration to the Royal Society at Göttingen; a fourth in August of the same year; a fifth and sixth in February, 1817. It is no wonder that he should have felt a sort of personal attachment to a theorem which he had made so completely his own, and which he used to call the gem of the higher arithmetic. His six demonstrations remained for some time the only efforts in this direction; but the subject subsequently attracted the attention of other eminent mathematicians, and several proofs, differing substantially from one another and from those of Gauss, have been given by Jacobi and Eisenstein in Germany, and by M. Liouville in France, the simplest of all perhaps being that which has been given by a Russian mathematician, M. Zeller, and which is of the same general character as the third proof of Gauss (see *Messenger of Mathematics*, vol. v. pp. 140-3, 1876). It would certainly be impossible to exaggerate the important influence which this theorem has had on the subsequent development of arithmetic, and the discovery of its demonstration by Gauss must certainly be regarded (indeed it was so regarded by himself) as one of his greatest scientific achievements.

The fifth section—"De Formis Æquationibusque Inde-

terminatis Secundi Gradus"—consists, as has been said with great truth by Dirichlet, of two distinct parts. Of these the first (Arts. 153-222) contains a complete exposition of the theory of binary quadratic forms, as far as it was known from the researches of Euler and Lagrange; although even these known results are completed in many respects and are exhibited from a new and independent point of view. The second part (Arts. 223-305) contains investigations which are entirely Gauss's own: the distribution of the classes of binary forms into genera; the determination of the number of ambiguous classes; the demonstration that only one-half of the genera possible *a priori* actually exist, and the proof of the fundamental theorem deduced from this result; a disquisition on ternary quadratic forms, introduced as a digression; the theory of the decomposition of numbers into three squares; the solution of indeterminate equations of the second degree in rational numbers; the determination of the mean number of the genera and classes; the distinction between regular and irregular determinants. Such is a brief list of the subjects treated of in these marvellous pages, each of which has been the starting-point of long series of important researches by subsequent mathematicians.

In the *Additamenta* to this section, Gauss intimates that he had succeeded in determining the relations between the determinant and the number of classes; and in a manuscript note he characteristically adds: "Ex voto nobis sic successit ut nihil amplius desiderandum supersit, Nov. 30–Dec. 3, 1800." It is remarkable that he should never have published the wonderful researches to which he here alludes. These researches first saw the light sixty-three years later in the second volume of the collected edition of his works; but the theorem to which they refer had in the interval been rediscovered and demonstrated by Lejeune Dirichlet. The demonstration of Dirichlet had been to a certain extent simplified by M. Hermite, and the form of demonstration found in Gauss's papers after his death approaches very nearly to that adopted by M. Hermite.

The sixth section contains some applications of arithmetical principles to various practical questions. Of these the first two are comparatively elementary, and relate to the resolution of fractions into simpler fractions, and to the conversion of vulgar into decimal fractions; the others consist in systematic methods of abbreviating certain tentative processes, such as the solution of quadratic congruences, the decomposition of numbers into their prime factors, the solution of certain indeterminate equations, &c. The methods of Gauss still remain the least unsatisfactory that have been proposed for the indirect treatment of these difficult problems, of which any direct solution seems impossible.

The seventh section, "De Æquationibus Circuli Sectiones Definientibus," is that which at once made the reputation of the "Disquisitiones Arithmeticæ." It is not too much to say that till the time of Jacobi the profound researches of the fourth and fifth sections were passed over with almost universal neglect. But the well-known theory of the division of the circle comprised in this section was received with great and deserved enthusiasm as a memorable addition to the theory of equations and to the geometry of the circle. One of Gauss's manuscript notes is interesting, "Circulum in 17 partes divisibilem esse geometrice, deteximus 1796, Mart. 30," because it shows that he was not yet nineteen when he made this great discovery. Even more remarkable, however, is a passage in the first article of the section (Art. 335), in which Gauss observes that the principles of his method are applicable to many other functions besides the circular functions, and in particular to the transcendents dependent on the integral $\int \frac{dx}{\sqrt{1-x^4}}$. This almost casual remark shows (as Jacobi long since observed) that Gauss, at the date of the publication of the "Disquisitiones Arithmeticæ," had already examined the nature and properties of the elliptic functions (the inverses of the elliptic integrals), and had discovered their fundamental property, that of double periodicity. This observation of Jacobi's is amply confirmed by the papers on elliptic transcendents now published in the third volume of Gauss's collected works.

The "Disquisitiones Arithmeticæ" were to have included an eighth section. This eighth section was at first intended to contain a complete theory of congruences, but subsequently Gauss appears to have proposed to continue the work by a more complete discussion of the theory of the division of the circle. Manuscript drafts on each of these subjects were found among his papers; the first of them is especially interesting, as it treats of the general theory of congruences from a point of view closely allied to that subsequently taken by Evariste Galois and by MM. Serret and Dedekind. This draft appears to belong to the years 1797 and 1798.

To complete this hasty outline of the arithmetical works of Gauss it only remains to mention (1) the remarkable geometrical interpretation of the arithmetical theory of positive binary and ternary quadratic forms, which will be found in his review (1831) of the work of L. Seeber ("Werke," vol. ii. p. 188), and (2) the two important memoirs on the theory of biquadratic residues (1825 and 1831). In the second of these memoirs Gauss introduces into arithmetic *complex* numbers of the form $a + bi$. He finds that in this complex theory every prime number of the form $4n + 1$ is to be regarded as composite, because, being the sum of two squares, *e.g.* $p = a^2 + b^2$, it is a product of two conjugate factors, $p = (a + bi)(a - bi)$. Thus the true primes of the complex theory may be defined to be the real primes of the form $4n + 3$, and the imaginary factors of real primes of the form $4n + 1$. Availing himself of this definition, Gauss discovered a theorem of biquadratic reciprocity between any two prime numbers, no less simple than the quadratic law, viz. "If p_1 and p_2 are two primary prime numbers, the biquadratic character of p_1 with regard to p_2 is the same as that of p_2 with regard to p_1."

Both this theorem of reciprocity itself and the introduction of imaginary integers upon which it depends are memorable in the history of arithmetic for the number and variety of the researches to which they have given rise.

It may perhaps seem remarkable that Gauss should have devoted so few memoirs to subjects of an algebraical character. If we except a comparatively unimportant paper on Descartes' rule of signs which appeared in *Crelle's Journal* in the year 1828, his only algebraical memoirs relate to the theorem that every equation has a root. Of this he gave no less than three distinct demonstrations, one in 1799, one in 1815, and one in 1816; the demonstration of 1799 was given in his first published paper—his dissertation as a candidate for the degree of Doctor of Philosophy in the University of Göttingen. This demonstration he repeated over again in 1849, with certain changes and simplification. There can be no question that these three demonstrations are prior to any other, though for various reasons those subsequently given by Cauchy have been justly preferred for the purpose of insertion in our modern text-books.

ANTHROPOLOGY IN AMERICA

WE cannot speak very highly of Prof. Otis T. Mason's "Account of Progress in Anthropology in the Year 1881," which was originally embodied in the Smithsonian Report for that year, and is now issued in a separate form. There is no comprehensive survey of the work done in this wide field during the period indicated, and the bibliography, of which the paper mainly consists, i

vitiated by too many subdivisions. These subdivisions are dealt with in the introduction, where a bewildering scheme of classification is proposed "in order to ascertain the opinion of anthropologists as to its merits." First the science is grouped under three main heads, indicated by terminations furnished by the three Greek words, γράφη, λόγος, and νόμος. Then each group is split up into thirteen minor divisions, yielding altogether thirty-nine distinct segmentations, and of course involving the whole subject in dire confusion. The student is expected, for instance, to distinguish between anthropography, anthropology, and anthroponomy; between pneumatography, pneumatology, and pneumatonomy; between hexiography, hexiology, hexionomy, and so on. However in the bibliography the author considerately limits himself to eleven headings, which will certainly be amply sufficient to try the patience of those who may have occasion to consult these alphabetical lists. Thus Nesbit's "Antiquity of Man" is entered under *Anthropogeny*, while Ameghino's "Antiquedad del hombre in La Plata" must be sought for in the section *Archæology*. These lists should obviously be fused together in one general catalogue, and all the nice subdivisions left to the fancy or ingenuity of the reader. To show their utter absurdity it may suffice to add that under the heading *Hexiology* there occurs the solitary entry—Buckley, "Climatic Influences on Mankind." Why, it may be asked in conclusion, does B. B. Redding's "Californian Indians and their Food," appear in the section *Technology?* The interests of science are not furthered by these minute subdivisions and barbarous nomenclatures, which are especially uncalled for in the case of a science whose broad divisions are already marked out with sufficient clearness and accuracy to serve all present practical purposes.

Prof. Mason has been much more usefully employed in the preparation of a series of "Miscellaneous Papers Relating to Anthropology," which also consist of reprints from the Smithsonian Report for 1881. Most of them have reference to the sepulchral mounds, earthworks, fortified lines, shell-heaps, and other remains of prehistoric and historic man so thickly strewn over the Mississippi basin, the eastern States and seaboard of North America. The great number and magnitude of these remains, their universal diffusion over an enormous area, and the character of the objects found in them, all tend to confirm the impression now generally entertained regarding the vast antiquity of man in the New World. On the other hand the views of those anthropologists who still attribute the old works to some superior pre-Columbian race of "mound-builders" distinct from the present aborigines are not strengthened by a more careful examination of these relics. Speaking of the mounds examined by him in Cass County, Illinois, Dr. J. F. Snyder remarks that "the intrinsic evidence of many prehistoric remains of this county sustains their claim to extreme antiquity; but no work or specimen of art of a former race has yet been found here above the capacity or achievement of the typical North American Indian. And in studying the life, habits, and burial customs indicated by these relics, I can see no necessity for ascribing them to the agency of a distinct or superior race, when they express so unmistakably the known status of Indian intellect" (p. 53). This conclusion is amply confirmed by the contents of the enormous shell-heap at Cedar Keys, Florida, which has been carefully examined by Mr. S. T. Walker. Here the pottery found in the successive layers, down to a depth of over twelve feet, shows a continuous advancement in the art from the rude heavy earthenware often mixed with coarse sand or small pebbles occurring in the lowest stratum, through the better finished and slightly ornamented types of the middle stage, to the delicate and beautifully ornamented specimens found near the surface. These objects thus show a progressive improvement *upwards*, not *downwards* as would be required by the theory of an extinct pre-Columbian civilised race, precursor of the present aborigines. A. H. K.

THE SIZE OF ATOMS[1]

III.

WE must then find another explanation of dispersion. I believe there is another explanation. I believe that, while giving up Cauchy's unmodified theory of dispersion, we shall find that the same general principle is applicable, and that by imagining each molecule to be loaded in a certain definite way by elastic connection with heavier matter,—each molecule of the ether to have, in palpable transparent matter, a small fringe, so to speak, of particles, larger and larger in their successive order, elastically connected with it,—we shall have a rude mechanical explanation, realisable by the notably easy addition of the proper appliances to the dynamical models before you, to account for refractive dispersion in an infinitely fine-grained structure. It is not 17 hours since I saw the possibility of this explanation; I think I now see it perfectly, but you will excuse me not going into the theory more fully under the circumstances.[2] The difficulty of Cauchy's theory has weighed heavily upon me, when thinking of bringing this subject before you. I could not bring it before you and say there are only four particles in the wave-length, and I could not bring it before you without saying there is some other explanation. I believe another explanation is distinctly to be had in the manner I have slightly indicated.

Now look at those beautiful distributions of colour on the screen before you. They are diffraction spectrums from a piece of glass ruled with 2,000 lines to the inch. And again look; and you see one diffraction spectrum by reflection from one of Rutherford's gratings, in which there are 17,000 lines to the inch on polished speculum-metal. The explanation by "interference", is substantially the same as that which the undulatory theory gives for Newton's rings of light reflected from the two surfaces, which you have already seen. Where light-waves from the apertures between the successive bars of the grating, reach the screen in the same phase, they produce light; there, again, where they are in opposite phases, they produce darkness

The beautiful colours which are produced, depend on the places of conspiring and opposing vibrations on the screen, being different for light waves of different wave-lengths; and it is by the measurements of the dimensions of a diffraction spectrum such as the first set you saw (or of finer spectrums from coarser gratings), that Fraunhofer first determined the wave-lengths of the different colours.

I have now, closely bearing on the question of the size of atoms, thanks to Dr. Tyndall, a most beautiful and interesting experiment to show you—the artificial "blue sky," produced by a very wonderful effect of light upon matter, which he discovered. We have here an empty glass tube—it is "optically void." A beam of electric light passes through it now; and you see nothing. Now the light is stopped and we admit vapour of carbon disulphide into the tube. There is now introduced some of this vapour to about 3 inches pressure, and there is also introduced, to the amount of 15 inches pressure, air impregnated with a little nitric acid, making in all rather less than the atmospheric pressure. What is to be illustrated here is the presence of molecules of substances, produced by the decomposition of carbon disulphide by the light.

[1] A lecture delivered by Sir William Thomson at the Royal Institution, on Friday, February 2. Revised by the Author. Concluded from p. 254.

[2] Farther examination has seemed to me to confirm this first impression; and in a paper on the Dynamical Theory of Dispersion, read before the Royal Society of Edinburgh, on the 5th of March, I have given a mathematical investigation of the subject.—W. T., March 16, 1883.

At present you see nothing in the tube; it still continues to be, as before the admission of the vapours optically transparent; but gradually you will see an exquisite blue cloud. That is Tyndall's "blue sky." You see it now. I take a Nicol's prism, and by looking through it I find the azure light, coming from the vapours in any direction perpendicular to the exciting beam of light, to be very completely polarised in the plane through my eye and the exciting beam. It consists of light-vibrations in one definite direction, and that, as finally demonstrated by Professor Stokes, it seems to me beyond all doubt, through reasoning on this phenomenon of polarisation,[1] which he had observed in various experimental arrangements giving minute solid or liquid particles scattered through a transparent medium, must be the direction perpendicular to the plane of polarisation.

What you are now about to see, and what I tell you I have seen through the Nicol's prism, is due to what I may call secondary or derived waves of light diverging from very minute liquid spherules, condensed in consequence of the chemical decomposing influence exerted by the beam of light on the matter in the tube, which was all gaseous when the light was first admitted.

To understand these derived waves, first you must regard them as due to motion of the ether round each spherule; the spherule being almost absolutely fixed, because its density is enormously greater than that of the ether surrounding it. The motion that the ether had in virtue of the exciting beam of light alone, before the spherules came into existence, may be regarded as being compounded with the motion of the ether relatively to each spherule, to produce the whole resultant motion experienced by the ether when the beam of light passes along the tube, and azure light is seen proceeding from it laterally. Now this second component motion, is clearly the same as the whole motion of the ether would be, if the exciting light were annulled and each spherule kept vibrating in the opposite direction, to and fro through the same range as that which the ether in its place had, in virtue of the exciting light, when the spherule was not there.

Supposing now, for a moment, that without any exciting beam at all, a large number of minute spherules are all kept vibrating through very small ranges[2] parallel to one line. If you place your eye in the plane through the length of the tube and perpendicular to that line, you will see light from all parts of the tube, and this light which you see will consist of vibrations parallel to that line. But if you place your eye *in* the line of the vibration of a spherule, situated about the middle of the tube, you will see no light in that direction; but keeping your eye in the same position, if you look obliquely towards either end of the tube, you will see light fading into darkness, as you turn your eye from either end towards the middle. Hence, if the exciting beam be of plane polarised light—that is to say, light of which all the vibrations are parallel to one line—and if you look at the tube in the direction perpendicular to this line and to the length of the tube, you will see light of which the vibrations will be parallel to that same line. But if you look at the tube in any direction parallel to this line, you will see no light; and the line along which you see no light is the direction of the vibrations in the exciting beam; and this direction, as we now see, is the direction perpendicular to what is technically called the plane of polarisation of the light. Here, then, you have Stokes's *experimentum crucis* by which he has answered, as seems to me beyond all doubt, the old vexed question—Whether is the vibration *perpendicular to*, or *in* the plane of polarisation? To show you this experiment, instead of using unpolarised light for the exciting beam, as in the previous experiment, and holding a small Nicol's prism in my hand and telling you what I saw when I looked through it, I place, as is now done, this great Nicol's prism in the course of the beam of light before it enters the tube. I now turn the Nicol's prism into different directions and turn the apparatus round, so that, sitting in all parts of the theatre, you may all see the tube in the proper direction for the successive phenomena of "light," and "no light." You see them now exactly fulfilling the description which I gave you in anticipation. If each of you had a Nicol's

[1] Extract from Professor Stokes' paper, "On the Change of Refrangibility of Light," read before the Royal Society, May 27th, 1852, and published in the *Transactions* for that date:—

"§ 183. Now this result appears to me to have no remote bearing on the "question of the direction of the vibrations in polarised light. So long as "the suspended particles are large compared with the waves of light, reflection "takes place as it would from a portion of the surface of a large solid immersed "in the fluid, and no conclusion can be drawn either way. But if the diameters "of the particles be small compared with the length of a wave of light, it "seems plain that the vibrations in a reflected ray cannot be perpendicular "to the vibrations in the incident ray. Let us suppose for the present, that "in the case of the beams actually observed, the suspended particles were "small compared with the length of a wave of light. Observation showed "that the reflected ray was polarised. Now all the appearances presented "by a plane polarised ray are symmetrical with respect to the plane of "polarisation. Hence we have two directions to choose between for the "direction of the vibrations in the reflected ray, namely, that of the incident "ray, and a direction perpendicular to both the incident and the reflected "rays. The former would be necessarily perpendicular to the directions of "vibration in the incident ray, and therefore we are obliged to choose the "latter, and consequently to suppose that the vibrations of plane polarised "light are perpendicular to the plane of polarisation, since experiment shows "that the plane of polarisation of the reflected ray is the plane of reflection. "According to this theory, if we resolve the vibrations in the [horizontal] "incident ray horizontally and vertically, the resolved parts will correspond "to the two rays, polarised respectively in and perpendicularly to the plane of "reflection, into which the incident ray may be conceived to be divided, and "of these the former alone is capable of furnishing a . . . . . ray reflected "vertically upwards [to be seen by an eye above the line of the incident ray, "and looking vertically downwards]. And, in fact, observation shows that, "in order to quench the dispersed beam, it is sufficient, instead of analysing "the reflected light, to polarise the incident light in a plane perpendicular to "the plane of reflection."

[2] In the following question of the recent Smith's Prize Examination at Cambridge (paper of Tuesday, Jan. 30, 1883), the dynamics of the subject, and particularly the motion of the ether produced by keeping a single spherule embedded in it vibrating to and fro in a straight line, are illustrated in parts (*a*) and (*d*):—

"8. (*a*) From the known phenomenon that the light of a cloudless blue "sky, viewed in any direction perpendicular to the sun's direction, is almost "wholly polarised in the plane through the sun, assuming that this light is "due to particles of matter of diameters small in comparison with the wave-"length of light, prove that the direction of the vibrations of plane polarised "light is perpendicular to the plane of polarisation.

"(*b*) Show that the equations of motion of a homogeneous isotropic elastic "solid of unit density, are

$$\frac{d^2\alpha}{dt^2} = (k + \tfrac{1}{3}n)\frac{d\delta}{dx} + n\nabla^2\alpha,$$

$$\frac{d^2\beta}{dt^2} = (k + \tfrac{1}{3}n)\frac{d\delta}{dy} + n\nabla^2\beta,$$

$$\frac{d^2\gamma}{dt^2} = (k + \tfrac{1}{3}n)\frac{d\delta}{dz} + n\nabla^2\gamma,$$

"where k denotes the modulus of resistance to compression; n the rigidity-"modulus; α, β, γ the components of displacement at (x, y, z, t); and

$$\delta = \frac{d\alpha}{dx} + \frac{d\beta}{dy} + \frac{d\gamma}{dz},$$

$$\nabla^2 = \frac{d^2}{dx^2} + \frac{d^2}{dy^2} + \frac{d^2}{dz^2}.$$

"(*c*) Show that every possible solution is included in the following:

$$\alpha = \frac{d\phi}{dx} + u, \quad \beta = \frac{d\phi}{dy} + v, \quad \gamma = \frac{d\phi}{dz} + w,$$

"where u, v, w are such that

$$\frac{du}{dx} + \frac{dv}{dy} + \frac{dw}{dz} = 0.$$

"Find differential equations for the determination of ϕ, u, v, w. Find "the respective wave-velocities for the ϕ-solution, and for the (u, v, w)-"solution.

"(*d*) Prove the following to be solutions, and interpret each for values of "$r\,[\sqrt{(x^2 + y^2 + z^2)}]$ very great in comparison with λ (the wave-length).

$$(1)\quad \begin{cases} \alpha = \dfrac{d\phi}{dx}, \quad \beta = \dfrac{d\phi}{dy}, \quad \gamma = \dfrac{d\phi}{dz} \\ \text{where } \phi = \dfrac{1}{r}\sin\dfrac{2\pi}{\lambda}\left[r - t\sqrt{(k + \tfrac{4}{3}n)}\right]. \end{cases}$$

$$(2)\quad \begin{cases} \alpha = 0, \quad \beta = -\dfrac{d\psi}{dz}, \quad \gamma = \dfrac{d\psi}{dy} \\ \text{where } \psi = \dfrac{1}{r}\sin\dfrac{2\pi}{\lambda}\left[r - t\sqrt{n}\right]. \end{cases}$$

$$(3)\quad \alpha = \left(\frac{2\pi}{\lambda}\right)^2\psi + \frac{d^2\psi}{dx^2}, \quad \beta = \frac{d^2\psi}{dx\,dy}, \quad \gamma = \frac{d^2\psi}{dx\,dz}.$$

prism in your hand, you would learn that when you see light at all, its plane of polarisation is in the plane through your eye and the axis of the tube; and I hope you all now perfectly understand the proof that the direction of vibration is perpendicular to this plane.

Now I want to bring before you something which was taught me a long time ago by Professor Stokes; and year after year I have begged him to publish it, but he has not done so, and so I have asked him to allow me to speak of it to-night. It is a dynamical explanation of that wonderful phenomenon called fluorescence or phosphorescence. The principle is mechanically represented by this model (described above

FIG. 10.—Diagram showing the different amplitudes of vibration of a row of particles oscillating in a period less than their least wave-period.

with reference to Fig. 2). A simple harmonic motion is, as you now see, sustained by my hand in the uppermost bar, in a period of about four seconds. You see that a regular wave-motion travels down the line of molecules represented by these circular disks on the ends of the bars; and the energy continually given to the top bar, by my hand, is continually consumed in heating the basin of treacle and water at the foot. I now remove my hand and leave the whole system to itself. The very considerable sum of kinetic and potential energies of the large masses and spiral springs, attached to the top bar, is gradually spent in sending the diminishing series of waves down the line, and is ultimately converted into heat in the treacle and water. You see that about half of the amplitude of vibration, and therefore three-fourths of the energy is lost in half a minute.

You will see on quickening the oscillation how very different the result will be. The quick oscillations which I now give to the top bar (the period having been reduced to about one and a half seconds), is incapable of sending waves along the line of molecules; and it is that rapid oscillation of the particles which, according to Stokes, constitutes latent or stored-up light. Remark now that when I remove my hand from the top bar, as no waves travel down the line, no energy is spent in the treacle; and the vibration goes on for ever (or, to be more exact, say for one minute) as you see, with *no loss* (or, to be quite in accordance with what we see, let me say scarcely any sensible loss). This is a mechanical model correctly illustrating the dynamical principle of Stokes' explanation of phosphorescence or stored-up light, stored as in the now well-known luminous paint; of which you see the action in this specimen, and in the phosphorescent sulphides of lime in these glass tubes kindly lent by Mr. De La Rue. (Experiments shown.)

Now I will show you Stokes' phenomenon of *fluorescence* in a piece of uranium glass. I hold it in the beam from the electric lamp dispersed by the prism as you see. You see the uranium glass now visible by being illuminated by invisible rays. The rays by which it is illuminated even before it comes into the visible rays are manifestly invisible so far as the screen receiving the spectrum is a test of visibility; because the uranium glass, and my hands holding it, throw no shadow on the screen. Also you see the uranium glass which I hold in my hand in the ultra-violet light, while you do not see my hand. I now bring it nearer the place where you see the air (or rather the dust in it) illuminated by the violet light: still no shadow on the screen, but the uranium glass in my hand glowing more brilliantly with its green light of very mixed constitution, consisting of waves of longer periods than that of the ultra-violet, which the incident light, of shorter period than that of violet light, causes the particles of the uranium glass to emit. This light is altogether unpolarised. It was the absolute want of polarisation, and the fact of its periods being all less than those of the exciting light, that led Stokes to distinguish this illumination, which you see in the uranium glass,[1] from the mere molecular illumination (always polarised partially if not completely, and always of the same period as that of the exciting light) which we were looking at previously in Dr. Tyndall's experiment.

Stokes gave the name of fluorescence to the glowing with light of larger period than the exciting light, because it is observed in fluor spar; and he wished to avoid all hypothesis in his choice of a name. He pointed out a strong resemblance between it and the old known phenomenon of phosphorescence; but he found some seeming contrasts between the two, which prevented him from concluding fluorescence to be in reality a case of phosphorescence.

In the course of a comparison between the two

[1] The same phenomenon is to be seen splendidly in sulphate of quinine. An interesting experiment may be made by writing on a white paper screen, with a finger or a brush dipped in a solution of sulphate of quinine. The marking is quite imperceptible in ordinary light; but if a prismatic spectrum be thrown on the screen, with the ultra-violet invisible light on the part which had been written on with the sulphate of quinine, the writing is seen glowing brilliantly with a bluish light, and darkness all round. The phenomenon presented by sulphate of quinine and many other vegetable solutions, and some minerals as, for instance, fluor spar, and various ornamental glasses, as a yellow Bohemian glass, called in commerce "canary glass" (giving a dispersed greenish light), had been discovered by Sir David Brewster (*Transactions*, Royal Society of Edinburgh, 1833, and British Association, Newcastle, 1838), and had been investigated also by Sir John Herschel, and by him called "epipolic dispersion" (*Phil. Trans.*, 1845). A complete experimental analysis of the phenomenon, showing precisely what it was that the previous observers had seen, and explaining many singularly mysterious things which they had noticed, was made by Stokes, and described in his paper, "On the Change of Refrangibility of Light" (*Phil. Trans.*, May 27, 1852).

phenomena (sections 221 to 225 of his 1852 paper), the following statement is given :—"But by far the most "striking point of contrast between the two phenomena "consists in the apparently instantaneous commencement "and cessation of the illumination, in the case of internal "dispersion when the active light is admitted and cut "off. There is nothing to create the least suspicion of "any appreciable duration in the effect. When internal "dispersion is exhibited by means of an electric spark, it "appears no less momentary than the illumination of a "landscape by a flash of lightning. I have not attempted "to determine whether any appreciable duration could "be made out by means of a revolving mirror." The investigation here suggested, has been actually made by Edmund Becquerel, and the question—Is there any appreciable duration in the glow of fluorescence ?—has been answered affirmatively by this beautiful and simple little machine before you, which he invented for the purpose. The experiment giving the answer is most interesting, and I am sure you will see it with pleasure. It consists of a flat circular box, with two holes facing one another in the flat sides near the circumference; inside are two disks, carried by a rapidly revolving shaft, by which the holes are alternately shut and opened; one open when the other is closed, and *vice versâ*. A little piece of uranium glass is fixed inside the box between the two holes, and a beam of light from the electric lamp falls upon one of the holes. You look at the other.

Now when I turn the shaft slowly you see nothing. At this instant the light falls on the uranium glass through the open hole far from you, but you see nothing, because the hole next you is shut. Now the hole next you is open, but you see nothing; because the hole next the light is shut, and the uranium glass shows no perceptible afterglow as arising from its previous illumination. This agrees exactly with what you saw when I held the large slab of uranium glass in the ultra-violet light of the prismatic spectrum. As long as I held the uranium glass there you saw it glowing; the moment I took it out of the invisible light it ceased to glow. The "moment" of which we were then cognisant, may have been the tenth of a second. If the uranium glass had continued to glow sensibly for the twentieth or the fiftieth of a second, it would have seemed, to our slow-going sense of vision, to cease the moment it was taken out. Now I turn the wheel at such a rate that the hole next you is open about a fiftieth of a second after the uranium glass was bathed in light; still you see nothing. I turn it faster and faster and it now begins to glow, when the hole next you is open about the two-hundredth of a second after the immediately preceding admission of light by the other hole. I turn it faster and faster and it glows more and more brightly, till now it is glowing like a red coal; further augmentation of the speed shows, as you see, but little difference in the glow.

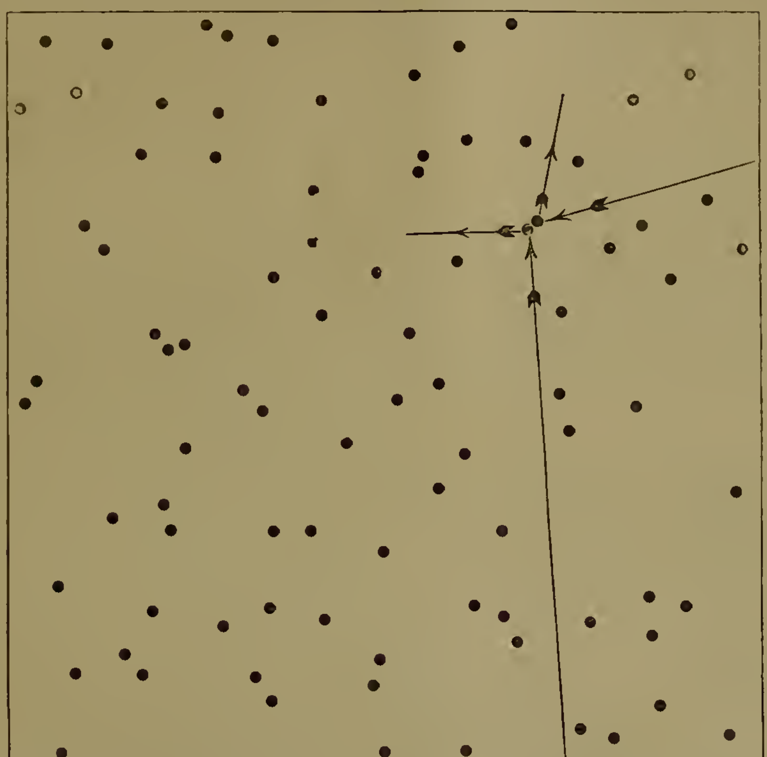


FIG. 11.—Diagram illustrating the number of molecules in a space of 1/10,000 of a centimetre square and 1/100,000,000 of a centimetre thick.

Thus it seems that fluorescence is essentially the same as phosphorescence; and we may expect that substances will be found, continuously bridging over the difference of quality between this uranium glass, which glows only for a few thousandths of a second, and the luminous sulphides which glow for hours, or days, or weeks after the cessation of the exciting light.

The most decisive and discriminating method of estimating the size of atoms, I have left until my allotted hour is gone :—that founded on the kinetic theory of gases. Here is a diagram (Fig. 11) of a crowd of atoms or molecules showing, on a scale of 1,000,000 to 1, all the molecules of air, of which the centres may, at any instant, be in a space of a square of 1/10,000 of a centimetre side and 1/100,000,000 of a centimetre thick. The side of the square you see in the diagram is a metre, and represents 1/10,000 of a centimetre. The diagram shows just 100 molecules, being 1/10,000 of the whole number of particles (10^6) in the cube of 1/10,000 centi-

metre, or all the molecules in a slice of 1/10,000 of the thickness of that cube. Think of a cube filled with particles, like these glass balls,[1] scattered at random through a space equal to 1,000 times the sum of their volumes. Such a crowd may be condensed (just as air may be condensed) to 1/1,000 of its volume; but this condensation brings the molecules into contact. Something comparable with this may be imagined to be the condition of common air of ordinary density, as in our atmosphere. The diagram with size of each molecule, which, if shown in it to scale, would be 1 millimetre (or too small to be seen by you), to represent an actual diameter 1/10,000,000 of a centimetre, represents a gas in which a condensation of 1 to 10 linear, or 1 to 1,000 in bulk, would bring the molecules close together.

Now you are to imagine the particles moving in all directions, each in a straight line until it collides with another. The average length of free path is ten centimetres in our diagram, representing 1/100,000 of a centimetre in reality. And to suit the case of atmospheric air of ordinary density and at ordinary pressure, you must suppose the actual velocity of each particle to be 50,000 centimetres per second; which will make the average time from collision to collision 1/5,000,000,000 of a second.

The time is so far advanced that I cannot speak of the details of this exquisite kinetic theory, but I will just say that three points investigated by Maxwell and Clausius—viz. the viscosity, or want of perfect fluidity of gases; the diffusion of gases into one another; and the diffusion of heat through gases—all these put together give an estimate for the average length of the free path of a molecule. Then a beautiful theory of Clausius enables us from the average length of the free path to calculate the magnitude of the atom. That is what Loschmidt has done,[2] and I, unconsciously following in his wake, have come to the same conclusion; that is, we have arrived at the absolute certainty that the dimensions of a molecule of air is something like that which I have stated.

The four lines of argument which I have now indicated, lead all to substantially the same estimate of the dimensions of molecular structure. Jointly they establish, with what we cannot but regard as a very high degree of probability, the conclusion that, in any ordinary liquid, transparent solid, or seemingly opaque solid, the mean distance between the centres of contiguous molecules is less than the 1/5,000,000, and greater than the 1/1,000,000,000 of a centimetre.

To form some conception of the degree of coarse-grainedness indicated by this conclusion, imagine a globe of water or glass, as large as a football,[3] to be magnified up to the size of the earth, each constituent molecule being magnified in the same proportion. The magnified structure would be more coarse-grained than a heap of small shot, but probably less coarse-grained than a heap of footballs.

[1] The piece of apparatus now exhibited, illustrated the collisions taking place between the molecules of gaseous matter, and the diffusion of one gas into another. It consisted of a board of about one metre square, perforated with 100 holes in ten rows of ten holes each. From each hole was suspended a cord five metres long. To the lower end of each cord in five contiguous rows, there was secured a blue coloured glass ball of four centimetres diameter; and similarly to each cord of the other five rows, a red coloured ball of the same size. A ball from one of the outer rows was pulled aside, and, being set free, it plunged in amongst the others, causing collisions throughout the whole plane in which the suspended balls were situated.

[2] *Sitzungsberichte* of the Vienna Academy, Oct. 12, 1865, p. 395.

[3] Or say a globe of 16 centimetres diameter.

SMOKE ABATEMENT

AN important meeting was held in the Egyptian Hall of the Mansion House on Monday last, to take further steps towards the abolition, or at all events the reduction, as far as possible, of the smoke nuisance. The Lord Mayor presided, and the following among others were present:—The Duke of Northumberland, the Duke and Duchess of Westminster, Sir William Siemens, Sir Frederick Abel, Sir Lyon Playfair, M.P., Sir Frederick Pollock, Sir T. Spencer Wells, Mr. George Cubitt, M.P., Dr. Farquharson, M.P., Col. Makins, M.P., Capt. Galton, Mr. Edwin Chadwick, C.B., Mr. Ernest Hart, Mr. C. Waring, the Hon. Rollo Russell, General Lowry, C.B., Mr. George Shaw (chairman of the City Commission of Sewers), Mr. W. R. E. Coles, Mr. W. Chandler Roberts, of the Royal Mint, and Mr. Gregory, Master of the Clothworkers' Company.

The proceedings were opened by the reading of a Report, which has been carefully prepared by the Council, detailing the steps which have already been taken, and giving particulars of the exhibitions of last year in London and Manchester. The Report also deals with the work which has been done regarding the chemical composition of smoke by Prof. Chandler Roberts, and the many tests of coal made by Mr. Clark. In this important investigation, attention was called to the fact that a very great discrepancy exists between the heating efficiency of various types of grates, stoves, furnaces, and the like. In some forms of grate, for instance, only 22 per cent. of the total heat is utilised, whilst others require nearly three tons of coal to do the same work which other stoves manage to get out of one ton.

The Council desired to report that, so far as they had been able to ascertain, the most marked benefit resulting from the movement had been in the increased use of gas and coke for heating purposes. The improvement in gas-heating apparatus had been considerable, and the use of coke had been greatly facilitated by its being supplied to the public in more convenient sizes than formerly, and by the introduction of firebrick or other slow-conducting substances used in the fireplaces for burning it. The use of smokeless coal had also been extended in the metropolis; but the Council found that the description of such coal supplied was in a large number of instances unsuitable or inferior, and from that cause, coupled with the fact that smokeless coals were not generally supplied by coal merchants, there had not been, so far, any very marked increase in its consumption. Marked improvement had, however, been made in open grates and stoves for burning that description of coal, and one firm of manufacturers, who brought out a cheap stove at the South Kensington Exhibition, had sold upwards of 14,000 during the past two years; and they remarked that the public seemed ready to burn non-smoky coal if proper stoves for using it were offered at a reasonable price. Appliances for improving the draught of chimneys had also been introduced lately, and that tended to facilitate the use of smokeless coal. The Council had examined the present state of the administration of the law for the suppression of smoke, and they considered that in view of the enormous extension of buildings and factories in London and large towns, and in view also of the evidence that smoke could be to a great extent, if not entirely, avoided, the scope of legislative enactments for abating smoke should be extended and their provisions duly enforced.

One part of the Report deals with a matter to which we attach the greatest importance. It is suggested that there should be some place which the public can visit and where they may examine any apparatus that is approved of, or which they may wish to purchase; but above this it is pointed out that a place is requisite where scientific, chemical, and other tests may be made for the information of the public generally, but especially for the help of inventors and manufacturers who may wish to try new suggestions. The Report also suggests that in connection with this there should be some place for discussion and public lectures, for the general advancement and diffusion of knowledge touching smoke abatement. The third proposal is certainly the most doubtful one, but the

first and second are so important that the less time that is lost in starting such an institution the better; and we are glad to learn that towards its foundation the Duke of Westminster has promised 500*l.*, Mr. C. Waring 100*l.*, and Mr. Cubitt 100*l.*

The most important speech, perhaps, was that made by the Duke of Westminster, in moving the adoption of the Report. He pointed out that we are face to face with a very gigantic evil—an evil not only gigantic in itself, but, considering the enormous yearly increase of 40,000 in the population, one of a very alarming character. Therefore it was necessary that some steps should be taken to abate, if not to entirely do away with, that monstrous evil, which affected the health and vitality of the inhabitants of the metropolis. They were all aware of the evil effects of smoke, and how far worse it became when mixed with fog, but they believed that it was an evil which might be considerably modified if not entirely prevented. They had indisputable authority for saying that smoke was very wasteful and destructive. The waste in London alone amounted to one million yearly, and the waste in the country must be taken in proportion to that in the metropolis. They had also the highest authority for informing the public that the evil affected the health of those who lived under the canopy of smoke. Its effect on public buildings was also most destructive, and Mr. Shaw-Lefevre had said that to repair the damage done by its agency to the Houses of Parliament alone involved an expenditure of 2500*l.* per annum, and there could be no greater curse and bane to the metropolis than that smoke nuisance. The object of the meeting was to impress upon the public the importance of the subject. The Smoke Nuisance Act had been useful in the past, and could be made more efficacious in the future if its provisions were more strenuously enforced. Quoting from the correspondence which had taken place between the Home Office and the Association upon the subject, the speaker said that the Home Secretary had stated that in the majority of cases the fines inflicted were far less in amount than had been contemplated by the Act. That was not a right state of things, and efforts should be made to remedy it as soon as possible; and it was not unreasonable to suppose that with a proper enforcement of the law a check to a certain extent might be put upon the nuisance. After some other observations, his Grace concluded by moving the adoption of the Report.

Sir Spencer Wells and Sir Frederick Abel spoke in favour of the Duke of Westminster's proposal, which was carried unanimously.

The next resolution was moved by the Duke of Northumberland, and was to the following effect:—"That the period has now arrived at which systematic inquiry is desirable into the application of the resources of technical science for the abatement of smoke now largely produced in industrial processes and in the heating of houses, as well as into the operation of the existing laws for smoke abatement; and that the Council of the National Smoke Abatement Institution be requested to urge upon the Government the desirability of appointing a Royal Commission for the purpose."

This was seconded by Sir Wm. Siemens and carried.

We are glad to see that it was acknowledged that the stated objects of the Smoke Abatement Institution, and the success which has attended its past efforts, had established a claim not only to the support of the meeting, but to that of the City of London and other great cities and towns.

We must congratulate the Council of the new institution upon the energy which they are displaying, and we believe that in a few years the success they will then have met with will lead one to hope that in process of time the smoke nuisance which kills its tens of thousands annually, and makes life in a great city like London almost unbearable, will to a certain extent be done away with.

NOTES

GREAT efforts are being made by the Council of the Society of Arts and its chairman, Sir William Siemens, who has again been elected to this office, to make their *conversazione*, to be held on the 25th inst. at the Fisheries Exhibition, a great success. The fountains are to be illuminated by coloured fires, and the gardens, as well as the Exhibition Buildings, will be lighted by the electric light. The band of the 6th Thuringian Regiment of German Infantry will perform in the building.

A MEETING which may have an important result upon science and art instruction in this country has been inaugurated at Manchester. An association has been established to effect the general advancement of the profession of science and art teaching by securing improvements in the schemes of study and the establishment of satisfactory relations between teachers and the Science and Art Department, the City and Guilds of London Institute, and other public authorities. It proposes also to collect such information as may be of service to teachers professionally, and it will endeavour by constant watchfulness to advance the status and material interests of science and art teachers in all directions. The president of the new Association is Prof. Huxley, and the vice-presidents are Dr. H. E. Roscoe, Mr. Norman Lockyer, Prof. Boyd Dawkins, Prof. Gamgee, Prof. Ayrton, Prof. Silvanus Thompson, Dr. John Watts, Mr. S. Leigh-Gregson, Mr. John Angell, Mr. W. Lockett Agnew, Mr. C. M. Foden, and Mr. J. H. Reynolds. Mr. W. E. Crowther, of the Technical School and Mechanics Institution, Manchester, is the Honorary Secretary, and all communications should be addressed to him, especially by those who are desirous of forming affiliated unions in other districts. We believe that branches are already being established at Newcastle-upon-Tyne and Liver pool.

DR. J. H. GILBERT, F.R.S., has been elected a Corresponding Member of the Institute of France (Academy of Sciences).

THE treasurer of the Darwin Memorial Fund has received through Dr. Elforing of Helsingfors a cheque for 94*l.* 4*s.*, that being the amount collected in Finland as a contribution to the memorial. That so large an amount should have been collected in so small a country is only an additional proof of the ready recognition which the great works of Darwin have received in other countries as well as our own. The fund now amounts to 3300*l.*

THE Lick Observatory, we learn from *Science*, has made much progress during the past year. The dome for the small equatorial has now been finished, and is certainly the most complete and convenient one of its size in America. The building of the observatory in which the great thirty-six-inch equatorial is to be placed is also progressing. The walls of the main building are half completed, and the cellar for the dome has been excavated. The four-inch transit-house and the buildings for the photoheliographs have been in working order now for some time, as they were used in a successful observation of the transit of Venus last December. In a few weeks the building for the meridian circle will be commenced, as well as a house for the astronomers and buildings to contain the appliances for moving the dome, and for the general heating and lighting of the observatory. Two brick reservoirs for spring water, the one containing 83,000 gallons, the other 20,000 gallons, have been constructed, and another reservoir to contain 83,000 gallons of rain-water will shortly be commenced. The roads have been extended. Some of the original arrangements of the observatory buildings, which were only provisional, have now been replaced by more substantial and permanent structures, and by the end of the season great progress will have been made.

Science announces the death last month of Stephen Alexander, Professor Emeritus of Astronomy at Princetown. He was educated at Union College, where he graduated in 1824. In 1840 he was appointed Professor of Astronomy at Princetown, and more recently he received a Professorship of Mechanics. It was as an astronomer, however, that he was most generally known.

We have been asked by the local secretaries of the Meeting ot the British Association for the Advancement of Science to be held at Southport in September next to call the attention of those who have in their possession scientific instruments, curiosities, and other objects of special or artistic interest, to the fact that there will be an exhibition of such articles in connection with the meeting of the Association. Intending exhibitors and others interested in this matter should communicate with Mr. Ch. de Wechmar Stoess, the Hon. Sec. *Conversazione* Committee, and Mr. Alfred Morgan, the Hon. Sec. for Exhibits, immediately.

The steamer *Pola* has just called at Reikjavik, in Iceland, on her way to Jan Mayen, to bring away the Austrian observation party wintering there. Towards the end of the present month the Swedish gunboat *Urd* will proceed to Spitzbergen to relieve the Swedish party wintering there. It is reported, both from Iceland and Norway, that the state of the ice in the Arctic seas is very favourable to navigation.

The *Sophia*, Baron Nordenskjöld's vessel, left Rödefjord, Iceland, for Ivigtuk, in Greenland, on June 10, leaving Count Strömfelt and Drs. Arpi and Flink behind to pursue geological and botanical researches there.

A letter from M. Thouard, the well-known French traveller, dated Santiago (Chili), states that he heard from Chiriguanos Indians that a part of the Crevaux party were still prisoners of the Tobas tribe. M. Thouard will try to assist his countrymen.

Mr. Crookes and Professors Odling and Tidy have lately given in their Report on the composition and quality of London water during 1882 to the Local Government Board. In that year they examined 2110 samples of water drawn in nearly equal proportions from the mains of all the seven London Companies; testing generally seven samples daily by their colour according to the registers of the colour-meter, by the quantity of free oxygen and ammonia contained in them, by the amount of oxygen required for oxidation of the organic matter present in them, by their proportions of organic carbon and nitrogen, of nitrates and chlorine, and by their initial hardness in degrees of Clark's scale. The results exhaustively set forth in numerical tables are further illustrated by seven diagrams, in each of which three wave-lines represent the fluctuations throughout the year of discoloration, of the proportion of organic carbon, and of the amount of oxygen required to oxidise the organic matter of the water of the London Company in question. These diagrams show to the eye what the statistics confirm, the remarkable parallelism existing between the degree of discoloration, the amount of organic carbon present in the water as determined by combustion, and the amount of oxygen requisite to oxidation of the organic matter as determined by permanganate. The Report altogether would seem to reflect most favourably on the quality of London water. Throughout the whole year the water of the New River Company as determined by the samples was, without exception, "clear, bright, and well filtered," a character supported by analyses of other kinds, and in only a few cases in the samples of the other Companies was the water describable as "turbid," "slightly turbid," or "very slightly turbid." For the nine months from February to October 1882 the organic matter in the water of all the London Companies is estimated at ·137 per 100,000, and the highest monthly mean for the same period at ·181 per 100,000. There is, however, one important factor in the question with which chemical analysis cannot directly cope, the comparative innocuousness, namely, of the organic matter present in the water according as it is of vegetable origin, or its comparative virulence according as it is of animal origin. As Prof. Huxley, in a lecture in 1880 to the Chemical Society, said, water as regards chemical analysis may be perfectly unobjectionable, and yet as regards its operation on the human body deadly as prussic acid.

We have received advanced copies of the following books from the Literary Superintendent of the Fisheries Exhibition:—"British Marine and Freshwater Fishes," by W. Saville Kent, F.L.S., F.Z.S.; "Zoology and Food Fishes," by George Bond Howes, Demonstrator of Biology at the Normal School of Science; "On the Capture of Salmonidæ and the Acclimatisation of Fish," by Sir James Ramsay Gibson Maitland, Bart.; and "The Fishery Laws," by Frederick Pollock. We must congratulate the Commissioners of the International Fisheries Exhibition on their activity, and on their care for the scientific aspect of the specimens in their collection. There are two kinds of books published by the Exhibition authorities. Reports of papers read at the conferences and the important discussions which have followed their reading are published, and other books are written in explanation of the exhibits and other subjects bearing upon fish and fish culture.

There is an interesting article on "The Import Duty on Scientific Journals" in *Science* for June 29. The writer ventures to suggest that at its next meeting, the American Association for the Advancement of Science should appoint a committee to draw up a definite list of those foreign technical journals of mathematics, physics, chemistry, mineralogy, geology, geography, botany, zoology, physiology, and ethnology which do not compete with similar enterprises of publishing firms in the United States, and urge Congress to pass a special Act putting these journals on the free list. The article goes on to say that, if a suitable Bill were drawn up, there is little doubt that some member of Congress could be found to introduce it, and if framed so that it touched no publisher's pocket, and vigorously supported by the scientific influence of the country, it would certainly become law.

The earthquake at Voss in Norway on June 13, reported in Nature last week (p. 233), was felt over the entire district between Bergen and Aalesund, but most severely in the well-known Dalsfjörd. A further shock was felt over the same district on June 15 at 1.50 p.m., and some people assert that another followed at about 11 a.m. on the following day.

On the evening of the 2nd inst. a terrific cyclone passed over Stockholm. Its course was north-west to north-east. Houses were unroofed, trees uprooted, and a number of people thrown down, while not a shred of canvas was left on the masts of the vessels in the harbour. Barely a mile from the track of the cyclone there was almost a perfect calm.

We learn that the Dutch Government have decided not to grant the sum of 30,000 guilders which Baron Nordenskjöld claims as the discoverer of the North-East Passage. The decision is founded on the motive which led the States General in 1596 to offer this award, viz. to find a passage of *commercial* value to the nation; Baron Nordenskjöld having, however, discovered what may be termed a purely *scientific* one, the award, it is argued, has not been earned. As several reasons have been advanced for this claim made by the gallant Swedish explorer, we do not think we err when we assert that it was his intention to have expended the sum in the interest of science, viz. on an expedition to the Arctic regions.

A State paper recently issued by the Minister of Public Works in France contains some interesting details on the French

mineral waters. There are 1027 sources which are worked. Of these 319 are sulphurous, 357 alkaline, 136 iron, and 215 salt; 386 are cold, that is to say, they do not exceed 15° C. in temperature, and 641 are thermal. They are distributed as follows: —Puy-de-Dôme, 94; Ardèche, 77; Vosges, 76; Ariège et Pyrénées Orientales, 69; Hautes-Pyrénées, 64. The paper also states the number of visitors to these different waters. It appears that the Hautes-Pyrénées are the most frequented. During the past year this department alone has had 44,476 visitors, thus distributed:—Puy-de-Dôme, 18,619; l'Alliers, 16,430; la Haute-Garonne, 14,230; les Landes, 12,954. The water flowing from all the 1027 sources is estimated at 46,412 litres per minute.

THE Chevalier Frédéric Franchetti, engineer at Leghorn, has referred M. de Parville to a curious passage in Galileo's "Dialogues" touching a possible early origin of the electric telegraph. In the dialogue Sagrado says that he calls to mind a man who wanted to tell him a secret which would give him the power by means of a certain sympathy of magnetised wires to speak to any one two or three thousand miles off. The bargain however fell through, as the inventor would not try any shorter distance, and Sagrado declined to go to Cairo or Muscovy to try the experiment. The story is told in the last number of the *Revue Scientifique*. The reference given is p. 97 of the first day, Leghorn Edition, 1874.

THE Executive Committee of the International Fisheries Exhibition has published a penny plan and tour as a complete guide to the leading and most interesting features of the Exhibition, which we think will prove useful.

WE have some very interesting figures before us comparing the different modes of illumination in respect to the amount of products of combustion:—

Light of 100 candles.	Products per hour. Water vapour, kilos.	Carbonic acid in cubic metres.	Heat in calories.
Electric lamp, arc	0	0	57–158
,, ,, incandescent ...	0	0	290–536
Gas, Argand burner	0·86	0·46	4860
Lamp, petroleum, flat flame...	0·80	0·95	7200
,, colza oil	0·85	1·00	6800
Candle, paraffin	0·99	1·22	9200
,, tallow	1·05	1·45	9700

These we think are quite sufficient to show the great supremacy of electric lighting over all other methods of illumination when considered as a matter of health.

WE learn from *Naturen* that a hitherto unknown form of the potato disease, which had been making slow but steady progress near Stavanger during the last ten or twelve years, has recently begun to show increased energy. The stalk of the plant is the part affected, and here Herr Anda has discovered small white fungoid growths, which after a time assume a greenish, and finally a black, colour, after attaining the size of a small bean. While the fungus is rapidly increasing at the expense of the plant, the interior of the stem is first reduced to a pulpy condition, and next shrivelled and hollowed out, until nothing remains but a mere outer shell, which breaks down on being touched. When the ripe black germs of the fungus have remained in the earth through the winter, they are found after the return of the next year's warmth to have developed small stalked fruits filled with minute spores, which penetrate into the young plants before they appear above the ground. The end of July or beginning of August is the time when the ravages of the fungus are most conspicuous, and at those periods whole fields of potato plants are often rapidly reduced to the condition of withered straw.

WE have received from the Minister of Mines of New South Wales the report of the Chief Inspector of Mines for the year 1882. Besides the usual statistics, a great part of the report is occupied with suggestions for the improvement of the present law of the colony for preventing accidents to workmen in mines.

FROM a comparison between the lists of birds observed at Saltdalen in Norway by the ornithologist Sommerfelts, from 1805 to 1825, and those which are now found in the district, it would appear, according to Herr Hageman of the Norwegian Forest Department, that the smaller singing birds are much more largely represented now than formerly. The ortolan and crossbill, *Hirundo urbica* and *rustica*, the common sparrow and the chaffinch, which are now abundant, were then unknown in the district, while the common sparrow was only observed on one occasion by Sommerfelts. Herr Anda ascribes the present increase in numbers and species to the better cultivation of the land and the clearing of the fir-woods.

THE additions to the Zoological Society's Gardens during the past week include a Kinkajou (*Cercoleptes caudivolvulus*) from South America, presented by Mr. H. V. Brackenbury; a Syrian Fennec (*Canis famelicus* ♀) from North Africa, presented by Mr. J. H. James; a Blau-bok (*Cephalophus pygmæus* ♀) from South Africa, presented by Mr. Ernest Honey; a Slender-billed Cockatoo (*Licmetis tenuirostris*) from Australia, presented by Mrs. A. C. Biddle; an Earl's Weka Rail (*Ocydromus earlii*) from North Island, New Zealand, presented by Mrs. Wilson; two Wood Owls (*Syrnium aluco*), British, presented by Mr. J. Metcalfe; two Black Guillemots (*Uria grylle*) from Ireland, presented by Mr. H. Becher; a Vervet Monkey (*Cercopithecus lalandii* ♂) from South Africa, a Moor Macaque (*Macacus maurus* ♂), a Bonnet Monkey (*Macacus radiatus* ♀) from India, two Common Snakes (*Tropidonotus natrix*, var.), European, a Spotted Cavy (*Cœlogenys paca*), two Hairy-rumped Agoutis (*Dasyprocta prymnolopha*) from Guiana, deposited; a Black Howler (*Mycetes caraye* ♂) from Brazil, purchased; a Japanese Deer (*Cervus sika* ♂), a Burrhel Wild Sheep (*Ovis burrhel* ♀), born in the Gardens.

OUR ASTRONOMICAL COLUMN

THE TOTAL SOLAR ECLIPSE OF AUGUST 28-29, 1886.—This great eclipse is a return of that of August 17-18, 1868, which was extensively observed in the Bombay and Madras Presidencies and in other parts of its track from Aden to Torres Straits. In 1886 the track of the central line is mainly over the Atlantic Ocean, and at that portion of it where the duration of totality is longest it will not be observable on land. It is therefore of interest to examine the possible conditions of observation. In deducing the results which follow, the places of the sun and moon have been taken from the *Nautical Almanac*, where Newcomb's corrections to Hansen's Lunar Tables are introduced. As will be seen from the Ephemeris, the central eclipse commences in longitude 79° 46′ west of Greenwich, and latitude 9° 48′ north, off Colon, in the isthmus of Panama, thence running in the direction of the Windward Islands across the northern parts of New Grenada and Venezuela; passing over the Island of Grenada, it traverses the Atlantic, and meets the coast of Africa in the Portuguese possessions, not far from St. Philip de Benguela, and crossing South Africa to Sofala, it ends on the east coast of Madagascar. At Cartagena the duration of totality is 3m. 2s., with the sun at an altitude of 5°; at Maracaybo the duration is 2m. 57s., with the sun 9° above the horizon. The southern extremity of the Island of Grenada will have the most advantageous conditions for observation, having regard to length of totality and accessibility. The total eclipse begins there at 7h. 11m. 0s. a.m. on August 29, and continues 3m. 42s., the sun being at an altitude of 20°; at the northern extremity of the island the length of total eclipse is about five seconds less. In Carriacou, the principal island in the Grenadines, the duration of totality is 3m. 21s.; at the northern point of Tobago it is 1m. 51s. On the Atlantic, where the sun is on the meridian at the middle of the eclipse, or in longitude 14° 27′ west and latitude 2° 57′ north,

totality, according to the elements we have adopted, will continue for 6m. 31s. Near St. Philip de Benguela, on the central line, we find the sun will be hidden for 4m. 23s., but the locality will hardly attract observers. It would rather appear that we must look for observations of this eclipse to the Windward Islands only. The small island of Blanquilla is close upon the central line, but the sun has of course a less altitude there than in Grenada.

The eclipse of August 7–8, 1850, one of the same series, was observed in the Sandwich Islands, the whole track of totality lying on the Pacific.

TEMPEL'S COMET, 1873, II.—According to M. Schulhof's corrected elements of this comet's orbit, which assigned a period of revolution of 5·200 years, at the last appearance in 1878, the next perihelion passage, neglecting the effect of perturbation, which can hardly be very material during the present revolution, may take place about November 19 under circumstances that will render observations difficult if they are practicable at all. Assuming the comet to be in perihelion on November 19·5 G.M.T., we should have about the following positions:—

1883.	R.A. h. m.	N.P.D. ° ′	Log. Δ.	Log. r.	Intensity of light.
Oct. 18·5	16 38·8	108 59	0·275	0·142	0·146
Nov. 19·5	18 33·1	114 1	0·286	0·127	0·149
Dec. 21·5	20 36·4	113 0	0·313	0·142	0·124

In 1873, under an intensity of light of 0·385, the comet was the *extremum visibile*, in a fine sky, with a 7-inch refractor.

SOLID AND LIQUID ILLUMINATING AGENTS

THE *Journal of the Society of Arts* publishes in a recent number an interesting lecture given by Mr. Leopold Field, F.C.S., on "Solid and Liquid Illuminating Agents." Mr. Field not only deals with the chemistry of these bodies, but he gives also a most interesting account of the means of lighting in use among the ancients, to which a brief reference may be made.

The earliest known method of illumination was in all probability that of the torch, formerly used largely in northern countries, and doubtless still furnishing the Lapp and the Finn with their light. The torch is cut from the pitch pine, and around it clings the exuded resin. When lighted it burns with a large red flame, producing a great deal of smoke. Used for cooking purposes a brand might get saturated with fat, so that it would burn longer without consuming its own fibre.

This, as pointed out by Mr. Field, was the old method of lighting. Substituting for their brand a piece of rope and saturating this with pitch or resin we get the modern link, connecting us on foggy days with the old modes of lighting. The work link itself, and probably also the idea, comes from the Greek λύχνος, or perhaps the Latin *luchnus* (Cicero) as the German *fackel* comes from the Greek φάκελος (faggot), a bundle of sticks—after, a torch. But our own word torch is more evidently from the Latin *tartitium*, a twisted thing, now however more properly applicable to the link. Our pine torch too is obtained from the Roman *tædæ*—slips of the tæda, or Italian pitch pine, that being the usual outdoor light of Rome; whilst *Funalia*, which Virgil tells us were used to light Dido's palace—

> "—— dependent lychni laquearibus aureis
> Incensi, et noctem flammis funalia vincunt."

is evidently from *funis*, a rope. Their composition was rather that of a finer kind of link, flambeaux, consisting of a centre of oakum, which was surrounded by alternate layers of rosin and crude beeswax, outside of all being a bleached coating of the latter. They were more costly than other kinds of torch, and giving a less smoky light were more generally employed for the illumination of halls, staircases, &c.

At what date this torch fell into disuse is a question which cannot be definitely answered, as in old times words applied to various illuminating agents, which have a very fixed and definite meaning in our day, were then interchangeable. In our translation of the Scriptures "candle" and "candlestick" are used indiscriminately with lamp, and, did we not know that candles proper and candlesticks were unknown at this period, we might infer that they were both in use. An explanation of this use of the words "candle" and "candlestick," however, is found in the fact that the Latin *candelabrum* and the Greek λυχνία, Latin *luchnuchus* (Cicero), meant "lampstand."

Again, in Matt. xxv. 1–5, where we find the parable of the Virgins, the word λυχνὸς is rendered lamp. But a study of the etymology of the words shows that they are derived from roots signifying to shine or burn—as *candela*, κανδήλα, akin to *candeo*, to shine (Persian, *kandeel;* Sans., *kan*)—λύχνος, *lucerna*, from *lux*, light (Sans. *lôk*), λάμπας, *lampas*, probably connected with *lame*, and the Hebrew *lapad*, to shine.

But although it is doubtful at what date the torch fell into disuse, it may be concluded that it was succeeded by the lamp. We find evidence of this in studying mythology. Thus Ceres, according to the old legend, sought her daughter in hell with a torch; Apuleius makes Psyche drop hot oil on Cupid from a lamp. Whether candles proper, *i.e.* wicks surrounded with wax, were known before or after lamps had come into use is doubtful. Martial (first century A.D.) speaks thus concerning the candle:—

> "Nomina *candelæ* nobis antiqua dederunt
> Non norat parcos uncta lucerna patres."—(Ep. xiv. 43).

Here, however, torch, *i.e. funalia*—which the old Romans in reference to its shining qualities would rather call *candela* than *funalia*—may be alluded to. In the Greek the word κανδήλα is a derivation from the Latin, not being met with until it is found in the writings of Athenæus. This author lived in the second century A.C., and in his "Deipnosophistæ" he says:—

> "ἐμοὶ δέ παῖ δωροίδειπνε ἀσσαρίου κανδήλας πρίω."

By that time, however, the rushlight had come into pretty general use, and no doubt it is to this that reference is here made.

But it is from a passage in Apuleius's Metam. iv. that we get the most valuable and conclusive information on this point. A noise being heard in the middle of the night, we are told that the household come in with "tædis, lucerna, sebaceis, cereis, et ceteris," that is with torches of pine, lamps, tallow candles, and wax tapers, which therefore clearly proves that candles both of wax and tallow were in use at this date. It seems, however, that the candle was probably used by the poorer people. At all events the lamp was a mark of respectability, as in another verse of Martial (Apoph. 42) we find that an apology is made for the use of a wax light instead of a lamp:—

> "Hic tibi nocturnos præstabit cereus ignis
> Subducta est puero namque lucerna tuo."

Juvenal (iii. 287) also speaks of the "breve lumen candelæ." In the British Museum, too, there is a fragment of a large candle found in Vaison, near Orange, and said to belong to the first century A.C. Such candles were probably provided with wicks consisting of the pith of rushes rudely covered with crude wax or tallow. Candlesticks for these existed, and later on they had a spike to penetrate the butt of the candle. However, the name candelabrum was more generally applied to the pillar on which the oil lamp stood or from which it was suspended. Since no attempt was made to provide for the current of air so necessary for proper combustion, these old lamps smoked exceedingly, so much indeed that it was the duty of one of the slaves of the household to go round each morning and wipe the soot from the pictures and statues. In one case, however, at the Erectheum of the Athens Acropolis, the lamp, which was of pure gold, was provided with a flue. This was a very large lamp, requiring to be filled but once in a year. Callimachus designed it for the new temple about 400 B.C., but the smoke was found to be so great an evil in anything designed for such a purpose, that the lamp was provided with a chimney in the shape of a bronze palm-tree inverted. But however magnificent and elaborate the design, it is certain that the economy of the lamp remained stationary.

It was generally filled with olive oil and provided with a wick either of oakum, or of the dearer Carpasian flax (cotton?). Occasionally, Pliny informs us, bitumen was used to fill the lamp; Italy, in some parts, being rich in springs of that mineral and petroleum. Further east, and especially among the tribes dwelling on the shores of the Dead Sea, bitumen and naphtha were much used as illuminating agents, and for other purposes. It may be suggested that the sacred pit-fire Nepti was of this nature. The well-known Egyptologist, Mr. Basil Cooper, has suggested the following as the origin of the word naphtha, viz. *NA*, water, of *Phtha*, the Hephæstos, or Vulcan of Egypt's deities, the god of fire. This idea receives some support from the fact that the Indians who sold the first petroleum as Seneca oil, and used it largely in their rites of worship, termed it fire-water, which name is now applied to alcohol.

Herodotus (ii. 62), writing of the Lychnokaie (feast of lamps) at Sais, in Egypt, in 450 B.C., only expresses surprise at the number of the lamps, and not at the lamps themselves, so that by this time they were getting into general use. Although their

introduction as a means of illumination was very gradual and slow in Greece, yet by the end of the fifth century B.C. they were probably in general use at least among the upper ranks of society. The lamp of which Herodotus speaks, which we have mentioned above, differed in no respect from that in use at Rome, the wick (θρυαλλίς) being made from the woolly leaves of an indigenous plant, which was passed through the nose (μυκτήρ) of the lamp into the crude olive oil.

So much for the methods of lighting in use in ancient times.

It is worthy of notice how the two elements of fire and light have ever been invested with divine attributes and set up for worship. The Persian monarchs have silver fire trays borne before them into battle. The Lychnokaie, the lamp feast of the Egyptians, referred to above, has a representative in the Chinese feast of lanterns, which takes place on the 15th of the first month. Not only this, but lamp festivals have been common to all nations. The Greeks had their λαμπαδη-δρομία, the Romans their Lupercalia, the latter of which gave way to the institution of Pope Gelasius, Candlemas, unless it be, as some have it, that Virgilius supplanted the Proserpina by this festival, but in any case they are both candle festivals. We learn from Pliny's "Natural History" that the Romans used wax candles in certain rites. They lighted lamps too in honour of Prometheus, who caught fire from heaven; of Minerva, who gave them oil; and of Vulcan, the originator of lamps; they had their *fax belli*, the war torch, the *fax nuptialis*, the marriage emblem.

Lamps, too, filled with scented oil were placed on the tombs of the dead. An oracular statue of Hermes in Achaia was "worked" by lighting a lamp before him and placing a small coin at his feet. Then there is the eternal lamp of Vesta, which was tended by damsels of established reputation, the ever-lighted lamps of Mahomet's tomb, Aaron's tabernacle, and Roman Catholic churches. Again there are those lamps in tombs said to have been found burning after the lapse of centuries. Boyle made a series of experiments with the air-pump which demonstrate the absurdity of such a belief. Mr. Field, however, suggests the possibility of an asbestos wick communicating with a supply of light naphtha burning in a tomb not absolutely air-tight as a way out of the difficulty, and concludes by indorsing Lamb's opinion of our badly-illuminated forefathers, that "one can never hear mention of them without an accompanying feeling as though a palpable obscure had dimmed the face of things, and that our ancestors wandered to and fro—groping."

THE ROYAL SOCIETY OF CANADA

THE second annual meeting of the Royal Society of Canada was held at Ottawa during May 22–25. The officers who had been elected at the close of the last meeting were all present, viz:—President, Principal Dawson, C.M.G., F.R.S.; Vice-President, Hon. P. J. O. Chauveau, LL.D.; Hon. Secretary, J. G. Bourinot, B.A.; Hon. Treasurer, J. A. Grant, M.D. Besides the members of the Society, there were present also delegates from the various local literary and scientific societies of Canada and from several British and foreign societies. Interesting inaugural addresses were delivered by His Excellency the Governor-General, who is Patron and Honorary President, by Principal Dawson, and by the Hon. Dr. Chauveau.

The report of the Council showed that a favourable answer had been received to the memorial to her Majesty the Queen, asking her gracious permission to name the Society the Royal Society of Canada; that an Act of Incorporation had accordingly been passed by the Dominion Parliament, and a sum of 1000*l.* sterling voted to assist in the payment of the expenses of publishing Transactions; and that steps had already been taken towards the formation of a national museum.

A considerable portion of the time of the Society was occupied by the discussion of a draft constitution which was submitted by the Council.

An address was presented by the Society to His Excellency the Marquis of Lorne expressive of the gratitude of the members of the Society to him for the efforts he has made during the time of his Governor-Generalship to further the interests of literature, science, and art.

Several interesting papers were read in the French and English Literature, History and Archæology Sections.

SECTION OF MATHEMATICAL, PHYSICAL, AND CHEMICAL SCIENCES

The following papers were read in this Section, which was presided over by T. Sterry Hunt, F.R.S.:—(1) Prof. J. G. MacGregor, D.Sc., Halifax, N.S., on "Experiments showing that the Polarisation of Electrodes is independent of their Difference of Potential." The same current was passed through two electrolytic cells (in series) containing dilute sulphuric acid and platinum electrodes. The cells had the same section but differed in length. The electrodes, therefore, differed in potential during the passage of the current, while the current had in both cells the same density. Curves showing the variation with time of the electromotive force of the respective cells after the cessation of the polarising current were drawn, and were found to coincide. The measurements of difference of potential were made by means of the quadrant electrometer. (2) Prof. B. J. Harrington, Ph.D., Montreal, on "An Analysis of two Minerals recently discovered in Canada—Meneghinite and Tennantite." During the discussion of this paper Dr. J. H. Ellis, of Toronto, exhibited a specimen of tellurium which he had extracted from the gold ores of Lake Superior. (3) C. Baillargè, C.E., Quebec, on "Hints to Young Geometers." (4) Prof. E. Haanel, Ph.D., Cobourg, on "Hydriodic Acid as a Blowpipe Reagent." The author had already proposed to use hydriodic acid as a blowpipe reagent in the case of four metals. This paper described the results of experiments made to extend its employment to others. Instead of charcoal he used flat plates of plaster of Paris, and in the case of all the metals which had been at the author's disposal, the blowpipe brought out on these plates easily distinguishable characteristic colours. Owing to the difference of volatility (chiefly) of the products of decomposition, three or four metals could be detected as present in a mineral by a single test, so distinctive are the colours of the iodides and other compounds formed. Prof. Haanel gave most successful experimental illustrations of the new method before the Section. (5) Prof. Coleman, Cobourg, on "The Spectra of certain of the Characteristic Colours of Prof. Haanel's Method of Blowpipe Analysis." (6) Prof. N. F. Dupuis, A.M., Kingston, on "The Construction of a Clock intended to show both Mean and Sidereal Time." The author had constructed the clock described; it gave a much closer approximation to accuracy than any such instrument hitherto proposed. (7) E. Deville, C.E., Ottawa, on "The Measurement of Terrestrial Distances by Astronomical Observations." The author deduced expressions for such distances in terms of differences of latitude and of azimuth respectively, and showed the influence of various sources of error in the use of these expressions. (8) T. McFarlane, M.E., Montreal, on "The Reduction of Sulphate of Soda by Carbon." (9) C. Baillargé, C.E., Quebec, on "Simplified Solutions of two of the more difficult cases in Hydrographic Surveying," and on "The Measurement of Surveys by Spherical Triangles and Polygons on a Sphere of any Radius." (10) Sandford Fleming, C.M.G., Ottawa, on "The Adoption of a Universal Meridian for the Regulation of Time." The author showed that the proposal he had made some years ago was meeting with a favourable reception. In connection with this paper the Section adopted a resolution urging the Society to memorialise the Governor-General, asking that he use his influence to induce the Imperial Government to grant representation to Canada at the International Conference on Standard Time to be held at the invitation of the President of the United States. (11) Reports by Prof. A. Johnson, LL.D., Montreal, and C. H. Carpmall, M.A., Toronto, Superintendent of the Meteorological Service, on "The Preparations made for the Observation of the Transit of Venus in Canada, and on the Observations which had been made." (12) Dr. J. H. Ellis, Toronto, on "A Remarkable Sulphur Spring near Port Stanley," and on "A Method by which the Tannin Determination of Löwenthal might be utilised for the Detection of Impurities or Adulterations in Spices." (13) F. W. Gisborne, Esq., Ottawa, on "Recent Improvements in Practical Telegraphy." (14) T. McFarlane, M.E., Montreal, on "The Decomposition of Zinc Sulphate by Common Salt." (15) T. Sterry Hunt, F.R.S., on "The Mechanical Transfer of Matter in the process of Segregation."

Prof. Cherriman, M.A., Ottawa, was elected president, Mr. T. McFarlane vice-president, and Prof. A. Johnson secretary of the Section for the next year.

SECTION OF GEOLOGICAL AND BIOLOGICAL SCIENCES

A. R. C. Selwyn, F.R.S., Director of the Geological Survey of Canada, presided over this section. The following papers were read:—(1) Dr. Selwyn, on "Notes on the Geology of Lake Superior." The points insisted on were: the conformity of the Laurentian and Huronian divisions of the older crystalline rocks; the Lower Cambrian age of the upper copper-bearing

rocks of Logan, called Animikie, Nepigon, and Keweenian, by Dr. Hunt, and the unconformity of the Animikie divisions to the underlying Huronian, by some geologists in the United States supposed to be of the same age. (2) Mr. W. Saunders, of London, Ont., "On the Influence of Sex on Hybrids among Fruits." This paper gave some of the results of Mr. Saunders's experience in hybridising fruits. The facts cited confirmed the view that the influence of the female is more strongly expressed in the habit, character of growth, and constitution of the vine, bush, or tree, while the influence of the male is more distinctly seen in the form, colour, and quality of the fruit, and in the case of hybrid grapes in the size and form of the seeds also. (3) Mr. G. F. Mathew, of St. John, N.B., on "The Method of distinguishing Lacustrine from Marine Deposits," based on careful observations on the deposits now taking place and accumulated since the Pleistocene period in lakes in New Brunswick. (4) Dr. J. A. Grant, of Ottawa, on "The Inferior Maxilla of the *Phoca Grœnlandica* from Green's Creek, near Ottawa. (5) Principal Dawson, of Montreal, on "Spores and Spore-cases, from the Erian Rocks." The author referred to the discussion many years ago by the officers of the Geological Survey of a bituminous shale at Kettle Point, Lake Huron, of vast numbers of minute round disks, which were recognised as the spore-cases of some cryptogamous plant, and named *Sporangites Huronensis.* More recently Prof. Orton, of Columbus, Ohio, Prof. Williams, of Cornell, and Prof. Clarke, of Northampton, have found in the Erian and Lower Carboniferous shales of Ohio and New York beds replete with these organisms, and Prof. Orton has shown reason to believe that they are connected with filamentous stems found in the same layers, and also that they have contributed largely to the bituminous matter present in the shales in which they occur. Similar bodies have also been found associated with the curious plants known as Ptilophyton and Trochophyllum. Still more recently specimens from the Erian of Brazil have been sent to the author by Mr. Darby, of the Brazilian Geological Survey, which seem to throw additional light on these bodies. They are oval or rounded or in the form of flattened sacs, containing numbers of rounded disks, and so closely resembling the utricles or spore sacs of the Rhizocarps as to make it extremely probable that they belonged to plants of this class. Should this conjecture be sustained by subsequent inquiries it would show that this peculiar group is of much greater antiquity than hitherto supposed, and that these plants were extremely abundant in the shallow waters of the Erian period. Dr. Dawson suggests the probable relation of these singular fruits not only with the Ptilophyton, but also with the other Erian and Silurian plants. (6) E. Gilpin, jun., on "The Folding of the Carboniferous Group in the Maritime Provinces." The author described each of its great subdivisions as exposed at various points, and showed that during the Carboniferous period, in addition to the continental changes of level, giving rise to conditions of deposition characterising the carboniferous limestone, millstone, grit, &c.. there were extensive foldings of a more local character, apparently in some cases marking the closing of these oscillations, These foldings and their subsequent denudations have played an important part hitherto but little studied in modifying the conditions arising from the larger and more extended movements which have hitherto principally received attention, and present the district as being far from an universal state of quiet and regular succession during the Carboniferous age. (7) Prof. R. Bell, M.D., on "The Causes of the Fertility of the Land in the Canadian North-west Territories." In the Canadian North-west a vast fertile tract stretches, with certain exceptions, from the Red River Valley to the Liard River, a distance of some 1400 miles. The soil of this tract was characterised as a dark loam, of varying depth, and of a nearly homogeneous consistency. The primary cause of the fertility of this region was the abundance of the underlying crude material out of which a finished soil could be made. This was derived partly from the widespreading crustaceous marls which were nearly coextensive with the fertile tract, and probably from the drift during the Glacial period. Dr. Bell next considered the process by which the black loamy soil was formed out of this subsoil, and he considered that the main agency was the work of moles and other burrowing animals. Worms appeared to be absent in the North-west, owing principally to the frost penetrating into the ground beyond the depth to which worms can burrow, but the moles and the ground squirrels, or gophers, more than make up for their absence. In the fertilised tracts the old and new mole-hills cover the whole surface, rendering it "hummocky," which may be easily observed after the prairie has been swept by a fire. The badgers often did what was compared to subsoil ploughing. All the animals referred to were very active in the autumn, digging many more burrows than appeared to be of any use to themselves. Each hummock thrown up by the moles covered about a square foot, and buried all the grass, &c., on this space. In this manner large quantities of vegetable matter were ultimately incorporated with the soil. The work of the moles also acted in another way in refining the soil, for they left behind the stones and coarse gravel, so that these in time became sunk beneath the layer of mould produced. By an interesting coincidence at the season when the burrowing animals are most active, the prairie vegetation is mature, and contains the largest amount of substance. The coldness of the soil during the most of the year tended to preserve the organic matter in it. While the circumstances given were the direct cause of its fertility, the ultimate reason was perhaps to be looked for in the climate of the North-west, for to this was due the growth of the vegetation which formed the manure and the food of the little workers which mingled it with the soil. Thus we could trace a mutual dependence of the circumstances which together have given to our North-west Territories that surpassing fertility of soil which cannot fail to attract to it a vast population. (8) Dr. G. M. Dawson, on "Notes on Triassic Rocks of the West," discussing the question as to the Triassic or Jurassic age of deposits found in British Columbia and the Rocky Mountains, and their correlation with the deposits of similar age in the territory of the United States. (9) Prof. L. W. Bailey, Ph.D., Fredericton, on "The Occurrence of Indian Relics in New Brunswick," probably deposits found at an old camping ground of the Malicete Indians. (10) Dr. T. Sterry Hunt, on "Studies on Serpentine Rocks." (11) Prof. J. Macoun, on "Notes on Canadian Polypetalæ." The geographical distribution of these plants in Canada was discussed, and interesting facts were adduced in connection with the number of species and genera in each order which showed certain relations between the present flora and that which had existed in the Tertiary period. (12) A paper by Mr. R. Chalmers was communicated by Principal Dawson, in which facts were stated showing important erosion on the coast of the Bay des Chaleurs by floating ice in the modern and later Pleistocene periods.

Dr. Selwyn, Prof. Lawson of Halifax, and J. F. Whiteaves, were re-elected president, vice-president, and secretary of the Section respectively.

The following were the officers elected by the Society for the present year:—President, Hon. P. J. O. Chauveau, LL.D.; Vice-president, T. Sterry Hunt, F.R.S.; Hon. Secretary, J. G. Bourinot, B.A.; Hon. Treasurer, J. A. Grant, M.D.

THE HYPOPHYSIS CEREBRI IN TUNICATA AND VERTEBRATA[1]

In most simple Ascidians the single nerve ganglion is situated near the anterior end of the body, and between the branchial and atrial apertures. In species where the atrial aperture is near or at the posterior end of the body, the ganglion may also be placed far back, but it still lies between the two apertures and always indicates the dorsal side of the branchial. The ganglion is usually elongated, and gives off nerves at both ends—one set anteriorly and ventrally towards the branchial aperture, the other set posteriorly and dorsally towards the atrial. In close relation with the ganglion are found two organs—the neural gland and the dorsal tubercle—which have been much written about, but apparently will bear a good deal of further investigation.

The neural gland lies upon the ventral and posterior face of the nerve ganglion, and consists of a number of more or less ramified cæcal tubules springing from a central space or tube immediately below the ganglion. The presence of this organ was first distinctly pointed out by Albany Hancock in 1868,[2] but until quite recently its function was not only totally unknown, but had been scarcely investigated.

The dorsal tubercle was described by Savigny in 1816[3] under the name of "tubercule antérieur." Since then it has received many names, but has usually been regarded as some sort of ol-

[1] Abstract of a paper read before the Royal Society of Edinburgh, April 2.

[2] *Journ. Linn. Soc.* (*Zool.*), vol. ix.

[3] "Mémoires sur les Animaux sans Vertèbres," pt. ii. fasc. 1. (Paris, 1816.)

factory organ. It is placed on the dorsal edge of the anterior end of the branchial sac, behind the circle of tentacles, and usually in a distinct "peritubercular" area, a diverticulum from the prebranchial zone formed by a bending posteriorly of the dorsal ends of the peripharyngeal bands.

The dorsal tubercle is, in the simplest form known, a funnel-shaped depression having its wider circular open end separated from the prebranchial zone in front of the branchial sac by a raised edge or lip, while its opposite narrower end is continued into a fine canal running dorsally and posteriorly. This simple condition is found in *Molgula pedunculata*; in *Eugyra kerguelenensis* the aperture is still wide, although its edge is square in place of being circular. In other simple Ascidians the anterior half of the edge has been apparently pushed backwards, so as to become invaginated and closely applied to the posterior half, thus reducing the circular aperture to a crescentic or semicircular slit. This condition is found in *Corella parallelogramma*. In most other forms more or less complication is produced by the ends of the slit, or "horns" as they may be called, being prolonged, often to a very great extent, and coiled in various directions, sometimes producing beautifully regular and closely placed spirals. The patterns produced by this curving of the horns are very numerous and often complicated, but their value in classification is slight, since they differ sometimes to a considerable extent in individuals of the same species, and on the other hand are sometimes very similar in members of different genera or even families.

This variously-shaped organ is histologically merely a depression in the connective tissue of the mantle, lined by epithelium continuous with the squamous epithelium covering the prebranchial zone, but modified upon the edges of the slit into cubical or columnar ciliated cells. Since the time of Savigny it has been almost universally regarded as a sense-organ of some kind—probably olfactory or gustatory, or in some way capable of testing the quality of the inhalent current of water. The reasons for this view have been :—

1. The position of the organ at the entrance of the branchial sac where a sense-organ would be of great apparent value.
2. Its structure—a ciliated depression covered in part by columnar cells, some of which closely resemble sense-cells.
3. Its intimate relation with the ganglion, and the presence of a nerve arising from the anterior end of the ganglion, running towards the branchial aperture close past the dorsal side of the tubercle, and presumably supplying it with nerves.

In 1876 Ussow showed that the gland of unknown function lying below the ganglion had a delicate duct, lined by cubical epithelium, which ran forwards and opened into the tubular posterior end of the funnel-like depression forming the dorsal tubercle; so that the slit of the tubercle was thus shown to be merely the aperture of the duct from the neural gland. In 1881 Julin[1] confirmed this discovery, described minutely the condition of the gland, the duct, and the tubercle in several species of simple Ascidians, and declared that there was no connection between the nerve running from the ganglion to the branchial aperture and the tubercle, and that consequently the latter was not a sense-organ, and was nothing more than the opening of the duct. In a second paper, published shortly afterwards, Julin[2] described the condition of these organs in two additional species, and enunciated the theory, suggested to him by E. van Beneden, that the neural gland was renal in function, and was the homologue of the hypophysis cerebri of the vertebrate brain. In favour of this homology may be considered :—

1. The position of the gland upon the ventral surface of the nerve centre and above the pharynx.
2. Its glandular nature.
3. Its connection with the anterior end of the pharynx by a duct—Balfour, Kölliker, and others having shown that the hypophysis or pituitary gland in higher vertebrates arises as a dorsal diverticulum from the stomodæum, but afterwards loses this connection.

From my own observations I can confirm Julin's statement as to the presence of the duct from the neural gland and its connection with the slit of the dorsal tubercle, and, like him, I am unable to find any nerve supplying the supposed sense organ. I have, however, in several cases seen certain of the epithelial cells covering the edges of the slit which had a striking resemblance to sense-cells, such as those in the ectoderm of *Actiniæ*. This observation, taken along with Julin's descriptions, and especially with the condition of affairs in some specimens of *Ascidia mammillata* which I have recently examined, has suggested to me that possibly the dorsal tubercle may be *both* the aperture of a gland corresponding to the hypophysis and also a sense-organ, probably of an olfactory or gustatory nature.

Ascidia mammillata is one of the forms discussed by Julin in his second paper. It is a large species with the branchial and atrial apertures rather far apart, and the ganglion at a considerable distance from the anterior end of the body. Julin found that the neural gland in this species did not form the usual compact mass, but was in a somewhat rudimentary condition, and that besides having the usual duct running anteriorly to communicate with the pharynx by the dorsal tubercle it had also a number of short funnel-shaped apertures into the peribranchial or atrial cavity inclosed by the mantle; so that in this species the products of the gland might be excreted either into the branchial sac (pharynx) or into the dorsal part of the peribranchial cavity, the region into which the intestine and the genital ducts also open.

In two specimens of *Ascidia mammillata* which I had an opportunity of examining recently I found the neural gland in exactly the condition described by Julin, but its duct had no aperture into the pharynx, the dorsal tubercle being entirely absent. The small funnel-shaped apertures into the peribranchial cavity were numerous and well developed, so that in the case of these individuals the neural gland was connected with the cloacal part of the peribranchial cavity only, exactly the arrangement to be expected if the gland had a renal function. It seems possible to me that this, or something like this, may have been the condition of affairs in the primitive Chordata previous to the point of divergence of the Urochorda. There may have been a renal gland placed ventrally to the nervous system, not necessarily at the anterior end only, and opening on the surface of the body by one or more laterally-placed apertures,[1] this gland being represented in the Tunicata by the neural gland, and in the Vertebrata by the glandular portion of the pituitary body.

Then the dorsal tubercle apparently is or was a sense-organ—possibly placed at first on the surface of the body, since the anterior part of the pharynx develops from the epiblast as a stomodæum—and I think it probable that the connection of the tubercle with the duct of the neural gland may be an after-change, caused possibly by the enlargement of the pharynx into a branchial sac, and the development of the peribranchial chamber. It may readily be imagined how, as the result of the formation of these cavities, the neural gland would be brought into closer relation with the dorsal tubercle, and one or more of the funnel-shaped ducts of the gland might, after having been carried in from the surface by the formation of the lateral atrial involutions, come to open into the ciliated depression of the tubercle in place of into the peribranchial cavity, thus producing very much the condition described by Julin in his specimens of *Ascidia mammillata*. By suppressing the original openings into the peribranchial cavity and leaving merely the secondary opening into the pharynx by means of the dorsal tubercle, we arrive at the condition found in all ordinary Ascidians. It is not easy to see the reason for this change, as there is no apparent advantage to be derived from it, but there is probably also no disadvantage, since there is abundant communication between the branchial sac and the peribranchial cavity through the stigmata or slits in the wall of the former.

This suggestion as to the origin of the present structure of the neural gland and neighbouring organs in most Tunicata implies that the pituitary body in the Vertebrata, which has lost its connection with the exterior, and probably also its function, has a similar history. In this view I am encouraged by some remarks by Balfour,[2] from which it is clear that he considered the pituitary body, judging from its development, to have been originally a sense organ opening into the mouth, and possibly corresponding to the Ascidian dorsal tubercle. He has also suggested,[3] as an alternative, the possibility that the neural gland in the Tunicata may be the homologue of the vertebrate pituitary body. This is of course the theory supported by van Beneden and Julin, and is open to the objection that it does not account for the remarkable structure of the dorsal tubercle. The view I hold combines both of those above mentioned by considering the pituitary body as the homologue of the neural gland,

[1] *Archives de Biologie*, vol. ii. p. 59.
[2] *Loc. cit.* p. 211.

[1] The lining of the peribranchial cavity, into which the ducts open in the Ascidian, is derived from the epiblast, being formed in the embryo by a pair of lateral involutions, which afterwards fuse dorsally.
[2] "Comparative Embryology," vol. ii. p. 359. [3] *Loc. cit.* p. 360.

and as being therefore the rudiment of a primitive renal organ,[1] which opened by lateral ducts upon the side wall of the body; while the connection of the pituitary body with the stomodæum in embryo vertebrates is regarded as being not its original and proper duct, but a secondary connection, which has been formed with a lost sense-organ placed at, or in front of, the anterior end of the pharynx, and homologous with the dorsal tubercle in the Tunicata.

Ussow and Julin have conclusively shown that the dorsal tubercle is not *merely* a sense-organ. The complex structure which the tubercle usually presents seems to indicate that it is not *merely* the aperture of a duct. Whether, as I suggest, it may be a sense-organ into which the duct has come to open can scarcely be determined on the evidence at present in our hands. The lines of investigation which may be reasonably expected to throw additional light upon the matter are: (1) the exact course of development of the neural gland and the dorsal tubercle, and further information as to the pituitary body; and (2) the examination of the condition of the gland and its ducts throughout the Tunicata, and especially in a large number of specimens of *Ascidia mammillata*, a species in which these organs appear to be in a variable and highly interesting condition.

W. A. HERDMAN

[1] Not the pronephros, since that is found along with the pituitary body in many vertebrates, but possibly more ancestral. Might it not be the homologue of the provisional trochosphere excretory organs described by Hatschek and others in *Polygordius* and some Mollusca?

UNIVERSITY AND EDUCATIONAL INTELLIGENCE

CAMBRIDGE.—Mr. W. H. Caldwell, B.A., Fellow of Caius College, has been selected to proceed to Australia to endeavour to solve the important questions connected with the reproduction and the embryology of the Monotremata, which have so long baffled inquiry.

Mr. S. F. Harmer, B.A., of King's College, 1st Class in the Natural Sciences Tripos 1883, has been appointed Demonstrator of Comparative Anatomy, in the vacancy caused by Mr. Caldwell's resignation.

Mr. W. F. R. Weldon, B.A., of St. John's College, has been appointed Prosector to the Zoological Society.

Mr. J. Bateson, B.A., of St. John's College, is proceeding to North America to study the life-history of Balanoglossus.

Mr. J. Roberts, B.A., of St. John's College, has been appointed assistant to the Woodwardian Professor.

Prof. Macalister will hold a class in Osteology in the long vacation.

Dr. Humphry has been elected Professor of Surgery.

SCIENTIFIC SERIALS

THE *American Journal of Science*, July.—On the genesis of metalliferous veins, by Joseph Le Conte. From his study of the phenomena of metalliferous deposit by solfataric action at Sulphur Bank and Steamboat Spring, the author argues against Dr. F. Sandberger ("Untersuchungen über Erzgänge," Wiesbaden, 1882) that all lodes have been formed by deposit from solutions. In this important paper the conditions under which the deposits take place and the character of the solvents are fully discussed. Besides simple water, whose solubility is greatly increased by super-heat and pressure, the most active agency appears to be alkali in the form of alkaline carbonates or alkaline sulphides, or both. Such alkaline carbonate waters, ascending slowly towards the surface through underground fissures, gradually lose much of their solvent power both by cooling and by relief of pressure, and must of necessity deposit in their courses, and form metalliferous veins. In this way even cinnabar and gold may be precipitated. Other powerful agencies may be organic matter of universal occurrence in subterranean waters, and known to be potent in reducing metallic oxides and metallic salts. Mainly by these methods it is argued that alkaline waters at various temperatures, but mostly hot, circulating in all directions, but mainly up-coming, and in any kind of water-way, but mainly in open fissures, form by deposit mineral veins. Amongst the many subjects incidentally treated are: Association with metamorphism, variation in vein contents; variation of richness with depth; origin of alkaline and metallic sulphides; occurrence of gold; irregular, brecciated, contact, and other kinds of lodes.—Evolution of the American trotting horse, by Francis E. Nipher. By an ingenious process of calculation the author arrives at the conclusion that the maximum speed to which the American trotting horse will constantly approximate without ever reaching it is a mile in ninety-two seconds.—The burning of lignite *in situ*, by Charles A. White. The ignition of the lignite beds still burning in Montana, and of others long extinct in Colorado, Wyoming, Dakota, and elsewhere, is attributed mainly, if not altogether, to spontaneous combustion, according as the deposits become by erosion successively exposed to atmospheric influence.—On the paremorphic origin of the hornblende of the crystalline rocks of the North-western States, by R. D. Irving. An examination of about 1000 sections representing the crystalline schists, and eruptives and basic eruptives of a region 400 miles by 300, and of three distinct geological systems, showed the occurrence of no hornblende not clearly or very probably secondary to augite.—On the constituents of the meteorites which fell at Bishopsville, South Carolina, in March, 1843, and at Waterville, Maine, in September, 1826, by M. E. Wadsworth.—A simple method of correcting the weight of a body for the buoyancy of the atmosphere when the volume is unknown, by Josiah Parsons Cook.—Recent investigations concerning the southern boundaries of the glaciated area of Ohio, by G. F. Wright. The limit is determined by an irregular line running from Aurora near New Richmond, in a north-easterly direction through Chillicothe, Newark, Dunville, and Canton, to New Lisbon, near the Pennsylvanian frontier.—On the variation of the specific heat of water, by G. A. Liebig.

Bulletins de la Société d'Anthropologie de Paris, tom. v. fasc. v. 1882.—On the tribes of Terra del Fuego, by M. O. Beauregard.—A paper by M. G. de Rialle on M. O. Beauregard's views regarding the origin of the Dardous, communicated to the Society in April, 1882, in which M. de Rialle contests the opinion that the Thibetan races are Mongols. He considers that the monosyllabic character of their language is a distinct proof of their non-Mongolian origin, the Mongol being an agglutinated form of speech belonging to the Altaic linguistic families. In reply to his objections M. O. Beauregard read a voluminous paper at a subsequent meeting, on the ancient and modern ethnography of Cashmere and Thibet, which is mainly based on Stanislas, Julien, Deguignes, and other older French authorities, and on modern English writers, more especially Major Biddulph, to whose important labours and accuracy M. Ujfalvy bore testimony in his defence of M. de Rialle's views.—Observations by M. Hamy on the anthropology of the Comalis of the East African coast.—Exposition, by M. de Nadaillac, of the scope and character of his work, "L'Amérique Préhistorique," presented by him to the Society.—Zoological observations in Equatorial Africa during M. de Brazza's expedition, by M. Cornevin, derived from the notes of the naturalist, M. Michaud. From these it would appear that in the valley of the Ogoone the climate is constant, the temperature standing generally at about 90° Fahr. Maize, manioc, and tobacco are grown. The people are courageous but peaceable. The sheep have no wool and only little hair. A dark, fierce race of cattle, feared by the natives, abounds in the forests, but there are no indigenous horses.—Observations on the Galibis, by M. Dally.—On the anthropological distinctions between the two races confounded under the common name of Kabyles, by M. Sabatier.—On the flint instruments, including a lasso of the Quaternary period, found in the gravel beds of Sarliève, by Dr. Pommerol.—On the horse of prehistoric and historic times, by M. Pietrement.—On the dental mutilations of the ancient inhabitants of Mexico and Yucatan, by M. Hamy.—On social instinct, by Madame Clémence Royer. This paper, intended to supplement the writer's larger work, "L'Origine de l'Homme et des Sociétés" (published in 1870), considers social instinct in relation to plants as well as to animals generally.—Craniological observations on a series of the crania of assassins, by M. Orchanski, considered specially with reference to the relation between the skull and the face. The author's determinations are in close accord with those of MM. Ten-Kate and Bordier.—On the existence of a rudimentary cæcal appendage in some of the Pitheci, by M. Hervé.—Remarks on certain differences between Catholics, Protestants, and Jews, as to the relations among them of deaths and births, by M. G. Lagneau. The author finds that the Catholics generally, with a somewhat higher natality, have a considerable infantile mortality, resulting in a correspondingly feeble increase of population, while among Protestants this increase is often much higher, notwithstanding a somewhat smaller natality, which, however, is corrected by a

lower infantile mortality. The Jews present a much more rapid increase of numbers than either of the other two religious bodies, for, although their natality is less than either, their mortality is remarkably low for all ages, these conditions being probably due to their dietetic and hygienic regulations, the infrequent occupation of women out of their homes, early marriages, and general sobriety.

SOCIETIES AND ACADEMIES

EDINBURGH

Royal Society, July 2.—The Astronomer-Royal for Scotland communicated a paper, which was read by Prof. Crum Brown, on the group *b* in the solar spectrum, as observed with the remarkably fine spectroscope which Prof. Tait had recently secured for the University. The main conclusion come to was that the speculations regarding the existence of *basic* lines were unwarrantable, since the lines b^3 and b^4 were both distinctly double lines, each real single line in all probability being due to one of the substances, magnesium, iron, or nickel. The paper gave a complete historical statement of the observations of the *b* group by Swan, Ångström, Thalén, Young, and others, since the year 1830.—Prof. C. G. Knott read a paper on superposed magnetisms in iron and nickel. The experiments were, in part, a repetition of Wiedemann's well-known investigations into the twisting of iron wire under the influence of longitudinal and circular magnetisations. With a steady current along the wire, and a varying current in a helix round the wire, a twist was obtained which in almost every case reached a maximum for an intermediate value of the helical current. The maximum occurred sooner when the longitudinal current was diminished. No such maximum was obtained in the case of nickel, which twisted more and more for greater and greater currents, until the point of magnetic saturation was reached. Again the nickel twisted in the opposite direction to iron, other things being the same—a result in accordance with Barrett's observation that nickel *contracts* when magnetised, while, as Joule first proved, iron extends. The effect of weighting the wires so as to subject them to different tensions, was also investigated, the general result being that the twist was greater for the smaller weight, except for special combinations of current strengths and weights. —Prof. Tait gave further results as to the lowering of the maximum density point of water under increased pressure. By an improved method he estimated the lowering to be 2°·7 C. for one ton's weight per square inch, a result in wonderful agreement with that obtained by the indirect method carried out by Professors Marshall and Smith and Mr. Omond.—In a note on surface emissivities, Prof. Tait drew attention to the apparent lack of data on this subject, which, however, could be largely supplied from the numerous observations by Prof. Forbes and himself on the rate of cooling of the bars used in the conduction of heat experiments.—Prof. Tait also submitted to the Society a photograph of the markings on the arm of the boy who had been struck by lightning at Duns some weeks ago.

PARIS

Academy of Sciences, July 9.—M. Blanchard, president, in the chair.—On the pyroelectricity in blende, chlorate of sodium, and borazite, by MM. C. Friedel and J. Curie.—On the separation of gallium from tellurium and silicium, by M. Lecoq de Boisbaudran.—Observations on M. Hirn's recently published work on "The Phenomena due to the Action of the Atmosphere on Falling Stars, Aërolites, and other Meteoric Objects," by M. Daubrée. In this work the author argues that the apparition of all kinds of meteors in space, their luminosity and explosion, and accompanying sounds depend directly and exclusively on their velocity. This general conclusion is questioned by M. Daubrée, who points out that account must also be taken of the chemical action produced at contact of meteoric substances with the atmosphere.—On the infra-red spectra emitted by metallic vapours, by M. Henry Becquerel. The metallic vapours here dealt with are those of sodium, magnesium, calcium, potassium, silver, and thallium. The method of analysis described by the author opens a new and wide field of observation, comprising between the wave-lengths 760 and 1300 an interval of wave-lengths greater than that existing between the extreme red of the visible spectrum and the last-known ultra-violet rays.—Researches on the destruction and utilisation of the bodies of animals that have died of contagious diseases, and especially carbon poison, by M. Aimé Girard. The method here proposed consists in dissolving the carcasses at a low temperature in concentrated sulphuric acid, and then utilising the liquid thus obtained in the production of a superphosphate of azotic lime.—A protest is presented to the Academy on MM. Delattre's recent paper (meeting of May 21) on the treatment of the waters used in woolwashing. MM. Gaillet and Huet claim to be the real authors of the process, and support their claim by sundry documents.—On the conditions of the subsoil under the Berlin Observatory; letter addressed to M. Faye by M. Foerster.—On a method capable of furnishing an approximate value for the integral $\int_{-\infty}^{+\infty} F(x)\,dz$, by M. G. Gourier.—Generalisation of the theorem of Jacobi on the partial determinants of the adjunct system, by M. Em. Barbier.—On the reduction of equations, by M. A. E. Pellet.—On a lever, a new system of Roman balance with automatic slider, by M. A. Picart.—General formulas of centred dioptric systems, by M. Monoyer.—A new method of determining the limits of electrolysis, by M. Ch. Truchot.—On samarium, by M. P. T. Clève.—On the blue colour obtained by the action of chromic acid on oxygenated water, by M. H. Moissan.—On tetric acid and its homologues, by M. W. Pawlow.—On the dimorphism of iodide of silver, by MM. Mallard and Le Chatelier.—On some new characteristic reactions of salts of gold, by M. Ad. Carnot.—On the alcoholates of soda, by M. de Forcrand.—On the pyrogenation of colophany, by M. Ad. Renard. —Researches on the curve of muscular shocks in various maladies of the nervo-muscular system, by M. Maurice Mendelssohn. —Development and structure of tuberculous begonias, by M. Henri Duchartre.—Contributions to the study of the fermentation of breadstuffs, by M. L. Boutroux.—The microbes of the lymph of marine fishes, by MM. L. Olivier and Ch. Richet. The presence of parasites is clearly determined, and the authors conclude that microbes are nearly always present in the lymph, and consequently in the very tissues of the marine fishes.—Method of determining the quality of the wines of the south of France, by M. A. Audoynaud.

BERLIN

Physical Society, June 8.—Dr. Martius discussed the two recently-discovered instruments which are employed for the measurement of small frequently-occurring variations of a current, the telephone and the capillary electrometer. The latter, as is well known, was constructed about ten years ago by Mr. Lippmann in the laboratory of Herrn Kirchhoff, and is based on the principle that a current passing through a meniscus changes its surface tension, and causes a movement of the meniscus. The frequent variations of weak currents are indicated with difficulty, if at all, by galvanometers and tangent compasses, but the capillary electrometer can make such variations, especially as they occur in electrophysiology, visible to the eye. It has therefore quite latterly been employed in physiological experiments, and Dr. Martius has undertaken to investigate the capabilities of the apparatus in the form designed by Prof. Christiani, and described below. A glass tube drawn out at one end to a capillary, and partly filled with mercury, stands vertically in a large glass vessel also containing some mercury, and above it dilute sulphuric acid, in which the capillary point of the tube dips, so that the acid passes into the tube and up to the mercury meniscus. The position of the latter is read with a microscope. Metal wires are dipped into the mass of mercury, and a current can then be sent through the capillary tube, the current causing a motion of the mercury meniscus either upwards or downwards according to its direction, on a positive current flowing downward from the mercury in the tube moving the meniscus downwards, a negative current, upwards. In this apparatus care must be taken to keep the current too weak to cause electrolysis of the acid; otherwise the instrument becomes useless and must be refilled. The observations were first made with a constant current which was interrupted at will, and they showed that under exactly similar conditions the displacement which a positive current produced were always greater than those caused by a negative current of like strength. On making and breaking contact rapidly, for instance about twelve times a second, a total displacement of the mercury, corresponding to the direction of the current, was observed, and also oscillations of the meniscus, the number of which was equal to the number of interruptions of the current. If the number of interruptions was increased, a stronger current had always to be used in order to make the

oscillations of the meniscus perfectly visible, weaker currents causing a total displacement of the mercury corresponding to the strength of the current, while the oscillations of the meniscus appeared only as a broad undefined rim. Dr. Martius then investigated the action of induced alternating currents, the behaviour of which was much more complicated inasmuch as, with equal intensity of the primary current and equal distances of the induction coils from one another, the four following different cases are to be observed: (1) The current on breaking contact passes through the mercury meniscus in a positive or anodic direction; (2) the current on making contact passes in a cathodic direction; (3) the current on breaking contact passes in a cathodic direction; (4) that on making contact passes in an anodic direction. All these four cases which group themselves in pairs in every experiment, affect the meniscus differently; for besides the difference of the anodic and cathodic current, already mentioned in the case of constant currents, the current on making contact under otherwise similar conditions was more effective than that on breaking contact, the action of the current on the instrument being, therefore, just the reverse of that on the nerves and muscles. The reason of this is that in the capillary electrometer the current on making contact produces a stronger polarisation than that on breaking contact, on account of its longer duration. The total effect which alternating induction currents produce on the capillary electrometer is the result of the individual effects of the current, and is certainly on this account very complicated, but it can be predicted acccording to the rules given above for every direction, strength, and frequency of the induction currents.- Prof. Kronecker demonstrated on a student the audibility of the muscle tone when the muscle was voluntarily contracted, by means of a pair of telephones. The telephones were connected with two needles, which the student placed in his biceps muscle, and the members of the Society convinced themselves that at every contraction of the muscle a deep soft breathing tone was heard.

Physiological Society, June 29.—Dr. Curt Lehmann explained two apparatus, which he had constructed with a view of maintaining artificial respiration in animals upon which other experiments are tried. The former method, which consists in blowing air into the lungs by means of a motor working in a certain rhythm, has the disadvantage that, in order to keep up the efficiency of the ventilation, the pressure must soon be increased, producing emphysema of the lungs, to which the animals succumb. Dr. Lehmann has obviated this by blowing air into some receptacle by means of the motor in question, and by letting it there be condensed to a certain moderate density (say 8 to 10 cm. of water). A second receptacle contains air in a corresponding degree of rarefaction. An indiarubber tube is tied into the trachea of the animal; this tube is forked at the other end, one branch communicating with the condensed the other with the rarefied air. An electric clock, which marks whatever intervals of time are required, is connected by means of a double lever with this tube, and alternately closes the one or the other of the branches. Thus air is either driven into the lung under a gentle pressure or is sucked out of it under the same pressure. In spite of the low pressure, the ventilation is perfect on account of the alternate driving in and sucking out of air; the lung of the animal is in no wise affected, and artificial respiration can thus be kept up without danger for eight hours. The second apparatus, which on the whole, after the same principle, connects the lung alternately with condensed and rarefied air, is constructed in a more complicated manner, as it contains bells for the collection of the respiration products, for the event that these may have to be examined. Both apparatus work automatically; the influence of the respiratory motion upon the blood pressure could be shown when they were used, just as easily as with animals respiring normally. The special experiments in which Dr. Lehmann used these apparatus referred to the influence of temperature upon the bacilli of Septicæmia. Developed in blood outside the body, the number of bacilli increased the more, under equal conditions otherwise, the higher the temperature, up to 43° C. With animals the experiments were made in such a way that in each series of experiments four rabbits were vaccinated with septicæmic bacilli. Of these No. 1 was kept at 42° C., No. 2 at ordinary room temperature, No. 3 strongly cooled by means of water (temperature 35° C. in the interior), and No. 4 poisoned with curare and cooled. No. 1 died first, although about two hours before its death but few bacilli were contained in the blood; soon afterwards No. 3 died, its blood containing many bacilli; a few hours later No. 2 succumbed, having attained the fever temperature of 42° C. much later than No. 1; the number of bacilli in its blood was moderate. No. 4 lived longest, although the number of bacilli in its blood was greatest.—A communication was then read concerning the important observations made by Prof. Pflüger (Bonn) regarding the division of frog's ova by a groove-formation after fertilisation. It is known that fertilised frog's ova turn over in such a way that their black hemisphere is turned upwards and the white one downwards, and that the axis passing through the centre symmetrically to both hemispheres is perpendicular. The normal grooving now begins with a division in a median plane passing through the axis; the second division is at right angles to the first, also passing through the axis; the third one takes place at right angles to the axis, somewhat nearer to the upper end. Prof. Pflüger prevented some fertilised frog ova from turning over by fastening them to glass, so that in the single ova the hemisphere axes pointed in the most varied directions; yet he found that the first division in *all* of them was always perpendicular, without any reference to the position of the axis; the second and third divisions of the ovum remaining in the same relative position with regard to the first anomalous division as if the ova had been in a normal position. The first traces of the groove of the back also invariably showed themselves on the upper side of the first division plane, thus frequently in the white hemisphere. But later on all the ova which were fastened at the bottom perished.

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ERRATUM.—On p. 264, col. 1, line 9, for *rabies* read *tabes*.

THURSDAY, JULY 26, 1883

ZOOLOGY AT THE FISHERIES EXHIBITION

I.

THE manifold relations of zoological science to the various fish industries are, on the whole, fairly well illustrated in the Kensington Exhibition if we take together into consideration all the exhibits of foreign countries, of these islands, and of British colonies. Considered alone, however, the British department is remarkable for the extreme paucity and insignificance of exhibits having any scientific value. This is due to the fact that no attempt was made by those who organised the exhibition to obtain scientific advice and direction, so as to enable them to make application to the individuals or museums possessing objects illustrating the scientific aspects of fish and fisheries, and that no individual with authority and responsibility has attempted to bring together that class of objects—which are abundant enough in both private and public collections in England, and form, on the contrary, a large portion of the exhibits of foreign countries. Thus under the direction of a properly-trained zoologist—Prof. Spencer Baird—the Smithsonian Institute has been able to form a collection which is sent over to this country by the American Government as the official representative collection. It is not an exaggeration to say that this collection, both on account of the range and variety of its objects and the instructive way in which they have been disposed and treated by the American Commissioner, Mr. Brown Goode, has been the admiration of all visitors. Similarly the Swedish authorities have intrusted Prof. Smidt with the duty of bringing together objects illustrating the zoological aspect of fish and fisheries in Sweden. Collections from the museums of Gothenburg and Stockholm and from eminent Swedish zoologists are consequently exhibited in the Swedish department. So also in the case of the Netherlands, of British India, and of New South Wales, we find the well-known naturalists, Prof. Hubrecht, Dr. Francis Day, and Mr. Ramsay, specially charged with such responsibility.

There can be no doubt that the collections, both public and private, of this country, might have been brought into requisition and made to furnish such an exhibition of marine and freshwater fishes, of their food, of their parasites, and other enemies, and again of the like objects in relation to oysters (both edible and pearl-bearing), lobsters, sponges, and precious coral, as no other country could possibly bring together.

The exhibits of zoological specimens may be classed under three heads, viz. (1) those which are strictly zoological, that is to say, intended to illustrate either the aquatic inhabitants of a particular district, or the structure and life history of a particular species; (2) those of economic significance, illustrating the cultivation or modes of occurrence of an aquatic organism or organisms having a direct commercial importance; (3) those having an ornamental or personal value, and being of the nature of trophies, such, for instance, as Lady Brassey's case of corals, and the many cases of dried stuffed skins of large trout and pike exhibited by angling societies.

The most important collection of the first group is one comprising representatives of all classes of marine animals preserved in alcohol, and numbering nearly 400 specimens. It is sent by Dr. Anton Dohrn, the director of the Zoological Station of Naples, and is not placed in the Italian court, but in the Eastern Arcade, since it is sent by a private individual, and not through the Italian Government. The remarkable feature about this collection is the extraordinary beauty of the specimens in respect of preservation. Every naturalist is aware of the difficulty of getting such creatures as polyps, jelly-fish, and Salpæ to retain when placed in a preserving fluid anything like a satisfactory semblance of their living form and colour. To improve the methods of preserving marine organisms for museums and the workshops of comparative anatomists has been for some years one of Dr. Dohrn's objects in the work of his "Station," and this collection shows how far he and his assistants have succeeded in devising methods. To appreciate Dr. Dohrn's success, we have only to pass to some of the other collections—very good in their way, and showing the best state of the bottling-art out of Naples—and by the inferiority of the condition of the specimens in the latter we learn Dr. Dohrn's merit. Sudden killing with saturated solution of corrosive sublimate and gradual transfer to strong alcohol is one general method used at Naples for retractile polyps and fragile worms; brief immersion in weak chromic acid before transfer to weak spirit is another method used for jelly-fish and mollusks; narcotising by aid of tobacco-fumes another device. But the skilful application of such ingenious processes variously appropriate to this or that kind of animal can only be satisfactorily learnt in the Naples laboratory itself. Accordingly Dr. Dohrn has made arrangements for giving special instruction in this subject to naval officers and others, such persons being admitted for a fee paid by the Governments to which they belong, to a three months' course of instruction in the preservation of marine organisms for scientific purposes. Already, much to the credit of the naval departments of their respective Governments, both German and Italian officers and navy surgeons have been sent to receive such instruction at Naples, and a collection of coral-polyps and Siphonophora has been received from Monte Video, prepared by an Italian officer who had availed himself of the Naples course of instruction. This collection has been pronounced superior in condition and fitness for study to any collection from tropical waters hitherto brought to Europe. A second collection made by the same officer in Magellan Straits is on its way to Europe. There can be no doubt of the very great value of the new line of activity which Dr. Dohrn has traced for the Naples Station.

The Naples exhibit contains some interesting fish and a particularly fine series of Salpæ, of Mollusks, and of Anthozoa and Medusæ. It should not be allowed to return to Naples, and we believe is offered for sale. Dr. Dohrn also exhibits the publications, comprising many beautiful coloured plates, of the Naples Zoological Station. The series of volumes illustrating the "Fauna and Flora of the Gulf of Naples" should be in the library of every lover of natural history.

Second to Dr. Dohrn's collection, but of value as a complete *local* collection of all classes of marine animals, is that from the Gothenburg Museum exhibited by Mr. Oscar Dickson. The most interesting specimens here are several series illustrating the development of Pleuronectid and other fishes.

A very interesting general collection is exhibited by the Government of New South Wales, in spite of some mishaps to bottles in the course of a long journey. The Sydney Museum promises to become one of the grandest zoological institutions in the world, the colonial Government having appreciated the unique interest attaching to the natural history of the Australian continent, and wisely having determined that what money can do to build up in Sydney *the* great illustrative collection dealing with that subject, shall be done. Though not exhibited as examples of preservation as are Dr. Dohrn's series, nor labelled and identified with neat accuracy as are Mr. Dickson's, yet the Australian collection now at Kensington is of great interest to the professional zoologist, comprising many marine invertebrates as yet undetermined. It is under the charge of Mr. Ramsay, the accomplished curator of the Sydney Museum, who has brought over some of the reserve stores of the collection under his charge.

The American exhibit has the advantage of being the actual permanent collection of the National Museum of Washington, which has come into existence under the combined auspices of the United States Fisheries Commission and the Smithsonian Institute. The whole collection is not here, but we have a considerable part of it. For example, an admirable series of coloured casts of the fishes of the American waters, lifesize reproductions of the gigantic Octopus and Architeuthis, a complete series of the Crayfishes (Astacidæ) of North America and of the edible Crustacea generally, and samples of the more remarkable forms of life obtained in deep-sea exploration off the American coast.

In relation to the deep-sea specimens, we cannot but regret that no collection is shown in the Exhibition illustrating the results of the *Challenger* and other exploring expeditions conducted by the British Government. No such collection has, we believe, ever been presented to the inspection either of the general public or of professed zoologists, and the present would have been a very suitable occasion for such an exhibition. Whilst the Americans have taken the trouble to send across the Atlantic the newest dredging and sounding apparatus devised and employed by Agassiz, Sigsby, and others in their recent explorations of the deep-sea bottom, no such exhibit on the part of our own authorities is to be found.

In the Canadian department there is no general collection of any scientific importance, but amongst the zoological specimens, which (so far as the Invertebrates are concerned) are nearly all erroneously named, badly preserved, and unintelligently arranged, are some which are noteworthy. A bottle four feet high from British Columbia contains several specimens (nearly putrid) of the remarkable Pennatulid *Osteocella*, with flesh and polyps attached. Some ten years ago the calcified axes of these Alcyonarians excited considerable discussion in England, being mistaken by an eminent zoologist for the notochord of an unknown fish. In another bottle is a fine specimen (not labelled) of the very rare *Cryptochiton Stelleri*, whilst in one of the table-cases is a very large and probably new Hexactinellid sponge.

The collections from British India are remarkable, as comprising the important collection of Indian fishes belonging to Dr. Francis Day. The invertebrate collections are also extensive, but are not fully named.

As a general collection illustrating the British fauna of a certain size and important in relation to the food of fishes, should be mentioned the exhibition of living microscopic organisms by Mr. Thomas Bolton of Birmingham (in the Western Arcade). From day to day various living marine and freshwater Crustacea, Worms, Polyzoa, and Hydroids (also oyster-spat and newly-hatched fishes) are shown in small aquaria and under the microscope by this enterprising and meritorious naturalist. A complete collection of the drawings issued by Mr. Bolton to the subscribers to his weekly "microscopic tubes" (concerning which our advertisement columns may be consulted) is also exhibited.

Special collections dealing with particular groups of animals are to be found scattered in the various foreign and British courts. A collection (preserved in alcohol) of freshwater Crustacea of remarkable completeness is exhibited by Dr. Lilljeborg in the Swedish Court. It comprises most of the species of Cladocera and Copepoda, which inhabit the great Scandinavian lakes and serve as food to fishes. Recently some remarkable species of Cladocera identical with these have been discovered by Mr. Conrad Beck in the lakes of Cumberland, and the collection now under notice has been purchased by two English naturalists to assist them in identifying the species present in the Scotch lakes, which they intend to explore immediately.

Aquatic insects and their larval forms have a special importance for fishes, since the larvæ often feed on young fish or fish-eggs, whilst the adult insects are preyed upon by the adult fish. In the Swedish department there are two interesting collections of such insects, and in the American department are two sample cases from the great collection of Prof. C. V. Riley, which exhibit in the most complete way both by actual specimens and adjacent illustrative diagrams the various phases of life of a few insects the larvæ of which inhabit the water. There is no serious attempt by any English exhibitor to deal with this subject.

In fact most of the English zoological exhibits come under our Classes 2 and 3. There are most complete and valuable collections dealing with the growth of the oyster and the various conditions affecting it, as encountered by the oyster-culturist. The exhibit of Mr. Fell Woods is the most important of these. Mr. Henry Lee shows a very pretty series of oyster-shells and pearls in relation to their importance in the manufacture of ornaments, buttons, &c. As trophies, we cannot pass without a word of admiration the gorgeous cases of corals, sponges, starfishes, and sea-mats exhibited by Miss Gardiner. They include finer specimens and a greater variety than either the trophy exhibited in Lady Brassey's name, or in the series from the Bahamas, which are well worth inspection. It is only proper that a protest should be entered here in the pages of a scientific journal, in referring to the

Brassey collection. Some of the specimens appear to belong to a dealer, Mr. Bryce-Wright, and to these and others he has assigned names as though he were a serious zoologist. This travesty of science should not have been permitted. The names attached to the specimens are either incorrect applications of existing names or are gratuitous inventions (as for instance that of *Brasseya radians*), which can only mislead persons not specially acquainted with the history of corals.

Amongst the gigantic lobsters, clams, and stuffed fish there are some few small collections of scientific merit in the British exhibit. Dr. Francis Day shows a series of British fishes (alcohol specimens), Prof. McIntosh of St. Andrew's some coloured drawings of marine animals admirably executed by his sister, and a series of specimens of the salmon at various stages of development. Dr. Traquair of Edinburgh shows some exquisite drawings of fossil fishes, and H.R.H. the Duke of Edinburgh a collection of shells, scientifically named and arranged.

The parasites of fishes are not well represented in any part of the Exhibition. Dr. Spencer Cobbold shows a small collection of internal and external parasites, and a still smaller series (having, however, some special interest) is to be seen in the Russian court, where also the naturalist should not fail to study Dr. Grimm's important collection illustrating the fauna of the Caspian Sea. The most remarkable exhibit in the way of parasites is that of Dr. Antonio Valli of Trieste, who shows (in the Austrian Court) a collection of eighty-five specimens of Copepod Crustacea parasitic on the fishes of the Adriatic, accompanied by drawings and descriptions.

Curiously enough there is next to nothing in the Exhibition illustrating the diseases of fish. Some stuffed salmon with cotton-wool attached in patches to the head and fins do duty for the "Saprolegnia disease," and a not too accurate drawing of the Saprolegnia itself is exhibited in a part of the building which is about a quarter of a mile distant from the stuffed specimens. In a third locality is a cast of a salmon with cotton-wool also gummed on to represent "the disease," and near it an insufficiently stuffed skin of an old Kelt, which is offered as an example of the effects of "the arrow-headed parasite."

In the space occupied by Chili, China, and the Straits Settlements some specimens of fishes, and of shell-fish, corals, &c., are shown, which are not however scientifically named.

Finally, we would direct the reader's attention to two peculiarly interesting branches of fishery which are represented, though very poorly, in the present Exhibition. These are the sponge fishery and the coral fishery: the pearl fishery appears not to be represented at all. Collections of economic importance, showing the mode of diving for sponges in use in the Levant, and samples of Turkey sponges are shown in the Greek Court by Messrs. Marks and Son. By mistake (as seems probable) a specimen of Hyalonema, from another locality, has been placed in the case containing this fine collection of officinal sponges. From the Bahamas samples of commercial sponges are sent, and also (of very similar quality) from Florida (in the American Court). The propagation of sponges by cutting is illustrated by two specimens in the American collection, but no attempt is anywhere made to show the officinal sponge in its natural state, or to illustrate its life-history and distribution.

Similarly as commercial products we have the precious coral exhibited in the Italian Court by Signor Criscuolo. This exhibitor, however, also shows the method of dredging employed in the Gulf of Naples for obtaining the coral, and displays a number of the wooden cross-bars with stone weights attached, and hempen tangles depending, which constitute the instrument used in this fishery. Specimens of other corals and shell-fish found in association with the red coral are also exhibited.

In no exhibit is there any attempt to illustrate the natural history of the precious coral, although its interest is no less than its value.

A strange illustration of the chance uses of such an exhibition as the present may be found in the Japanese department. Nothing could be worse or more unworthy than the Japanese exhibit. It consists of some sardines, a large crab (*Macrocheira*), *three pieces of red coral*, and some silks and lacquer work. The three pieces of coral are the first commercial examples of a new species of precious coral which will henceforward form an important article of trade for Japan. They have been purchased by Signor Criscuolo at a high price, and are said to be of the very finest quality. The new Japanese coral fisheries are destined to make the fortunes of those who first set them going, and will very possibly seriously injure, if they do not ruin the Neapolitan fishermen. Similar precious coral may in all probability be discovered by dredging operations on the shores of one or more of the numerous British colonies.

On a future occasion we shall publish some notes by Prof. Giglioli of Florence, on the whales, seals, birds, and fishes now to be seen at the Exhibition.

PRECAUTIONS AGAINST CHOLERA

EARLY in the month the Local Government Board issued an Order to Port Sanitary Authorities conferring upon them special powers with a view of preventing the importation of cholera into this country. But cholera is a disease having many degrees of severity, and although "choleraic-diarrhœa" is to be regarded by the Port Authorities as synonymous with the fully developed affection, yet it is at times so mild that it may at any moment escape detection, and those suffering from it may make their way into our towns and villages. To meet such emergencies, and by way of aiding inland authorities and private individuals to rid their districts and their homes of the conditions favourable to the propagation of the cholera infection, a Memorandum on the Precautions against the Infection of Cholera has just been issued by Dr. Buchanan, F.R.S., the chief medical officer of the Local Government Board. The document, whilst expressing no opinion as to the channels of infection and the means of favouring the spread of the disease in other climates, declares with confidence that in England cholera is not infectious in the same degree and manner as are small-pox and scarlet fever, but that the matters which the patient discharges from his stomach and bowels contain the poison, and that their peculiar infectiveness is favoured by special local conditions which give the disease

facilities for spreading by "indirect infection." To get rid of these conditions should be our special aim at such a moment.

We have already pointed out how the poisonous discharges infect all receptacles into which they may be received and which tend to retain them, such as cesspools, sewers, and drains; how, when these receptacles are leaky, the soil around them becomes infected, leading to the pollution of air and of the water-bearing strata; and to a less extent it must be remembered that clothing and linen which have become soiled by these discharges are in a similar way liable to retain the infection. But of all these sources of infection none are so dangerous as those which are liable to infect our public water-services; indeed single attacks of cholera in its slightest form may, if the discharges can by means of streams or otherwise reach our water-sources or reservoirs, "exert a terribly infective power on considerable masses of population." Measures of cleanliness, taken beforehand are, according to the Memorandum, of far more importance for the protection of a district against cholera than removal or disinfection of filth after the disease has actually made its appearance, and even if cholera fails to spread to this country all action taken in this direction will, by preventing disease and ill-health from other causes, in the long run turn out to be remunerative.

Immediate investigation as to the wholesomeness of water-services should be made. The sources and the reservoirs should be examined by the authorities; intermittent services should, as far as possible, give way to constant supplies; cisterns should be kept scrupulously clean, and above all the waste-pipes leading from them should be so contrived as to flow in the open air. All accumulations of filth and house refuse should be removed regularly and at frequent intervals from the proximity of dwellings; house-drains and waste-pipes should be well ventilated, and so disconnected from the main sewers as to prevent the possibility of air from the public culverts from making its way into them. Action in these directions will do more to save households from infection than all the quarantine measures ever devised, and it is the absence of such action that has enabled cholera to spread itself broadcast throughout Egypt, notwithstanding the rigid measures of quarantine that have been adopted in that country.

THE LIFE OF EDWARD HENRY PALMER

The Life and Achievements of Edward Henry Palmer. By Walter Besant, M.A. (London: Murray, 1883.)

THE tragedy of Palmer's death gives his biographer the right to look to a wider circle of readers than would in ordinary cases feel interest in the life of an Oriental scholar and explorer. Mr. Besant has used his opportunity with the skill of an accomplished story-teller. Those who have dipped into the author's imaginative works will quickly recognise the familiar methods of art by which the reader's interest is sustained and carried on, the whole narrative disposed so as to lead up to the final catastrophe, and the figure of the hero invested even from childhood with something of an unearthly glamour. This method of treatment is a little disappointing to those who do not need to have their interest in Palmer stimulated, but only wish to learn as much about him and his work as possible; but it is fair to remember on the one hand that Mr. Besant is no Orientalist, and so naturally looks at Palmer's linguistic achievements through a mysterious haze, the effect of which is very artistically imparted to the reader's mind, and on the other hand that the exceptional nature of Palmer's powers, and the exceptional course of education in which these powers found their fitting development, are really calculated to stir the sentiment of wonder which the biographer has chosen to make the keynote of his book.

Palmer's linguistic talent was not analytical but mimetic; it was associated in his youth with histrionic tastes; and the love of mimicry, as Prof. Nicholl has well observed in his appendix on "Palmer's Work as an Oriental Scholar," had a large part in his literary compositions in Oriental tongues. It was through the mimetic faculty—not ot course by mere vulgar superficial mimicry, but by a childlike gift of sympathy and imitation—that Palmer learned languages. His teachers were men, not books; and when he learned Arabic, for example, he did not merely learn grammar and vocables, but acquired the power of thinking and expressing himself like an Arab. When he spoke or wrote an Eastern tongue he seemed to be for the time a real Oriental; to hear him recite Arabic was to feel one's self carried back to a camp in the desert. The talent, or rather the type of mind, which all this implies is very rare in the West; in the East it is more common, though hardly in the perfection in which Palmer possessed it; and this perhaps is the reason why Oriental languages ultimately became the study of his choice. His gifts put him in thorough sympathy with the tastes and aims of modern Oriental scholarship; it was the later models of Eastern literature, themselves imitative and full of dexterous variations of fixed themes rather than of original ideas, that fascinated him and called forth his powers in not unsuccessful rivalry with the best native writers of the day. The precise character of Palmer's scholarship cannot be expressed by a single Western term. He was more than a linguist and yet less than, or other than, a scholar of the Western type; for he was singularly destitute of the critical faculty which we esteem inseparable from scholarship. He was in a word an Oriental *Adib*, a man who loved language for the feats that could be done with it, and not for the ulterior scientific purposes which are the chief concern of most Western Orientalists.

Mr. Besant does not seem to have clearly grasped the peculiar type of Palmer's learning. He sees that he differed from most Orientalists; but he has the curious notion that the difference lay in a sort of grammatical pedantry which Palmer lacked, and to which other men give undue importance. That of course is purely imaginary. Palmer more nearly perhaps than any other Occidental who ever lived realised the Eastern ideal of literary culture. But the best Western Orientalists have been great just because they had a different and, it must be added, a more fruitful conception of the aims and uses of linguistic knowledge than the East has attained to. In criticism, in comparative philology, in the use of language to throw light on the past history of our race, Western scholars have solved problems which the most accomplished Oriental never even contemplates, and in

this department Palmer, true to the masters and models from whom he drew his lore, never excelled and never even showed much interest. His history of Jerusalem, his introduction to the Koran, and writings of a similar class, on which Mr. Besant lavishes praise as freely as on his really marvellous exploits in other lines, are disappointing performances; and it is extremely unjust to his memory to speak of them as if they displayed any part of his real strength. The same want of discrimination appears in a more unpleasant form in the querulous tone which runs through the book and represents every honour conferred by his University on other Orientalists as a gratuitous insult to Palmer's reputation as a scholar. The University was certainly happy which possessed in its two Arabic chairs men like Palmer and Wright, so different from one another, yet each unrivalled in his own line. But it is absurd to fasten a charge of unfairness on the University because in the candidature for the Adams chair it preferred the senior scholar. For the maintenance of the scientific *diadoche* in the characteristic features of the modern European school of Semitic learning Dr. Wright had qualifications to which Palmer never pretended—*e.g.* a profound comparative knowledge of the dialects—and the choice which Mr. Besant ascribes to petty motives was made on principles obvious to all who knew the case, and received the unanimous approval of learned Europe. The personalities which disfigure this part of the biography are based on a perfect tissue of errors as to fact; and the groundless charge of intrigue brought against honourable names acquires all its plausibility from statements which with the smallest care might have been seen to be erroneous. The very year of the election is wrongly given—1871 for 1870—a somewhat important error, as in the earlier year none of Palmer's principal writings had appeared; the salary is given at 300*l.* instead of 70*l.*; the fellowship at Queens', subsequently conferred on Dr. Wright to facilitate the conversion of a non-resident into a resident chair, is represented as a bribe to induce Dr. Wright to be a candidate, whereas in point of fact the election took place without his knowledge or consent. That Mr. Besant's researches into the facts on which his interesting record is based have been very slight appears all through the book—he is for example unable to say positively whether Palmer wrote articles which have appeared *with another signature* in the "Encyclopædia Britannica"—but the carelessness of the bookmaker deserves a stronger name when it touches the honour of men who are still living, and with whom Palmer himself continued to maintain friendly relations after the "insult," as Mr. Besant calls it, which "never was forgotten or forgiven."

The life of Palmer, who learned so much from the living voice, and had a unique gift of adapting himself to every kind of human life, must have been rich in incidents of the most interesting and instructive kind. Unhappily he does not seem to have kept full record of these, and except in the account of his last wonderful journey from Gaza to Suez we seldom hear his own voice in this volume. The reviewer knows from his own intercourse with the gifted traveller that but a small part of Palmer's observations in the East was ever given to the world, and as he certainly had many jottings—at least in Arabic if not in English—there was some reason to hope that the biography might make important additions to our knowledge of a land and race in which science as well as literature has a deep interest. This hope has not been realised; little is added to our knowledge of Palmer's earlier travels except one or two striking anecdotes. Are there no note-books to be found which can still supply this blank?

One is sorry to find so many grave faults with a book which after all gives a brilliant if not a discriminating picture of a very remarkable and attractive character; and it would be wrong to close without a word of thanks for the history of the heroic task, undertaken in no foolhardy spirit but in a spirit of courageous patriotism, which cost Palmer his life and England one of her most brilliant sons. Many points in the tragedy still remain obscure; but enough has now been set forth to leave upon the reader a profound impression of the intrepid bravery, the ready resource, the genuine devotion to duty, which, still more than his rare gifts of intellect, will keep the memory of Palmer green in the hearts of a people which prizes true manhood above the profoundest learning.

W. ROBERTSON SMITH

ANTS AND THEIR WAYS

Ants and their Ways. With Illustrations, and an Appendix giving a Complete List of Genera and Species of the British Ants. By the Rev. W. Farren White, M.A., M.E.S.L., Vicar of Stonehouse, Gloucestershire. (London: The Religious Tract Society, 1883.)

ANT literature is now so extensive and the subject is so popular, that it was an excellent idea to give in a handy volume a *résumé* of all that is known of the economy and life-history of these interesting insects. The writer is well fitted for the work, having made ants his special study for more than twenty years, during which time he has observed in their native haunts nearly every species of British ant, and has been able to confirm some of the most curious facts of their social economy. Although full of detailed and interesting information, and containing the results of the most recent observations of Sir John Lubbock, Dr. McCook, Forel, and other writers, the book is written in a lively and gossiping style well fitted to attract the young and persons who are not usually readers of scientific works; but many will think that liveliness of style is carried too far when we find such sensational headings as "Political Demonstration in the Ant-world," "Funeral Rites," "The Ants at their Toilet," &c., &c.

Coming however to the original observations of the writer, we find him disputing the statement of Sir John Lubbock, that ants dislike light. He says:—

"That they prefer working underground is certainly true, and that they construct their chambers and passages out of sight is clearly established, and that they will not work against the sides of the bell-glass if exposed to the light is undoubted fact. But it is not, I believe, because they dislike the light, but because, for sanitary, educational, and protective reasons, it is necessary that their many chambers should be arranged at certain depths below the surface, and therefore at varying distances from the light of day."

He then goes on to record a series of experiments showing that ants are attracted to the sunlight and bring their young beneath its influence for the sake of the

warmth which accompanies it, and that in the same way they are attracted by the light of a candle placed close to the sides of the *formicarium;* the glass being warmed and becoming a source of radiant heat. The elaborate experiments of Sir John Lubbock, showing that ants preferred the red end of the spectrum and avoided the violet end, are all explained by their preference for the greater warmth accompanying the red rays, though he also thinks they dislike the effect of the chemical rays. His general conclusion is, that there is no evidence that they distinguish colour or prefer one colour to another, but that they always prefer warmth, and dislike the action of the chemical rays of light, while to light itself they have no objection whatever.

Mr. White reproduces from the *Proceedings of the Linnean Society* for 1861 a remarkable account of some Australian ants burying their dead in a methodical manner strongly resembling our funerals, and supports it by some curious observations of his own. In one of his newly procured nests there were many dead ants, which were carried up from below and placed against the glass. Three small card trays containing honey for the ants were placed in the *formicarium,* but instead of eat.ng the honey the trays were used as cemeteries, and in two days 140 dead ants were placed in one tray and 180 in each of the others. In another case he observed the ants burying the dead in subterranean cemeteries, the bodies being covered with earth and the passage leading to the vault being stopped up.

A good account is given of the various creatures found in ants' nests, such as the crustacean *Platyarthrus Hoffmanseggii,* the various species of beetles, some of which are never found elsewhere, and seem to depend on the ants for their subsistence, and the aphides which the ants actually breed for their own use just as we do cattle. Some ants have small colonies of other ants domiciled with them, apparently as guests or lodgers, while others capture the pupæ of distinct species and bring them up to work for them like veritable slaves. This extraordinary habit of slave-making is fully described in two very interesting chapters, and Mr. White is one of the few Englishmen who have been so fortunate as to witness the slave-hunters at their work.

We cannot better illustrate our author's style and his mode of viewing the subject of ant-economy than by quoting the passage in which he sums up the result of his observations and inquiries :—

"And now, surely enough has been said, ample evidence has been brought forward, my own personal testimony having been confirmed when necessary by the experience of others, to warrant me in earnestly demanding for my little clients a favourable verdict. When you bear in mind the self-devotion of the queen for the commonwealth ; the loyalty of her subjects, their affection towards their youthful charges, preserving as they do a happy medium between undue severity and over-indulgence ; their liberal system of education without the aid of privy councils and revised codes ; their plan of drainage, most effectual before boards of health and city corporations had ever been heard of ; their public works and national enterprises, planned and executed with the most surprising promptitude, uncontrolled by parliamentary committees, orders in council, and circumlocution offices ; their social institutions, their provident clubs and savings banks, gathering as they do their meat in the summer—the continental and foreign ants grain and honey, the British ants their aphides for future use ; when you bear in mind their perseverance under difficulties, that no poor-house or assessment committee or sanitary authorities are needed, for all live as brethren, all sympathise with each other in trouble and difficulty, and share everything in common as members of the same happy family, 'he that gathers much having nothing over, and he that gathers little having no lack ;' when you remember their habits of early rising, of cleanliness, of moderation, of economy, of temperance, their love of fresh air, their skill and industry in their many trades, the magnificent scale on which they construct their houses ; their language, which, though more difficult to acquire than Chinese, yet is to them so intelligible that there are no misunderstandings, all speaking it fluently, and by means of its mysterious agency communicating their ideas to each other ; when you recall how they carry out concerted plans thoroughly, noiselessly, uninterruptedly, not resting till their work be finished, animated by one spirit, pursuing thus the end, fulfilling thus the law of their brief existence—you must allow that surely this 'little people' are 'exceeding wise.'"

Though somewhat anthropomorphic and highly coloured, this passage brings before us in a striking manner the many marvellous characteristics of the habits and instincts of ants, and also serves to show the thorough and enthusiastic study which the writer has bestowed upon them.

The book is well illustrated with numerous woodcuts from original drawings ; and in an appendix is given a complete list of British ants with careful descriptions of all the species, forty-one in number. It will therefore be of great assistance to any entomologist wishing to commence the study of our native ants ; while as an interesting volume for the general reader, or as a gift-book for children with a taste for natural history, it may be safely recommended as among the very best of its kind.

ALFRED R. WALLACE

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts, No notice is taken of anonymous communications.*

[*The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to insure the appearance even of communications containing interesting and novel facts.*]

The Matter of Space

In his letter on this subject in NATURE (vol. xxviii. p. 148), Prof. Morris strikes, I believe, a keynote of very great interest in the general theory of motion, when he lays it down as a primary principle that all motion naturally tends to attain a condition of stationariness in which, though it still constantly springs or swings hither and thither, it is yet permanently localised in some fixed field, contained within definite inclosing boundaries.

Singular as the law appears that motions, bound and hemmed in as we see them everywhere around us, are only ostensibly confined to their spheres by combinations of directed forces, while they are really inclosed in them by a governing principle in matter which constantly models its directed courses either by continuous or by interrupted stages into forms of stationariness ; and strange as the statement sounds, that all matter thus tends constantly to form *in situ* veritable universes[1] externally re-

[1] A pamphlet, "The Universe, or the Science of the Twentieth Century," maintaining exactly this microcosmical theory (by what course of reasoning arrived at I cannot guess), reached me not long ago from a writer, Mr. John Tate of Portadown, in Ireland, with another ("A New Theory of Electricity,") describing electricity as a kind of twisting power, both of which, from the independent practicality of their treatment, seem to have been entirely prompted and suggested to the author by exact meditative study and by clear original reflections.

posing and quiescent, and internally passive, neutral, and indifferent to all surrounding material universes, yet I am disposed to concur with Prof. Morris in his emphatic enunciation and very appropriate and varied illustrations of this law, because the idea of established boundaries, prescribing fixed terms and limits to motor-vigour's local actions, has, in an investigation of the principles of thermodynamics which lately occupied me, already presented itself to me as an indispensable foundation for a theory of heat, in which temperature was identified with motor-couple's dual power of dispensing motor-vigour between ordinary and ether masses, partly by opposing undulatory, and partly by contending diffusive motions of ether's and gross-matter's aërilian parts.

Easily as that theory lent itself in other respects to a deductive establishment of the laws of heat, it yet stumbled abruptly upon this blank presumption, or frowning precipice, *of how* boundaries of the kind (to such forms of aërilian action) come to be established and imposed between ether and gross matter, as well as between material bodies generally, wherever superficial contact between their substances takes place?

Granting indeed, provisionally, that we may freely accept Prof. Morris's somewhat too simple, and in fitness for its purpose much too meagre and unassuming supposition (which I should also say that he errs in describing me at the beginning of his letter as being just as willing and contented to accept and conform to as he is himself), that "particles of ponderable matter consist of aggregations of ethereal substance," or that "ether is a substance whose condensation yields particled matter," it would then be making a step of inference which would neither be a positively ungrounded one, nor (supposing that nature's system were really such a simple one as this hypothesis assumes) at all a likely one to conduct us to any embarrassing or perplexing consequences, to describe the "excessively disintegrated matter" which in his *aperçu* of the retinues of space "replaces ether," as ordinary matter in a "fourth state" of attenuation; because we would immediately reflect that the boundaries between the solid, liquid, and vaporous forms of such a multistructured substance as ethrogenous matter would then be, are themselves well known to be the seats of a certain diffusive and undulatory struggle and balanced equipoise, the real nature of which, beyond what is known of its laws of relation to pressure, heat, and temperature, cannot be accurately described. The fact that temperature and tension regulate it does, indeed, assimilate it to the similar dual balance of motor-couple's diffusive and undulatory actions at the borders between ether and ordinary matter which I found to be indispensable as a first starting-ground for basing a mechanical theory of temperature, heat, and entropy on mathematical properties of motor-couples; and our ignorance of how the boundaries are established in each case is not only no greater, but it actually appears to be of precisely the same nature and description in one of these cases as in another.

The parts which collision and vibration play in distributing motor-vigour in solids, liquids, gases, and in ether, are abundantly well-instanced and described in Prof. Morris's letter; and it again affords me extreme gratification to note the exact parallelism which his views present with those furnished by a systematic and not perhaps altogether unmathematical treatment of the subject which I have pursued, if, as I surmise, undulation and diffusion are kinds of motor-action (both active in a motor-couple) of such primitive simplicity of construction in their agitational or motor-type, that, in virtue of their elementary mathematical character, one single mechanical explanation really suffices for and applies with equal exactitude to all those instances of material conflict just considered, which occur at the boundaries between the several gross and ethereal states of matter.

But both physical and mathematical considerations have besides this led me to suppose, as I trust that they may also in the end influence Prof. Morris's decision, that the title of the "fourth state" of matter which we might thus quite fairly at first sight and provisionally apply to ether, is in the all-essential meaning of the words an undeniable misnomer; because mutual conversion of the two substances composing the first three and the last of the forms in question one into the other is *bonâ fide* shown by the clearest evidence of experience, and equally by theoretical proofs based on the two substances' motor relations, to be, even more certainly than making gold out of copper, an impossible physical proceeding. With such plain reasons as I will try briefly to produce for pronouncing ether and ordinary matter to be perfectly distinct and totally untransmutable fellow-occupants of space, it is really more consistent with simple fact, and a more precise and correct use of language, to speak of ether as "matter of the second class" or of the second grade or order, than it would be to call it either dubiously matter "in a fourth form," or to give it the still more erroneous title of a "fourth state of ordinary matter."

While, in fact, we know innumerable chemical and physical forces capable of altering to any give-and-take extent the boundaries between liquids and their vapours, between similar and dissimilar solids and liquids, and like and unlike gases and molecules, so as to change entirely all their physical and chemical states, or groupings, yet no force of art or nature can make any portion of gross matter change its weight by condensation or escape of ether. Even chemistry, to whose reactions Prof. Morris assigns the greatest power of altering molecular groupings, although tested in this direction with the delicacy of a vacuum-balance in Mr. Crookes' researches, has been found to be powerless to do so. It is true that its reactions only employ the sedatory tendency of motion in order to produce new groupings, and the electric current, which first disclosed the existence of the elements sodium and potassium, and whose arc of light gives us glimpses of chemical dissociations scarcely less complete than those detected by the spectroscope in the sun, overcomes and reverses the power of chemical affinity to form combinations in this way more effectually than any other force, and breaks up all chemistry's compound productions more completely than any other force can do. Yet, while no dissipation of weight of ordinary materials by electric currents has yet been detected, it is just as certain that ponderable matter has never yet to our knowledge gained or increased in weight in virtue of the exertion of any possible chemical affinity which it may have for ether, although this affinity, if it exists, must yet be of extraordinary strength, since it can successfully resist every effort that has yet been made to loosen it! Either imponderability of ether or immutability of its boundaries of junction with gross matter, or both of these together, must therefore be assumed to account for the sum of this experience; and whichever of the alternatives we are led to choose, distinctly differentiates the two substances from each other as regards this particular character of mutual convertibility of substance, for no known ordinary matter arising from ether's condensation is imponderable, or, on the other hand, if ether has weight, experience still shows that no condensation of it into ponderable ordinary matter is possible.

Another conspicuous peculiarity of ether consists in a special independence between its motor-vigour and that of ordinary matter, of which instances of the plainest proof are afforded by Doppler's theory and by the theory of the aberration of light. The motions of ether in an ether-replenished field are not in the least degree affected by the directed motion across it of a mass of ordinary matter, just as a perfectly smooth anchor would leave no permanent agitation whatever behind it in water or liquid inwardly and outwardly as smooth as itself, through which it takes its way. It is only by such a passing body's aërilian or undirected motions that ether can be disturbed, and with those it harmonises or collides, mutually receiving from and imparting to the body it so touches motor-vigour (which may either take the form of actual heat or of stresses in the ponderable body) by the primitive aërilian processes of wave-impact and diffusion-blows of the two substances at the boundary between them. With the absence of these (if we could imagine the privation to exist) the bodily or directed motion of the two substances, like those of a smooth anchor swinging in a stream of frictionless water, would all the while be wholly unaffected by, indifferent to, and independent of each other. The ether therefore stands in such motor-relations to gross matter that the two can only exchange motor-vigour with each other by means of the aërilian impulses of their touching parts.

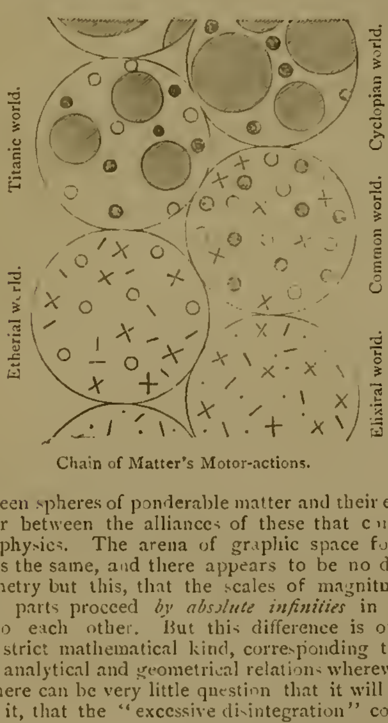
Now this theory of ethereal action, suggested to me by an accidental consideration of the well-known mathematical equation of stationary motion, which was at once seen to furnish, on closer examination, a very consistent interpretation of the second law of thermodynamics, and of its several thermal quantities, led me to describe in my former letter (NATURE, vol. xxvii. pp. 458 and 504) some of the necessary postulates or maxims of the new theory in its integrity of fitting enunciation for such applications.

If the mutual motor-relations between ether and gross matter are indeed (as I have very full grounds for confident assurance) of the extraordinary nature and description there set forth, there seems to be no room to pause or to waver and hesitate over nicely raised but unavailing protests of prejudice and predilection in

their contemplation. In the ocean of universal ether are described baric points and masses "nestling" together, and "nestled" in their attendant or "bound" ether ones, which themselves cluster or "nestle" like atmospheres about them. Each such ethrobaric assemblage is a universe, when in repose, independently of the unbound and unbounded ether-ocean, which alone stands aloof as a universe by itself. And among all these the instantaneous as well as the hare-and-tortoise-footed paces of time take effect, and swiftly or gradually, along with many other actions, cluster the island-masses together more and more.

By what rigid cord the clustering tendency to establish certain boundaries is controlled, what struggle for existence gave their present forms to elements and suns and planets and to the ether atmospheres belonging to them, appears to be a question of just the same cyclopean vastness, and in some measure of the same description, as that which presents itself to our inquiries in animated nature. And since it is exactly this ruling rein which sets the boundaries to bodies, no harder problem can perhaps be contemplated than that of defining how, at a point of contact, the boundary between two dissimilar physical bodies is preserved.

In particular the contact of our physical world of ethrobaric alliances with universal ether, where to us complete and perpetual silence reigns, and in the other direction of inconceivable hugeness instead of smallness of integration, common ethrobaric matter's contact with a universe just as conservative as ether is of suns' and galaxies' corporeal struggles, but in this case beyond the ken and vision of the most gigantic telescopes, are probably *par excellence* the seats of strife and contest of all or at least of many more orders and successive grades of matter than take part in



Chain of Matter's Motor-actions.

those between spheres of ponderable matter and their ether atmospheres, or between the alliances of these that constitute our world of physics. The arena of graphic space for all these universes is the same, and there appears to be no difference in their geometry but this, that the scales of magnitude of their dis-integral parts proceed *by absolute infinities* in their proportions to each other. But this difference is of such an essentially strict mathematical kind, corresponding to precisely equivalent analytical and geometrical relations wherewith, sooner or later, there can be very little question that it will be possible to express it, that the "excessive disintegration" contemplated by Prof. Morris is really one of infinite disintegration. And what it is which sets bounds to the universal ether by itself, so as to make it a third party to the exchanges of motor-vigour between the bound intergroupings of gravitating and ether-matter (and perhaps a shaping and forming link of these to the larger-statured universe out of reach of telescopic vision), unless it is a substance of more infinite disintegration still than ether, an elixir of ether as we may style it, shaping and forming both that ether itself and its alliances with baric matter, it would certainly be exceedingly difficult to say.

Thus in the above figure it will be seen how boundless graphic space (denoted by the inclosing circles) may all be filled and occupied at the same time by a continuous chain of matter-triads consisting of matter in innumerable different untransmutable grades of fineness of disintegration, of which only three adjoining ones are physically concerned together in any one of the linked world-systems of the chain's horizons, in producing that world-system's or horizon's natural phenomena. The functions of ether and "elixir," for example, can be traced in the figure in giving inanimate nature its form and stature, and in producing its physical phenomena in the world of ordinary or common matter; and that of common matter and ether, again, in doing the same in the larger-statured or "Titanic" world; and so on for worlds of vaster, or *per contra* of finer, textures than our own.

But as I reasoned at some length to show in my former letter, a proper branch of geometry must be specially developed and explored to describe even the space-relations of these several material horizons to each other clearly; and there is besides this the part which time plays in the control and evolution of motor-actions by their transmission from one horizon to another, to be investigated and considered, of which it can hardly be foreseen that the research will be easier, although sure in due course to be prosecuted successfully, than the investigation of the geometrical relations.

It cannot therefore be expected that the beginnings of physical phenomena like those of light, heat, magnetism, and electricity (and of chemical phenomena in addition), due to motor-vigours of imponderable substance, should all be easy to fathom and unveil at once. But very grateful reception and approval must yet be freely and fairly accorded in the meanwhile to such able and successful attempts as Prof. Morris makes and proffers in his letters to unravel them, as being unquestionably of very great present, and of incalculably greater prospective use and value to assist in pointing out the right road and in paving the way towards their final elucidation. A. S. HERSCHEL

Newcastle-on-Tyne, June 25

P.S.—A little more inquiry shows me that it is not essentially in absolute size, *but in volume-density* of the integrant parts, that "titanic," "ethereal," and other kinds of matter differ by infinity, and by infinity beyond infinity, from common ponderable matter. An integrant part or "atom" of common matter, for instance, becomes by infinite expansion[1] an infinite-sized network of extremely far-separated (countlessly numerous) titanic matter-atoms, whose expansion will have rendered them all ordinary substance and will have raised all their internal constituent atomic parts, like themselves, one grade in attenuation; while the original common-matter atom itself will not in the least degree lose its individuality by its enlargement of stature, but will become at the same time an infinitely large common-matter, and an infinitely large ether-atom. The titanic members also, although an infinite-fold larger and less dense than titanic atoms of a mean size, do not lose their proper relativities with their normal-sized fellows, although they acquire a new consociation by assumption of a lower density, with atoms of common matter; so that exchange of energy or of motor vigour by the ordinary processes of diffusion and wave-motion can in these circumstances subsist between ordinary and titanic matter on a footing of equality. And it is the same, in the common-matter atom's state of ethereal hyper-attenuation for its exchanges of energy and momentum with ether-atoms of the next higher order *of magnitude* than those which we call mean-sized.

To how many successive grades such hyper-attenuation may be carried there is no actual evidence to show, but in the system's theory itself there is nothing to restrict it. We must only remember that each successive grade is an infinite step onwards in expansion or contraction; and since common-matter's atoms or first integrant parts are known (as Sir W. Thomson has most clearly shown, NATURE, vol. xxviii. pp. 203, 250, 274) to be of finite, though of excessively small dimensions, their hyper-attenuated forms are of an immensity whose size is mathematically infinite, and we cannot therefore point to them. A single common atom's transition-form to ether-density pervades all visible space. Its transition-form to "titanic" density occupies no visible space at all, and is graphically a material point, although entropically it is infinitely composite; and the motions of each of these forms are absolutely invisible to us, but not less real and effective in their contributions of motor vigour to ordinary matter at the confines of its contact with ether megaspheres, and with titan micro-points in graphic space.

[1] If time is allowed any homogeneous assemblage of matter-atoms to equalise their temperatures, the whole assemblage and its parts, consisting of common and of remoter matter-grades, will, I conceive, all have one and the same common rate of volume expansion (as described above) to whatever extent, finite or infinite, the expansion or contraction is continued at one settled temperature.

However many times material atoms may be hyper-attenuated or condensed, their substance no doubt retains its original material *status*, although removed by numbers of grades or orders of attenuation from it to which the mathematical principles of the theory assign no limit; and boundless space is thus strewn at once with a grade of common-matter atoms, which in their original *status* may have properly belonged to any other grade of unknown remoteness. But this fixity of matter's original grades of size and density with only infinite insulations from other grades, is not more notable than the unrotativeness or fixed directions of some coordinate axes of mechanical motions in space which does not prevent the motions from being just as perfectly describable by the selection of any other equally fixed ones. We are in the same way unable to say by how many revolutions the hands of a clock have reached a certain position on its dial, unless we examine and properly employ to estimate it the state of wear and attrition of the wheel-work of the clock's driving train, or unless we know the number of times that the clock had been wound up.

The solution of some very bewildering physical questions is offered by this hypothesis[1] when we reflect, as I have before endeavoured to explain, that the expansions here considered are all of them variations of a quantity ϕ (or "entropy" of a homogeneous body at the same temperature throughout), which, by its mathematical description, is obviously the ratio index of a describing point's place upon a hyperbola, and which therefore passes continuously through an endless series of values 0 and ∞ (which revisit each other in graphic space, just as a circle-radius revisits its former place after every passage through four successive right angles), while the describing point pursues the curve continuously.

There is enough evidence in geometry to show that this hyperbolic variable of position, and the angular one on the hyperbola's auxiliary circle of a certain configurated point on that circle, cannot pursue their geometrically configurated course together through more than a quadrant of the circle and hyperbola from the two curves' common apex without violating the axioms of ordinary geometry. Thus it is clear that in the transition state of the measure ϕ through infinity from one "grade" of a mass's state of attenuation to another, there is needed a new law of geometry (or at least of continuous material motion) allowing a new pair of tracing-points supplanting the disused former pair at each dead-point of the two curves, to describe a new quadrant of the hyperbola and of its auxiliary circle from that point, with a constant geometrical configuration to each other without violating geometrical axioms.

This transition law and the nature of the configuration which it frees from geometrical contradictions while giving it continuous validity round the whole circuit of the circle and hyperbola together, is so exactly what has just been described of the nature of material points' or of physical integrant-parts' compositeness while still remaining points in their motor properties, that almost all reason for doubt and question seems to be excluded that it is the sought-for law and mode of motor connection between θ and ϕ (or angle- and entropy-position of a point or homogeneous body), which links universal heat-motion of matter to all those other, no doubt therefrom derivable but otherwise unaccountable descriptions of matter's motions which we see in physics.

[1] In particular, as will be easily gathered from the above brief comments, of the law of dissipation or of a fixed tendency to gradual reduction and to universal uniform diffusion of all forms of energy *in a given link* of matter's grades in one common form of the energy of heat, or of the work of entropy-expansion.

On Lord Rayleigh's Dark Plane

In Nature, vol. xxviii. p. 139, was printed a communication from Lord Rayleigh to the Royal Society on the subject of the dark plane which is seen above hot bodies in dusty and illuminated air, and which had long been used by Tyndall, and after him by science teachers generally, as an illustration of the fact that light which does not enter the eye cannot be seen.

It had never occurred to me to doubt the validity of the commonly-received explanation of the dust-free space, viz. that the dust in the dark region had been either burnt up or dried up by contact with the hot body, and I was struck and greatly interested in the definite character of the phenomenon as described by Lord Rayleigh in your pages, and in his conclusive shattering of the old explanation by the simple device of using a cold body instead of a hot, and so getting a down-streaming dust-free space instead of an up-streaming.

I was however quite unable to accept Lord Rayleigh's very tentative hypothesis that the curvature of the stream-lines and consequent centrifugal actions might possibly account for the phenomenon, nor do I imagine that he himself ever regarded this as anything more than a guess thrown out for want of a better.

I mentioned the matter to Mr. J. W. Clark, whose services as Demonstrator I have lately had the good fortune to secure, and he proceeded to make a few simple experiments with a view first of repeating the observation, and next of testing an electrical hypothesis which suggested itself.

The hypothesis is one that has failed to verify itself, but it may be just worth stating. The difference of temperature between the solid and the air causes convection currents, the air thus made to stream over the surface of the solid electrifies itself by friction, and the dust particles are expelled from the electrified air.

We were early led to doubt whether the insignificant amount of friction which alone was acting in some cases could possibly produce the effect; and in fact it was soon found that though electrification modified the phenomenon it pretty certainly did not cause it.

A doubt then arose whether the space was actually dust free or only optically so; whether anything like mirage due to unequal densities could account for the darkness. These ideas, however, would not bear consideration, and we soon convinced ourselves that the region is really transparent air free from dust, though its extreme sharpness and blackness render it difficult at times to refrain from thinking of it as a black opaque film.

Irregular dark striæ obviously allied to the regular dark plane are to be perpetually observed in any dusty air disturbed by convection currents; and nothing but the want of the necessary illumination prevents our commonly observing what must be one of the most universal appearances, viz. dust-free regions streaming from every solid body.

We are now pretty well convinced that differences of temperature have nothing to do with the real nature of the phenomenon; *we find that solid bodies have sharply-defined dust-free coats or films of uniform thickness always surrounding them*, and that these coats can be continually taken off them, and as continually renewed, by any current of air. The slightest elevation of temperature of the solid causes its dark coat to stream upwards; the slightest depression of temperature below that of the atmosphere causes the coat to stream downwards; but the coat is there all the time, independent of convection currents, though I believe it gets thicker as the body gets warmer. *Why* the air near a solid is free from dust we are not prepared to say.

A few of our earlier experiments might readily enough have suggested the old exploded explanation that the smoke was either burnt up or dried up or otherwise temporarily rendered invisible by heat. Take for instance a long piece of ordinary quill glass tubing; blow it half full of tobacco-smoke, and hold it horizontally in a beam of light. The first thing to notice is the curious way the end of the stream of smoke draws out to a point with a sharply defined edge, and how it falls about inside the tube when the tube is rotated. Next warm a part of the tube gently: a space clear of smoke at once appears and widens. Next heat the tube in the flame of a Bunsen and blow smoke gently and continually through it: the smoke narrows down to a mere thread as it passes the hot place, or it may disappear altogether in a pointed cone; but it reappears on the other side of the hot place, and it issues from the end of the tube.

Our experiments have been mostly conducted in a glazed cigar-box with one or more horizontal copper rods passing into it through insulating glass tubes, the ends of the rods carrying binding-screws into which could be clamped scraps of sheet copper of various shapes. The illumination was either sunlight or an oxyhydrogen lamp, or more usually, and far the most conveniently, a Serrin arc-lamp in its lantern, fed by a secondary battery. The smoke employed was nearly always tobacco, for we soon satisfied ourselves that the nature of the smoke or dust did not affect the essence of the phenomenon, and we consequently used that which was the easiest and for which the implements were always at hand. Sal-ammoniac was, however, occasionally used instead.

It was wholly unnecessary to heat the rod in order to start the dark up-current, for if it is not infinitesimally warmer than the air to begin with, the beam of light will warm it sufficiently in an instant. Still the rods can be heated by a lamp outside the

box if desired, or on the other hand their projecting ends can be bent down and immersed in a freezing mixture when a cold dark plane is wanted.

The transition from the cold down-current to the warm up-current is a thing I specially wished to observe, and it can be readily seen by first letting the rod get thoroughly cold in the dark and then turning on the light without removing the freezing mixture. The down-current is now visible, and it persists for a short time, varying from a second or two to a minute; but as the rod is warmed by the beam, it soon visibly slackens, turns round, and establishes itself as an up-current; the transition from a strong down- to a strong up-current only occupying a few seconds altogether. If the light be now interrupted for a short time, and then renewed, the down-current will be observed as before, and in fact one may make the alternations with great rapidity, permitting now the freezing mixture and now the hot beam to gain the mastery.

The turning bodily round of the dark plane is doubtless due to a general convection current produced by the warming of the glass of the box by the entering beam. The beam was, however, always filtered through water in order to bring its heating powers within manageable limits.

To witness the effect of a diminution of pressure on the phenomena, a thick platinum rod with its end beaten into a narrow spade, was sealed into an old lamp chimney closed by plane glass ends, and connected with a water air-pump on one side, and with a $CaCl_2$ tube, a tobacco pipe, and pinchcock on the other. A little exhaustion and an intermittent opening of the pinchcock was able to smoke the pipe in the orthodox mannner, and the exhaustion was then proceeded with further; an accumulation of vacuum being often quickly turned on. At low pressure the dust-free space surrounding the spade became large and ill-defined, and the convection-currents were lazy and ineffective; the exhaustion was not pushed to extremes, because it was difficult to keep any smoke still suspended, but the general fact that the dark region broadened considerably under diminished pressure was fairly well made out. The coat is enormously thicker, however, than any Crookesian or free-path layer.

When examining the effects of electrification we sometimes brought electrified rods near to the dark plane, and sometimes we electrified the rod from which the plane was streaming. The latter is by far the most effective, and the results are very striking and interesting. It is not sensitive to minute differences of potential however, and it required from fifty to one hundred Leclanché's to exhibit distinct effects. We then found that positive electrification of the rod rendered the coat and the stream broader, but made their outline hazy. Connecting the rod to earth instantly sharpened it up again, making it beautifully clear and distinct. Negative electrification sharpened the outline still more, and narrowed it down still further, but the effect of positive electrification was more marked than that of negative.

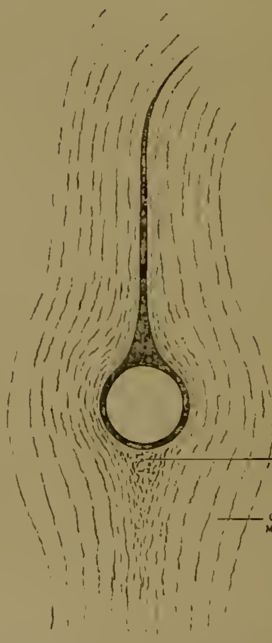
When comparatively high potentials were used, such, for instance, as would give millimetre sparks if permitted, the effects were violent. As the potential rose, the dark coat and stream broadened, and ultimately disappeared; reappearing again and closing in from each side in a curious way, so as to reestablish the clear dark plane depicted by Lord Rayleigh in his paper above-mentioned, the instant an earth contact was made. Violent negative electrification exhibits somewhat similar effects. If any brush discharge took place, there was a violent black chimney-like rush, and the whole box rapidly cleared of smoke.

The electrical effects are not easy to describe, but they are worth seeing. We sometimes used two solid brass cylinders with rounded edges screwed on to brass rods and insulated from each other, cylinders say three centims. by one; sometimes we used a cylinder and a point, sometimes two flat spades, and so on. Connecting them with the poles of a Voss-Holtz machine and turning very slowly, the change from two well defined and sharp Lord Rayleigh planes through an interval of indistinctness to vigorous and curiously-shaped black streamers is very striking. But in a few seconds all the smoke has gone; it has not been driven out of the box, it has been condensed on the box surfaces and on the electrodes, which latter soon look as if they had been lacquered by an amateur, and make yellow greasy marks on one's fingers.

Moderate positive electrification of the rod, then, widens and renders hazy the coat and the dark stream; earth connection, or still better weak negative electrification, narrows it and renders its outline beautifully sharp and distinct. The stream itself does not show signs of electrification. Obstacles in its path deflect it, and it curls round them, forming rather a pleasing stream-line illustration.

As soon as we had made out that the dark plane was continuous with a dark coat surrounding the body, we paid more attention to the coat than to the plane. It seems to me a somewhat important fact that solids have surrounding them a layer into which dust particles do not enter, of a thickness which we estimate as comparable with 1/1000th of an inch, though it certainly varies with temperature, pressure, and electrical potential. We first observed the dark coat as a lining to a semi-cylindrical scrap of copper sheet held in the binding screws formerly spoken of, with the hollow turned towards the light. It can be seen quite well, however, on a simple round rod or straight thick wire; and for many reasons this is preferable. To avoid the shadow of the rod and to see the coat all round it we return the light on its path by a mirror, often also illuminating from above by means of a 45° mirror.

When the smoke is thick a feeble light is sufficient, but I prefer a thin smoke and a powerful light. After tobacco has been in the box some time, say half an hour, the smoke particles have aggregated together and can be individually seen. It is then very instructive to look along the end of the rod through a low-power microscope. The diagram attempts to illustrate the appearance.



The coat of dust-free air is perpetually being rubbed off and renewed; the attachment of it to the rod is not individual. I believe all dark striæ seen in a smoky sunbeam are the wiped-off coats of solid bodies, which, however, are now rapidly disappearing by reason of the general diffusiveness of the dust particles.

The transparent coat on the inside of a glass tube full of smoke can be seen, and when a point is heated the coat thickens and rises, making a clear dark space, and then it proceeds to roll itself up along with some of the dust into two distinct spirals one on each side of the hot place.

I have no hypothesis whatever ready to account for the dust-freeness of the film of air in contact with solids. But I believe the existence of this film, and its electrical modification, to have a close connection with various phenomena; for instance, the easier discharge from negatively electrified bodies than from positive—the dust-free coat is thinner: the convective discharge of electricity by hot bodies and its dissymmetry as observed by Guthrie, the dissymmetry of the Lichtenberg tracings, the abnormal dielectric strength of thin films of air as observed by Sir Wm. Thomson ("Reprint," chapter xix.). For I imagine that disruptive discharge would more easily commence in dusty air than in clear air, and consequently that when the sparking bodies are approached so that their dust free coats touch, the dielectric strength is likely to be great.

Maxwell indeed suggested ("Electricity," vol. i. p. 56) that a layer of extra dense air equivalent to an extra layer of ordinary air about 1/200th inch thick surrounding solids would account for Sir Wm. Thomson's remarkable and puzzling results; and this is a dimension of the same order of magnitude as the thickness of the dust-free coat on bodies at an ordinary temperature. I by no means intend to imply that the dust-free layer is not composed of extra dense air—I have no evidence on the subject—but the dust-freeness may *possibly* account for its greater strength without the hypothesis of extra density.

The dust-freeness itself remains to be accounted for. Numberless experiments suggest themselves. We have not yet tried other gases even, though that is an obvious thing to do.

It struck me some time ago that the motes in a sunbeam would be convenient weightless bodies for many purposes, to exhibit statical lines of force for instance, but the particles of the smoke we have hitherto used have not been sufficiently elongated for this purpose. But I anticipate that the examination of all kinds of electrical phenomena in the strongest possible light, instead of in the dark as usual, may lead to various fresh observations.

The rapidity with which an electrified point clears the box of smoke is so noticeable as to suggest several practical ideas. It is somewhat surprising considering the perfection to which electrostatic machines have been brought that they have not yet received any practical application. The electrical clearing of the air of smoke-rooms, or of tunnels, is perhaps not an impracticable notion. The close relationship between fogs, epidemics, &c., and the suspension of solid particles in the air, suggests the use of electrical means for sanitation, and for weather improvement. It has long been known that lightning clears the air, and though ozone may be credited with a portion of the beneficial influence, I fancy the sudden driving away of all solid particles and nuclei must have a great deal to do with it.

If the germs driven out of the air are condensed on the earth's surface, a partial explanation is suggested of the way in which "thunder turns milk sour," a fact which has always puzzled me, and which appears to be well established.

I cannot help thinking that the human race will ultimately acquire some means of artificially affecting the weather in a less injurious manner than that which they have hitherto attempted with only too great success, namely, the manufacture of solid nuclei in prodigious numbers for moisture to condense round, and of oily matter to cover the surface of such moisture with, in order to prevent its evaporation. As soon as this artificial pollution of the atmosphere has been decisively checked, it will be time to consider whether it may not be possible to keep off even natural mists and rain when they are not wanted, and to assume some sort of control over the weather at critical seasons, instead of halting between superstitious appeals to Providence on the one hand, and a helpless resignation to fate on the other, which are our attitudes at present.

Meanwhile is it not possible that a periodic optical examination of the atmosphere by a strong beam of light might convey useful meteorological information? OLIVER J. LODGE

University College, Liverpool, July 11

Antihelios

BY means of a current of air passed through an ice closet or a closet otherwise reduced in temperature the air of living-rooms might be gauged to any temperature, but say 60° or 70° F. if we pleased. If the air were driven through a preliminary water chamber arranged on the principle of the hubble-bubble pipe, mosquitoes and other flying pests would be excluded absolutely. Imagine the comfort of sitting down to a meal whereat one's food should not be hidden by flying vermin, of reposing in a cool chamber wherein these intruders should be excluded absolutely. When I lay ill of fever in West Africa the atmosphere about me felt simply like the blast from a furnace. What an element of recovery, of possible health and physical wellbeing, would it not prove in hospitals when poor fellows languishing in disease should be surrounded by pure, cool, insectless air instead of air at a hundred degrees or even higher. People—some people—say doctors do not feel, but I say that a doctor's heart is rent with anguish when he enters a chamber wherein the air is pestilential, where the sores of wounded men are maggot-infested and the men themselves are eaten up with vermin. All this cooler air would prevent or tend to prevent. The festive hall, the school-room, the living-room, the barrack, the church, would all experience, the occupants regarded, commensurate relief. It would be just as available in ships as on shore. The Red Sea transit and the blazing oceans of the tropics need no longer be things of terror. In steamships a small percentage of steam power would suffice for driving the cool air current. Wind, water, hand, and steam power could also be rendered available. The vans employed to supply blast-furnaces should suffice for anything, but there is the winnowing van which horse or mule, indeed any animal, could work. Even the simple circular bellows would keep an apartment cool. In towns or in a contonment, a stationary engine with air-ducts leading to the different dwellings would satisfactorily replace apparatus adjusted to each separate house. HENRY MACCORMAC

Belfast, July 21

Disease of Potatoes

THE paragraph in NATURE, vol. xxviii. p. 281, regarding a "hitherto unknown" disease of potatoes near Stavanger, appears to be identical in every way with the disease which destroyed the "champion" potatoes in the West of Ireland in August, 1880, described and illustrated by me in the *Gardener's Chronicle* for August 28, 1880. The bodies described by Herr Anda, as about the size of a small black bean, are *Sclerotia*, or masses of highly condensed mycelium, and they have nothing to do with the potato fungus proper, *Peronospora infestans*.

It is a remarkable fact that neither horticulturists or botanists had ever noticed these large black *Sclerotia* in potatoes in Britain before 1880, and as far as I know no one has ever seen them since. There was a prodigious and destructive growth in 1880, and several botanists as well as myself tried to make the *Sclerotia* germinate, but a failure resulted in every instance. It appears that Herr Anda has seen the *Sclerotia* germinating; it is therefore to be regretted that he has not identified, or got some one else to identify, the perfect fungus.

WORTHINGTON G SMITH

"Waking Impressions"

I HAVE before me now a record, written the following morning, of a waking impression of the same order as that told by Mrs. Maclear in NATURE, vol. xxviii. p. 270, but which I think shows more clearly the sort of duplexity of brain action that one sometimes detects in dreams.

I awoke with a clear vision of a pamphlet I was holding. The subject was cookery, and about four-fifths of the cover was occupied by an engraving of pots and pans, trussed chickens, and other culinary matters. Below this, in one line, printed in capitals all of the same size, was the title which I was reading at the moment of awaking, "FOOD, OR THE ASTROLOGY OF EVERY DAY."

My first waking impression was of the utter irrelevance of the alternative title; but on looking at it with closed eyes more carefully I saw that the paper in one place had been rubbed, and that a little bit was curled up, leaving a wider space between "the" and "astrology" than between the other words. The conviction then came to me that a letter was missing, and that the word in full must have been "Gastrology." This of course made sense of the title; but it is curious that one's waking intelligence should be needed to interpret the inventions of one's dreams. E. HUBBARD

1, Ladbroke Terrace, July 21

A Remarkable Form of Cloud

WHILE preparing to observe the moon on Sunday, the 22nd inst., at 10h. 20m. p.m., my attention was attracted to a peculiar patch of grayish white light a few degrees from the moon, which upon closer examination I found extended right across the heavens, from the north-north-west to the south-south-east point of the horizon, passing through the zenith. It had a breadth of about 2°, and was sharply defined on both sides, more especially the northern, excepting near the zenith, where it was broken up into three or four detached cloudlike masses. All other parts of the sky were perfectly free from clouds, so that this one appeared like a gigantic arch spanning the heavens; so much so that a person to whom I pointed it out compared it to a rainbow, which it very much resembled in form. At 10h. 45m. it was reduced about one-half in width and had shifted 20° from the zenith

towards the north-east, though it still extended from the south-south-east to north-north-west. By 10h. 55m. it had broken up into four irregular streaks of clouds of various breadths and parallel to each other, the only portion of the original arch being a narrow streak extending from the south-east to the meridian, where it faded away. This was the "beginning of the end," for the remnant of the original arch and the other clouds in a short time disappeared below the eastern horizon, leaving the sky beautifully clear.

I should much like to know whether any other observer was fortunate enough to observe this remarkable cloud; I say remarkable because, though I have been a pretty constant observer of the heavens for the last eight years, I have never noticed anything of the kind before. B. J. HOPKINS

10, Malvern Road, Dalston, E., July 24

Triple Rainbow

IN the afternoon about 5.30 a week or ten days ago, I noticed a rainbow of the ordinary type, and quite complete, which lasted about five minutes; the portion to the right hand then faded away, as well as the upper and lower portions apparently of that part of the bow visible to the left hand; but the middle portion of the remainder of the bow divided apparently into three parts, each one complete in their prismatic colouring, and yet none of them parallel to each other.

There was a slight difference in size, possibly in favour of that portion belonging to the original bow, and which constituted the outermost of the three arcs.

This portion of the phenomenon lasted for about five minutes, and was also similarly observed by a gentleman walking with me at the time.

Unfortunately some large trees prevented us from seeing the lower portion of the three arcs, where presumably they should have been united into one. R. P. GREG

Coles, Buntingford, Herts, July 23

A Remarkable Meteor

IN regard to the meteor seen by your correspondent P. F. D. at Hendon on the 6th inst., at 8.53 p.m., in a clear sky and broad daylight, I have the following entry in my diary under the same date: "Meteor going south-east through Cassiopeia at seven minutes to nine; daylight." It was indeed a remarkable meteor. The sun had set about half an hour. I happened at the time to be looking intently at that part of the north-east sky in which it appeared. What struck me most was the brilliant sparkling silvery light given off by the fragments into which it divided just before disappearing. I estimated that it would strike the horizon about the south-east point. B. G. JENKINS

Dulwich, July 21

The Function of the Sound-Post in the Violin

MAY I be permitted to correct a careless expression in my letter appearing in your last issue on this subject? The passage I refer to is this: "If the bridge [of the violin] were placed near one end of the instrument, the case would be different," *i.e.* the tone would be louder. I ought rather to have said: "If the bridge were placed nearer to a firm support, the case would be different." The statement is perfectly true as it stands with a sound-board which is equally thin all over, or where the edges are thicker than the middle. It is not true with a construction like that of the violin, where the edges are extremely thin and flexible. A sonorous wave always transmits itself best from the stronger part of the surface to the weaker. R. HOWSON

Sand

MR. MELVIN is at fault in assuming that my paper on sand was "an attempt to distinguish by the aid of the microscope whether sand had been formed by the action of wind or of surf." Its primary object was to show that chalk-flint had scarcely any place in its formation; but few particles of it appearing even from the midst of rolled shingle whether that be ancient or modern. Other problems of course may be determined or solutions suggested by an extensive examination of ancient deposits, compared with those now forming. I have shown that quartz is he great staple of "sand." The size of its particles, whether rounded by attrition or flat, rough, and angular, must be accounted for by observing the conditions under which it exists in modern formations. A large series is being examined by me, and a record will be made of the result. As yet I have no theory whatever. I simply record *facts*. J. G. WALLER

68, Bolsover Street, W., July 18

ON MOUNTING AND PHOTOGRAPHING MICROSCOPIC OBJECTS

WE have received from Mr. E. Wheeler of Tollington Road, Holloway, a collection of mounted microscopic objects, comprising anatomical, botanical, entomological, and other preparations, and we have much pleasure in testifying to the general excellence of the work. One of the objects—a vertical section of the human small intestine—deserves special mention. It shows the glandular cells especially well. The nerves and ganglia of Auerbach's plexus can be seen, and interspersed among the epithelial cells of the villi and Lieberkuhnian follicles are numerous goblet cells.

Space will not allow more than a bare mention of the other objects, including a large transverse section of the stem of Lepidodendron from coal, transverse sections of the stems of spruce fir (*Abies excelsa*) and mare's tail (*Hippuris vulgaris*), the former showing resin canals and sections of bordered pits in the wood cells; Spirogyra in various stages of conjugation, from the first modification of the conjugating cells to the maturation of the zygospores; various Diatomaceæ, including the rare *Coscinodiscus excavata*; injected preparations of intestine of cat and toe of white mouse, and various entomological objects. They are all well prepared, and represent a stock which Mr. Wheeler informs us amounts to fifty thousand objects.

Although the legitimate use of professionally-mounted objects such as these may tend in no small degree to the diffusion of scientific knowledge, the microscopist who employs his instrument for no better purpose than the examination of bought slides will derive little benefit from the pursuit. He should be able to prepare objects for himself, and although there is abundance of accessible information on every detail of the art, it is believed that there is yet a useful work to be accomplished. By showing the facility with which this can be done without resort to the multiplicity of processes usually considered necessary, we shall endeavour in this article to show how any possessor of a microscope may make for himself preparations which, though they may not equal by many degrees the productions of the best professional mounters, yet have a far higher educational value, as their preparation will afford information which could not be otherwise acquired.

The necessary materials and instruments are few and inexpensive. For the support of the objects a supply of the usual 3″ × 1″ glass slides with ground edges, and of thin cover glasses (preferably circular) of various sizes should always be at hand. These when bought will be dirty, and it saves time to clean them all at one operation.

For securing the cover to the slide various cements are used, but of these two only need be mentioned, as they will be sufficient for all ordinary purposes. Gold size is undoubtedly the most reliable cement, but it takes days or sometimes even weeks to harden. It is, however, exceedingly tenacious and tough, and does not become brittle with age. It should always be used in cases where objects are mounted dry or in liquid, but when viscid media are employed, the medium helps to secure the cover, and there is no danger of leakage. Under these circumstances the use of asphalte varnish is recommended. The Brunswick black of the oilshops is a common form of this varnish, but is not so good as the preparation supplied by the opticians. When the varnish is to be used, it must be warmed by standing it in a cup

of hot water, and the slide should be warmed also if this can be done without injury to the object. The varnish should then be applied with a camel-hair brush. It dries in a few hours at the ordinary temperature, or in a few minutes at the temperature of a cool oven, but it has not the tenacity of gold size, and is liable to become brittle with age. To keep the cover in place during the hardening of the cement, spring clips will be required. One very useful form can be made by bending a piece of brass wire into the shape shown in Fig. 1, and fixing it by means of glue into the end of a piece of cedar (end of cigar box) a little larger than the slide.

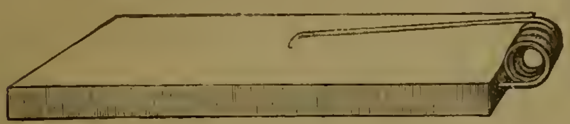


FIG. 1.

When the object is of considerable thickness or when it would be injured by the pressure of the cover glass, a wall or cell of some kind must be raised round it. In general a very shallow cell made by drawing a ring of gold size or asphalte on the slide is sufficient, and a stock of these cement cells of various sizes should be always ready for use. For their manufacture and for finishing the slides a turntable should be provided. This in its simplest (and in the writer's opinion its best) form consists of a heavy brass disk $3\frac{1}{2}$ inches in diameter, capable of rotation in a horizontal plane on a central steel pin. The slide is held in a central position on this table by two spring clips. Then on whirling the table round and applying to the slide a brush charged with varnish, a neat circle will be struck out.

When cells of greater depth are required, solid rings must be cemented to the slide.

For the performance of such dissections as are necessary, the mounter will require two or three small scalpels, one or two razors, a pair of small scissors with sharp points, and two pairs of forceps, one large, with its points roughened where they meet, and one small and slender, with smooth points. Small camel-hair brushes and common sewing-needles fixed in cedar handles like those used for the brushes are indispensable.

Pipettes of various sizes are useful for transferring small quantities of liquids or catching small aquatic animals. They are easily made from pieces of glass tube of various sizes, some being left widely open and others drawn off to a point at one end, which may be left straight or bent at a small angle. The most useful form of pipette is made by tying a piece of sheet indiarubber across the bell of a very small thistle funnel, the stem of which may be either left widely open or drawn to a point as with other pipettes. Pressure with a finger on the indiarubber will displace a quantity of air, and when the open end is placed under water and the pressure removed a quantity of the liquid will be drawn up and can be removed and delivered drop by drop or in a rapid stream. If (the indiarubber being pressed down) the open end of the tube be brought near any small animal in the water and the pressure suddenly relieved, there will be such a rush of water into the tube that the strongest swimmer can be easily captured.

Two or three section-lifters of various sizes and a dozen watch-glasses for holding staining solutions will complete the list.

The objects of mounting are twofold: (1) to render visible structures that could not be seen without such preparation, and (2) to preserve the bodies so prepared as permanent objects for future study.

Various fluid media are employed for the preservation of objects, and much of the mounter's success in his art depends upon a knowledge of the medium most suitable for each particular object. In Figs. 2 and 3 an attempt has been made to show how largely the visibility or invisibility of particular structures is determined by the nature of the medium in which they are mounted.

Both of these figures represent longitudinal sections of the stem of the spruce fir cut from the same shaving of a deal plank, the only difference being that the former (Fig. 2) was mounted in air, and the latter, after staining, was mounted in balsam. In the former case the bordered pits in the wood cells are perfectly shown, but the boundaries of the cells themselves and the medullary rays are indistinct and confused, while in the latter case the wood cells and medullary rays are clearly defined, but the penetration of the highly refractive balsam which has

FIG. 2.—Longitudinal section of stem of spruce pine mounted dry, $\frac{1}{4}''$ objective.

affected this change has reduced internal reflection so far, and rendered the whole section so transparent, that the pits have become almost invisible.[1]

The same truth was forcibly brought home to the writer a few years ago in cutting some sections of fossil coniferous wood (siliceous), which during the latter stages of grinding down displayed the characteristic glandular cells, &c., admirably, but when mounted in balsam became almost perfectly invisible. They were too opaque to be mounted dry, and the only liquid in which they were well displayed was distilled water. The sections mounted in balsam were by no means spoiled though, for the transparency which obliterated all structure when viewed by ordinary light rendered them peculiarly suitable for examination

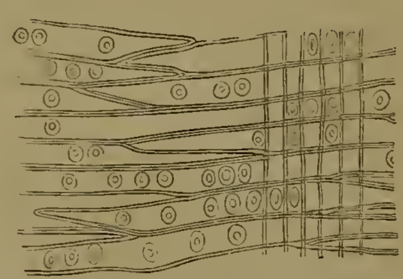


FIG. 3.—Longitudinal section of stem of spruce pine mounted in balsam, $\frac{1}{4}''$ objective.

by polarised light, and when so viewed all their structure returned and they became most beautiful objects.

It would be impossible in these articles to describe all the media employed in mounting microscopic objects, and all that will be attempted is to give instructions for mounting objects dry (that is, in air), in balsam, and in glycerine jelly.

The dry method is employed for such objects as are unaffected by air, and are either intended to be viewed as opaque objects by reflected light, or are sufficiently transparent without previous preparation to be examined by transmitted light. The object of this method is, in fact, simply to afford mechanical support to the object, and to protect it from dust and moisture.

It is necessary that the objects should be perfectly dry

[1] In Fig. 3 the pits are shown much too plainly.

before they are sealed down, or moisture will rise and dim the cover glass, and fungoid growths may make their appearance to the entire ruin of the specimen. A simple and efficacious mode of desiccation is to place the objects on a piece of blotting paper, cover them with an inverted tumbler or bell glass, and place the whole on the top shelf of a kitchen dresser or other warm place for a few days, or in extreme cases weeks. When an object has to be kept perfectly flat during drying, it may be placed between two ordinary slides held together by a letter-clip or American clothes peg.

To illustrate the general method of procedure, we will suppose that the first "mount" is to be a section of deal. Such sections can often be obtained in the ordinary operation of smoothing a plank with a very sharp plane. A piece about half an inch square is to be cut from the thinnest shaving, and dried by two or three days' exposure to warm air, as previously described. Next place it in the centre of a shallow cement cell, take a clean cover a little smaller than the outside diameter of the cell, apply a little gold size round its edge, and place it on the cell. Keep the cover pressed down by a clip and set it aside for a few days in a warm place for the size to dry. The only object of using the cell *in this case* is to prevent the liquid gold size running in between the glasses by capillarity. When the size is dry, fix the slide on the turntable and apply a ring of gold size extending a little way on to the surface of the cover and beyond the cell on to the slide. When this has dried a second coat should be given, and a final ring of asphalte will complete the sealing. It only then remains to label the slide.

We will consider in detail one more case—a preparation of sole's skin to show the overlapping ctenoid scales. As this object is of considerahle thickness, it must be mounted in a cell cut or punched out of a piece of thin cardboard and stuck to the slide with gold size or marine glue, and being opaque and intended for examination by reflected light, a black background should be provided for it by gumming a piece of black paper to the bottom of the cell or varnishing it with asphalte. A large piece of the colourless skin from the under side of the sole must be carefully washed with a camel-hair brush in several changes of warm water to remove the mucus, and then placed between two pieces of glass held together by a strong clip and laid aside for a fortnight to dry. A carefully selected portion is then to be cut out and cemented to the bottom of the cell by a very small quantity of marine glue. The cover may then be applied and the slide finished as before

Having mounted these objects, no difficulty will be experienced in treating in a similar manner wings of insects, entire lichens, and small fungi, fructification of ferns, equisetums, &c., and vegetable hairs, scales, pollen, and seeds. The objects may be dried in their natural condition or under pressure, according to circumstances.

The calcareous and siliceous skeletons of Foraminifera and Radiolaria are usually mounted dry, but space will not allow a description of the processes adapted for freeing them from the dirt and debris with which they are usually associated.

Wood, bone, and hard vegetable tissues are sometimes mounted dry, but as they require to be cut into very thin sections, their preparation will be described in another place.

Heads of insects mounted dry to show the eyes, antennæ, mouth-organs, *i.e. in situ*, require very careful drying, and some support, such as wax, to secure them in the cell in the most favourable position for observation.

Objects of too perishable a nature to be mounted dry, or too opaque to reveal their structure when so mounted and viewed by transmitted light, are most commonly preserved in a thick liquid resin known as Canada balsam. This substance owes its value chiefly to its great penetrating power and high refractive index, by which internal reflection and scattering of light are greatly reduced, and bodies immersed in it are made remarkably transparent. These properties, however, render it entirely unsuitable for mounting objects intended to be viewed by reflected light.

Pure Canada balsam is now seldom used, it being much more convenient for most purposes to replace its natural solvent, turpentine, by a more volatile substance, such as benzole. To prepare the solution the balsam should be exposed to the heat of a slow oven for about two days, until on cooling it becomes hard. Its colour will darken during this process, but the temperature must never be allowed to rise sufficiently to darken it beyond a deep amber colour, and must not be continued long enough to render it brittle. The hardened balsam is then to be mixed with about an equal volume of benzole and allowed to stand, with occasional stirring, until all dissolved. This yields a pale, amber-coloured liquid which flows readily at ordinary temperatures and may be used cold. It should be kept in a wide-mouthed bottle with a large stopper ground accurately to the *outside* of the neck, and a glass rod should be left standing in it.

Before an object can be put up in balsam several preliminary processes are necessary to free it from air and water, and these will be best considered by describing in detail the preparation of some one object—say, a small insect—the common flea.

The creature must be killed without destroying any of its parts, either by immersion in boiling water or by covering it with a watch-glass, under which is then inserted a small piece of blotting paper soaked in chloroform. In a few moments it will be dead, and may then be placed in a 5 per cent. solution of caustic potash for ten or twelve days.[1] This will thoroughly soften and partly dissolve the viscera, the remains of which may be removed by placing the insect between two glass slides and squeezing it flat under water. The effect of this pressure is to squeeze the softened viscera out of the thorax and abdomen through the anus, and the spiracles on each side, or, if the pressure be violent, through an opening which is forced at the extremity of the abdomen, or between the thorax and first abdominal somite. The flattened flea should then be very carefully washed with soft camel-hair brushes, and soaked for two days in two or three changes of water to remove every trace of potash. It is then to be placed between two slides held together by a clip, and put aside in a warm place for a week to dry.

FIG. 4.

The water has now been eliminated, and the next process is to soak the flea for a day or two in spirit of turpentine, which will penetrate all its interstices and displace the air, thereby rendering it beautifully transparent, and preparing the way for the penetration of the balsam.

It only now remains only to mount it in the balsam. A small table, with a brass top $3\frac{1}{2}$ inches long, 2 inches wide, and 3-16ths of an inch thick, is very useful for supporting the slide. On its centre should be engraved or scratched an oblong space 3 inches long by 1 inch wide with a central point and two or three concentric circles to serve as guides for centring the slide and cover respectively. A cleaned slide should be held in the centre of the table by a spring clip of the shape shown in Fig. 4, so placed in this case that its edge is a quarter of an inch to the left of the centre of the slide. The flea is then to be taken out of the turpentine by means of a section

[1] Common shallow earthenware ointment pots with lids are very convenient for holding solutions in which objects have to be soaked for any length of time.

lifter, and properly arranged on the centre of the slide. A drop of balsam is next taken up on the glass rod and allowed to fall upon the object and spread a little way beyond it. A half-inch circular cover glass, previously cleaned, is taken up with a pair of smooth-pointed forceps, and its lower edge allowed to rest against the spring. It is then slowly and very steadily lowered, guided by a mounted needle held in the left hand. In this way a wave of balsam will be driven before it, and will reach the edges of the cover without including any air. Very often the object is displaced by this wave, but this can generally be remedied by a slight pressure with a needle on the side of the cover to which the object has moved. When it is again by this means worked to the centre of the slide, a little firmer pressure should be applied to the centre, so as to press it down and squeeze out all excess of balsam.

(*To be continued.*)

ON THE OLD CALENDARS OF THE ICELANDERS[1]

THE old Icelandic system of measuring time, which to some extent still holds its ground in the island, has the peculiarity of being based on the week as its fundamental unit of measurement, although it recognises a year consisting of fifty-two weeks, the 364 days of which were included in twelve months of thirty days each. To the last of these months, which belonged to the summer, four days were added under the name of *Sumar-auke* or "summer addition." In accordance with this arrangement every given day of a month always fell on one and the same day of the week, as in the lunar year's calendar the first day of each month coincides with the period of new moon.

The Icelandic year was further divided into two half years, viz. summer and winter, known as "*misseri*," the former of which began on a Thursday in April, thence called "summer day," and the latter on a Saturday in October, the "winter day." These "*misseri*" were more used than the year itself to measure time, and Icelanders gave the name "*Misseristal*," or half-year's reckoning, to their calendar, while they habitually counted by the weeks of these winter or summer measures in referring to the everyday occurrences of the passing year, just as they spoke of *winters* and not years, the former being assumed to include the summers which directly followed them in the ordinary course of nature. By an analogous mode of reasoning they spoke of "nights" instead of days in referring to the twenty-four hours of night and day. This custom no longer exists among the modern Scandinavian nations, but traces of it still survive among ourselves in the expressions "fortnight" and "se'nnight," which are undoubted survivals of an ancient northern mode of reckoning time, unknown to southern peoples. This proof of the prevalence of a system of counting by nights among the common ancestors of the Icelanders and Anglo-Saxons makes it the more remarkable that the modern Scandinavians alone among European races should have a separate word to express the twenty-four hours of a day and night, as *dygn* in Swedish, and *dögn* in Dano-Norwegian, which have been derived from the O.N. *dagr*, day.

Each of the Icelandic "*misseri*" was divided into two parts, known as "*mál*," measures. Of these the second half of winter began on a Friday in January, distinguished as "midwinter day," while "midsummer day" fell on a Sunday in July, which was the first day of the second half of the *Sumar-mál*. This last of the four quarters contained ninety-four days, owing to the addition of the four nights of the "Sumar-auke," while the other three contained only ninety days each.

[1] "Om Islændernes gamle Kalendere." By Herr Geelmuyden. *Naturen*, No 4, 1883.

The errors of this method of computation, which gave only 364 days to the year, were early detected, for, as we learn from an interesting manuscript of the twelfth century, known as the "Rimbegla," which is preserved in the Royal Library of Copenhagen, the first reform of the Icelandic calendar was effected by the learned Thorstein Surtr, who, as the grandson of Thorolf Mostrarskegg, one of the original colonists, could scarcely have belonged to a later period than the middle of the tenth century. In accordance with the naïve mode of narration common to the chroniclers of the time, the "Rimbegla" calls in dreams and visions to explain the introduction of a more correct method of counting time among the Icelanders. Thus we are told that when, after long pondering on the reason why the summer was falling back into spring, Thorstein Surtr bethought himself of a way by which the *misseri* might be brought again to their ancient courses, he dreamt that he was standing on the Law-Hill of the Althing, and that while all other men slept, he was awake, but when he seemed to himself to be sleeping, all others were watching. This dream was interpreted by the wise Osyv Helgason to imply that while Thorstein spoke at the Law-Hill, all men must keep silence, and that when he ceased speaking all must proclaim aloud their approval of his words. Accordingly, when he proposed at the Thing that in every seventh summer seven nights should be added to the four nights of the "Sumar-auke," all men agreed to the change without question or hesitation. By the adoption of Thorstein's suggestion, the Icelandic year acquired 365 days, similar to that of the ancient Egyptians, although by retaining the early mode of intercalation in the summer term, the old relations between the days of the month and week remained unchanged. From this time forth the expression "Sumar-auke" was applied equally to the original four annual intercalary days, and to the seventh year's week added by Thorstein, which has retained the term to the present age. In the modern calendar the word "aukanætr," added nights, has, however, replaced the older appellation of "Sumar-auka."

Soon after the introduction of Christianity into Iceland in 1000, the national calendar was brought into closer relations with the Julian system, on which the clergy everywhere based their determinations of the festivals of the Church, and by adding a week to the old "Sumar-auke" five, instead of four, times in twenty-eight years, the average year acquired an addition of one-fourth of a day, and was thus made to approximate more nearly to the Julian year.

In the "Rimbegla" full directions are to be found for comparing the periods of the beginning and ending of the ancient *misseri*, or seasons, with the divisions of the year observed in other Christian lands, while this authority is, moreover, the only source from which we obtain a clear insight into the methods originally adopted for determining for any given year the amount of the irregularities, known as "Rimspiller," which necessarily occurred in a system that took no account of the Julian leap-year. It is curious to observe that while in Iceland, as elsewhere in the middle ages, the fixed and movable festivals of the Church were made to regulate the divisions of time, and to fix the periods of political and social events, the old Icelandic modes of computing time were never eradicated. But although the people continued to count by "*misseri*," winters, weeks, and nights, the beginnings and endings of the "*misseri*" were fixed in Christian times by the dates of the great Church festivals, which similarly controlled all national events, and thus we find that the exact date of the annual "Riding to the Thing," and the duration of the session of the Althing, were regulated by the day of the week on which the Festival of St. Peter and St. Paul (June 29) happened to fall.

The twelve months are spoken of in the older Edda under their respective names, but from the earliest

times the common usage in Iceland, as we have already observed, was to count by the weeks of each of the "*misseri*" instead of referring to months. According to Prof. Munch, the Northmen originally divided the week into five days, the so-called *Fimt* (Fifth), the later hebdominal week having been borrowed, like the names of the days, from the south. The latter, in spite of their apparent northern character, are in point of fact mere adaptations of the names of the Roman deities Mars, Mercury, Jove, and Venus, which reappeared in the old northern calendar as Ty, Odin, Thor, and Freja. Saturn alone failed to find a representative in this system of nomenclature, for to the genuine Northman it would seem that the last day of the week could have no other designation than that of "Laugar-dāg," or "Thvott-dāg," washing or bathing day. And this name has been retained through the intervening ages, being the only one that escaped the ban of the Church, when a century after the establishment of Christianity an episcopal ordinance interdicted the application of the names of heathen gods to the several days of the week, which were thenceforth known in accordance with their order of sequence, although *Sunnudāg* and *Mánadāg* in course of time replaced the older designations of "First Day" and "Second Day."

The new style was introduced into Iceland at the same time as in the foster- and mother-lands of Denmark and Norway, and in accordance with a royal edict, the day after February 18 in the year 1700 was reckoned as March 1. From that period to the present time the Icelandic calendars have given double tables based on the Gregorian, and the locally modified Julian system. A few modifications have, however, been made in modern times in the older national methods of intercalation, "summer day" falling on the Thursday between April 19 and 25, while in strict accordance with the past methods of computation it should fall on the Thursday between April 21 and 27. The intercalated week of the old "Sumar-auke" has also been shifted from midsummer to the close of the summer measure, and thus falls partly in September, "Haustmánadr," and partly in October, "Gormánadr."

THE ORFE, A FISH RECENTLY ACCLIMATISED IN ENGLAND

THE fine specimens of the "Orfe" presented by his Grace the Duke of Bedford to the International Fisheries Exhibition, and exhibited in one of the tanks of the Aquarium, fully deserve the notice of all interested in the culture of our freshwater fishes. They are some of a number which Lord Arthur Russell succeeded in importing from Wiesbaden in March, 1874, and which were placed in a pond at Woburn Abbey in Bedfordshire. Owing to the succession of cold summers these "Orfes" did not breed until last year, and we may hope that this season will also prove favourable. This species may now be considered as acclimatised, and will become a permanent acquisition to our ornamental waters.

The Orfe, whose bright yellow or golden colours resemble those of the Goldfish or Golden Tench, is, like these two latter fish, a permanent variety of a wild and much less brightly coloured race, belonging to the same genus as, but specifically distinct from, the Chub, with which it was confounded by some writers. Its systematic name is *Leuciscus idus*; of vernacular names those of "Aland" and "Nerfling" are those most generally used in Germany, whilst the Swedes know it by the name of "Id." The name "Orfe" refers to the golden-coloured variety only, which has been cultivated for centuries in inclosed waters in Bavaria. Willughby knew it well; he says in his "Historia Piscium" (Oxon, fol. 1686), p. 253:—"At Augsburg we saw a most beautiful fish, which they call the 'Root oerve,' from its vermillion colour, like that of a pippin apple, with which the whole body is covered, except the lower side, which is white." As in the Golden Tench, individuals of pure golden-yellow tints are scarce, the majority retaining marks of their origin from a plain-coloured ancestry in brownish spots or blotches on some part of their body. The ordinary size of this species is ten or twelve inches (and this is about the size of those at the Exhibition); but it is known to have attained to double that size and to a weight of six pounds.

The Orfe will thrive in all inclosed waters suitable to Roach and Goldfish; as an ornamental fish it is preferable to the latter on account of its larger size, livelier habits, and rapid reproduction; it takes the bait, and is eaten in Bavaria. As an ornamental domestic fish the Goldfish will always hold its own, but for waters of any extent and free from Pike and Perch we know of no more ornamental fish than the Orfe, a worthy rival of the Golden Tench, which has been so successfully acclimatised by Lord Walsingham; and we trust that his Grace will soon rear a sufficient number to secure to the Orfe a home in many different parts of the country. A. G.

SNOW AND ICE FLORA[1]

THIS work, which is included in Baron Nordenskjöld's studies and investigations arising out of his travels in the extreme north, is quite as interesting and important as regards the snow and ice flora of the Alps and Arctic regions, as the great traveller had led us to expect (see NATURE, vol xxviii. p. 39). It is, as far as the materials on hand permit, an exhaustive account of the subject of which it treats.

As might be expected, the first pages of the work are devoted to "red snow," than which there are few subjects that have more engaged the attention of scientific travellers in the Arctic districts. This little plant has been found in the Arctic regions of Europe and America, thereby suggesting, as Prof. Wittrock observes, the former union of the two continents. It also appears in the north of Scandinavia, on the high Alps, the Pyrenees, and the Carpathians. Various were the opinions as to whether it belonged to the animal or vegetable world, and many the names by which it was designated. The prettiest of these names is certainly that given to it by C. Agardh—"the snow-flower." While, however, "red snow" will probably continue to be its trivial name, Prof. Wittrock has restored to it the scientific name of *Sphærella nivalis*, bestowed on it by Sommerfelt in 1882.

Until Nordenskjöld's expedition to Greenland in 1870, this alga was thought to be the only living plant on the ice and snow; but during their wandering on the inland ice, Nordenskjöld and Berggren discovered several algæ, among which was one new to science, namely, *Ancylonema Nordenskjöldii*, which was seen in such abundance, that it gave to the adjacent ground a peculiar purple-brown colour. Other algæ seemed to be mixed up with the fine sand (ice-dust, *kryokonit*), which here and there spreads a thin covering on the ice, or lies in a thick layer at the bottom of the funnel-shaped holes which are formed in it. Baron Nordenskjöld lays great stress on the important part which these algæ, and especially Ancylonema, play in the melting of ice. "The dark mass (algæ)," he says, "absorbs a larger portion of the sun's rays than the white ice, and therefore produces deep holes in the ice, which in a great degree conduce to its melting." He even thinks that this Ancylonema once performed the same office in Scandinavia, adding, "We have, perhaps, to thank this plant that the ice deserts which formerly covered Europe and America with a coating of ice, now give place to shady woods and undulating fields of corn."

[1] "Om Snöns och Isens Flora. Särskildt i de Arktiska Trakterna." Af Veit Brecher Wittrock. Ur "A. E. Nordenskjöld, Studier och forskningar föranledda af mina resor i höga Norden." (Stockholm, 1883.)

Subsequent investigation proved that the ice and snow flora was richer than had been anticipated. Dr. Kjellman found at Spitzbergen not only "red snow," but "green snow." Some of the "material" was brought home in a dry state; on being afterwards examined, it was found to contain above a dozen other plants, some of which were of a class even lower than "red snow"; others belonged to plants of higher organisation. Mosses also in the protonemata state were met with, but of very diminutive size. The ice and snow vegetation of this and other localities is described in detail. Special interest invests the kryokonit[1] with which all the specimens from South Greenland were mixed, because it was found to contain a number of germinating spores of *Sphærella nivalis.* During the winter of 1880-81 Prof. Wittrock was fortunate enough to enable some of these spores to develop themselves, hence it was considered that they were resting spores. They endure, without taking any harm, to be during the greater part of the year, frozen up in the ice and snow of the Arctic regions, and also to be dried up for some months by the heat of the sun. The author's observations on the conditions of plant life in the Arctic regions and on the glaciers of high northern tracts are particularly interesting. He observes that these tracts are certainly not entirely deprived of the powerful and life-giving influence of the sun's rays. They are, it is true, during a great part of the year (in winter) enveloped in continual darkness and gloom; but at another period (in summer) they are in the enjoyment of perpetual light. During this period the sun's rays, although oblique, may exercise a powerful influence. At midday the heat may be surprisingly strong. Nordenskjöld found that the warmth of the air a short distance above the surface of the ice at midday in July rose to 25°–30° of Celsius. It is evident that a great melting would take place on the surface of the glaciers and snow-fields. There is then formed a layer of snowy and icy water, which, though not much above the zero of Celsius, is enough to satisfy the demands for warmth of this portion of the simplest organisations of the vegetable kingdom. That they thrive under these hard conditions of life is evident from the immense multitudes in which they occur. "Probably," adds the author, "there is no other species on earth which is richer in individuals than red snow."

Prof. Wittrock gives a full description of the structure and fructification of these minute plants; then follows a summary of their characteristics. The latter may be thus briefly stated:—The flora of the ice- and snow-fields consists almost entirely of algæ of microscopical size and of extremely low organisation; the greater part of the plants are unicellular; they are sometimes solitary, sometimes in colonies. The fructification is very simple, asexual, and of one kind only. These algæ are generally of bright and full colours. The "snow-flower" is blood-red, *Ancylonema Nordenskjöldii* purplish-brown; many Conferveæ and Desmidieæ are bright green The land vegetation is represented entirely by mosses, which appear to be nearly in the same low state of development as the algæ.

The orders, families, genera, and species of which the Arctic flora is composed are well arranged in tabular form at pp. 112, 113. In this table the flora of the snow is distinguished from that of the ice. It will be seen that the most common plant is "red snow;" the next in frequency is *Ancylonema Nordenskjöldii.* The snow flora is richer than that of the ice. The former includes thirty-seven species; the latter ten only. The mosses and Conferveæ belong exclusively to the snow flora. Ancylonema is the only plant which is limited entirely to the ice flora. Of Phycochromophyceæ the ice flora has two species only, while that of the snow possesses ten. The snow flora of Spitzbergen is rich in Conferveæ, that of Lapland in Desmidieæ. In the middle north the Phycochroms prevail. It is stated that *Bacteria termo* is occasionally found within the limits of the ice and snow flora. *Chytridium hæmatococci* may also, observes the author, belong to the Arctic flora, as it was found parasitic on *Sph. nivalis* on the Berner glacier in Switzerland.

[1] Analyses of kryokonit will be found at pp. 95, 96.

It appears that the Arctic regions possess a microscopic fauna as well as flora. The limits of this notice will only permit a reference to p. 116, where the small animals of which this fauna consists are described. One fact connected with these little creatures may be mentioned. With the object of a further study of the algæ, Prof. Wittrock put a portion of the dried material brought from Spitzbergen into distilled water. He found that not only the algæ came to life again, a fact which he had before observed with respect to red snow, but what was more astonishing, even the little worms revived, and ate a great deal of food, which could be distinguished under the microscope as the reddish-yellow contents of the intestinal canal of these transparent, colourless creatures.

The work is illustrated by two woodcuts and by five lithographic plates, one of which contains figures from drawings by Prof. Wittrock of some of the plants; the others consist of views from drawings by Dr. Berggren, of the inland ice of Greenland, representing localities from whence portions of the material containing the ice and snow flora were obtained.[1] The view of the intermittent spring which the travellers met with about 45 kilomètres from the coast, and which, bursting from a cleft in the ice, throws up a jet of water to a great height, is of special interest from the indications it gives of the probable existence of warm conditions in the interior of Greenland. It will be observed that the "sky-line" of the distance in some of the views shows an undulating outline, suggesting a hilly country in the interior.

MARY P. MERRIFIELD

NOTES

THE Lords of the Committee of Council on Education have, by a recent minute, decided to withdraw the prizes hitherto given to candidates in the Science Examinations who obtain a first class in the elementary stage of the various subjects of science, substituting certificates of merit, and retaining only the prizes given in the advanced stage. The money hitherto devoted to prizes will be employed in providing thirty-six National Scholarships—twelve each year—which will be offered in competition to students of the industrial classes, and awarded at the annual examinations of the department. The National Scholarship will be tenable, at the option of the holder, either at the Normal School of Science, South Kensington, or at the Royal College of Science, Dublin, during the course for the Associateship—about three years. The scholar will receive 30*s.* a week during the session of about nine months in the year, second-class railway fare to and from London or Dublin, and free admission to the lectures and laboratories. This is a most important step in advance.

WE have already announced that the Thirty-second Annual Meeting of the American Association for the Advancement of Science will be held at Minneapolis, Minnesota, from August 15-21 next. A Local Committee has also been formed to carry out the arrangements at Minneapolis, and members expecting to attend the meeting are requested to send a notification to that effect to its secretary, Prof. H. N. Wurchell, Minneapolis, without delay. Full titles of all the papers to be read at the meetings

[1] Those who are interested in these algæ may like to know that specimens of fourteen of them are included in the Fasciculi of dried freshwater algæ distributed by Prof. Wittrock and Dr. Otto Nordstedt, of which ten parts have already appeared. The 11th fasciculus, containing other portions of the Arctic flora, will shortly be issued at Lund, Upsal, and Stockholm, under the following title:—"Algæ aquæ dulcis exsiccatæ præcipue Scandinavicæ quas adjectis algis marinis chlorophyllaceis et phycochromaceis distribuerunt Veit Wittrock et Otto Nordstedt."

must be forwarded to the permanent secretary as early as possible, accompanied by an abstract of their contents and a statement of the time which they will occupy in delivery. By the kindness of a member, provision will be made for the illustration of papers by means of a lantern if the authors bring their slides to the meeting. Altogether the arrangements are very complete, and a cordial welcome will, we doubt not, be given to any foreign members or visitors who are making arrangements to attend this meeting of the American Association.

THE Council of the Yorkshire College announce that the Cavendish Professorship of Physics has been established as a memorial to the first President of the College, the late Lord Frederick Charles Cavendish, M.P. The fund required to endow this chair was 7500*l.*, and 7560*l.* 13*s.* has been contributed. Prof. Rücker retains the position he has occupied with much distinction from the foundation of the Yorkshire College, as Professor of Physics, but his title will in future be "Cavendish Professor of Physics."

DR. CARGILL G. KNOTT, F.R.S.E., Secretary of the Edinburgh Mathematical Society, has been recently appointed Professor of Physics in the Imperial University of Tokio, Japan.

ENGLISH chemists may be interested to learn that an election to fill the Chair of Chemistry, including General and Industrial Chemistry, in the University of Virginia, vacant by the resignation of the present incumbent (J. W. Mallet, Ph.D., F.R.S.), will be held by the Board of Visitors of the University of Virginia, on September 11, 1883. The salary of the professor is 3000 dollars, with a commodious house, rent free. Applications, with testimonials, must be addressed to "The Rector and Visitors," P.O. University of Virginia, Albemarle County, Va. We understand the Chair is open to English competitors.

WE regret to announce the death, at the early age of thirty-seven years, of Mrs. Chaplin Ayrton, the wife of Prof. W. E. Ayrton. Mrs. Ayrton was in many ways a remarkable woman. As Miss Chaplin she was one of the first to take up the practical question of women's professional education, and it is in part due to her exertions that the medical career is now opened to women. Her long struggle, from 1869 to 1873, to obtain the necessary permission to present herself for examination told seriously on her health. In addition to attending all the medical classes open to women in Edinburgh, and gaining honours at all the examinations held in connection with them, Mrs. Chaplin Ayrton studied at the hospitals and the Medical School of Paris, and there took her degree of M.D. in 1879. Her graduation theses, "Researches on the General Dimensions and on the Development of the Body among the Japanese," is full of valuable scientific experiments.

THE Queen has been pleased to confer Baronetcies upon Dr. Andrew Clark and Mr. Prescott Hewett.

A BLUE-BOOK just issued contains reports on the mineral wealth of Corea. The explorers found numerous veins of iron, copper, lead, and also some gold. These were worked in the rough native fashion, and it is noticeable that no indications of coal were found. In twenty days' journey ten mines were seen, and many of them, especially those of iron and copper, are said to be of great richness.

IN NATURE, vol. xxvi. p. 15, will be found an illustrated description of Negretti and Zambra's patent deep-sea thermometer. This firm have now adapted their inverting thermometer for recording variations of atmospheric temperature at any desired interval of time. Twelve of such thermometers are arranged on a suitable frame in connection with a clock, a galvanic battery, and a series of small electromagnets in such manner that at every hour the galvanic circuit is completed by the clock, this releasing a detent and allowing one of the thermometers to reverse and record the temperature at that moment. In the present form of the apparatus twelve thermometers have been mounted to record hourly temperatures; this period can be easily altered to half-hours or less, or on the other hand to longer intervals, say, of two hours or more. This apparatus differs, it is claimed, from all other registering or recording thermometers in the following important particulars:—1. The thermometers contain only mercury, without any admixture of alcohol or other fluid. 2. They have neither indices or springs, the registrations being by the column of mercury itself. 3. These thermometers may be carried in any position, and cannot be disarranged except by actual breakage. 4. They will record exact temperature at any given hour of the day or night.

THE Berlin Academy of Sciences has granted the following amounts from its Humboldt Fund: 5000 marks (250*l.*) to Dr. Otto Finch, for working at the collection he made during his journey in Polynesia; 6000 marks (300*l.*) to Dr. Ed. Arning (Breslau) for researches on the leprosy epidemic in the Sandwich Islands; the same amount to Dr. Paul Güssfeldt to enable him to continue and extend his exploring tour in the Andes of Chili.

THE Anthropological Museum of Leipzig has been presented with an annual grant of 6000 marks (300*l.*) from the Grasse Fund by the town authorities.

THE sixty-sixth meeting of the Swiss Natural History Society will take place at Zürich on August 6 to 9 next.

THE German Society of Analytical Chemists met at Berlin on June 16-18 last; the most important transaction at the meeting was the adoption of certain uniform methods in the analysis of wines.

THE 27th of June last seems to have been remarkable for earthquakes in various parts of Europe. At Corfu a violent shock was felt at 11.25 a.m. on that day, and at Darmstadt a moderate shock was observed at 11.18 a.m. At the last locality three other shocks occurred in the night following, and a number of oscillations on June 28 at 11.38 p.m. On July 6 at 3.20 a.m. Constantinople and its environs were visited by an earthquake.

IN Sardinia phylloxera is ravaging the vineyards to such an extent that the inhabitants are beginning to despair of being able to overcome the plague.

THE half-yearly general meeting of the Scottish Meteorological Society is to be held in Edinburgh to-day. The business will be—(1) Report from the Council of the Society; (2) The Meteorology of Ben Nevis, by Mr. Alexander Buchan, Secretary.

W. H. EDWARDS announces, according to *Science*, that he will not, at present, complete the synopsis of species commenced in the tenth part of his "Butterflies of North America," but substitute for it a mere list of species, which will be issued with the next (concluding) part of the second series.

THE municipality of Algiers has established a chemical office, on the pattern of the similar Parisian institute, for analysing alimentary substances and discovering adulterations.

AT the last sitting of the French Société d'Hygiène M. Marie Davy, who was in the chair, gave an account of the results of his analysis of the water of the Seine; he found that the impurity is five hundred times greater below Asnieres than at Paris.

THE April number of the *Chrysanthemum* magazine of Yokohama contains a continuation of Capt. Blakiston's notes on Japanese ornithology; also an article by Mr. Eastlake on the ornithology of Hong Kong, and the continuation of a history of Japanese keramics by Capt. Brinkley. The numerous possessors of pieces of "real old Satsuma" in England will hear with annoyance from this skilled authority that large quantities of

Kioto ware have been fraudulently placed on the Western markets as genuine Satsuma. The former is less dense than the latter, and its colour is as a rule darker. In truth, this writer says, not more than a fraction of the ware which has been attributed to the Satsuma workshops was ever manufactured there. The outcome of the factories was always comparatively small. They worked to order, and nine-tenths of their productions were cups, tea-jars, and other small articles. Large vases and portly incense-burners were exceptional; and in the matter of old Satsuma Western collectors have among Japanese *virtuosi* rivals who are at once more competent judges and very much more liberal purchasers than themselves.

THE city of Rouen is to establish at Pont de l'Arche on the Seine large waterworks for the generation of the electric light. A lamp is to be placed on the top of the Cathedral, and directed by a reflector on the surrounding streets.

THE largest display of electric light in Paris is probably at the Hippodrome, where, in the large hall, sixteen regulators and 142 Jablochkoff lights are used, exclusive of those in other parts of the building and outside. The dimensions of the arena are about 45 metres by 120, and the height about 30 metres. The effect is really splendid.

THE past month has brought with it its annual science examinations, and we have been especially struck with the questions of the City and Guilds examination papers in the electrical subjects. One fact that we notice is that there seems very little difference between the pass and honours grades, two or three of the questions being similar, if not identical in each; secondly, in the electric lighting and transmission of power papers there seems to be a great paucity of questions on these topics, and one or two rather prominent questions on theoretical electricity. Are not papers of this kind somewhat misleading to teachers who are preparing classes for these subjects?

A VERY interesting legal case has just been decided by the Solicitor-General in the matter of the Lane-Fox disclaimer. Mr. Lane-Fox sought to be allowed to disclaim from his patent of 1878 for incandescent lamps and storage batteries, all except the use of secondary batteries as a means of storage, and regulation of a supply of electricity. This is a very broad claim, and, to judge by present appearances, of vast importance. Although not the first to use accumulators, yet Mr. Lane-Fox is the first man who worked out a system in which they played the part of regulators for a steady supply. The disclaimer was allowed by the Solicitor-General after a protracted discussion.

ANOTHER so-called "God's waggon" has been discovered in the Deibjerger Moor near Ringkjöbing (Jutland). Our readers will remember our reporting the discovery of the first some two years ago. Dr. Petersen, the keeper of the Copenhagen Museum, has proceeded to the spot to superintend further researches.

TWO further volumes of Hartleben's "Elektrotechnische Bibliothek" have just been published. They are entitled "Die elektrischen Leitungen und ihre Anlage," by J. Zacharias, and "Die elektrischen Uhren und die electrische Feuerwehr Telegraphie," by Dr. A. Tobler.

OF the "Encyclopädie der Naturwissenschaften" (Breslau, Ed. Trewendt) we have received the 14th part of the second division and the 33rd part of the first division. The latter contains the continuation of Wittstein's "Handwörterbuch der Pharmakognosie der Pflanzenreichs," and the former of Dr. Gobel's "Vergeleichender Entwicklungsgeschichte der Pflanzenorgane" in the "Handbuch der Botanik." Both works will, we are informed, be soon brought to a conclusion.

DR. MACGOWAN of Wenchow is endeavouring to procure records of earthquakes in China from the residents in various parts of that country, and with that object has addressed a letter to the local journals. He directs attention especially to Formosa, where earthquakes are most common in November and December, confirming so far Mr. Mallet's observations. A Chinese record thus describes the effects produced on the sea by submarine causes—among them probably earthquakes:—"Peculiar noises of the sea are sometimes heard which are commonly regarded as indicative of change of weather, sounds coming from the foreboding rain, those from the south being followed by wind. Hissing noises are heard; at times they are low, at others loud. When low they resemble the beating of a drum or the dropping of beans on the same instrument. Now the sounds are near, and now distant; stopping suddenly, or continuing for hours. When the noise is loud, it is more noisy than a hundred thousand men, and the sea bubbles up; in very protracted cases the noises continue day and night for half a month, and when of short continuance the sound lasts three or four days. During the sounds the sea is agitated by fearful billows and furious waves."

SEVERE tornadoes are reported as having occurred in Southern Minnesota and Wisconsin (U.S.) on Monday. A railway train was overturned and many of the passengers killed.

DR. H. REUSCH, who last year took part in the geological investigations of the west coast of Norway, made under the direction of Prof. Kjerulf, has given to *Naturen* the results of his examination of the fossils of the fjelds and islands near Bergen. The richest find was discovered on the little island, Stordö, outside the Hardangerfjord, where numerous well preserved remains of crinoids, a great variety of corals, graptolites and shells of mollusks were obtained which belonged to the Silurian period. The rocks of this district were mostly of compressed conglomerates. The small group of islands beyond Espevær, and the neighbouring Siggen Fjeld, exhibit the most strongly marked volcanic character, and owe their origin to the eruption of streams of molten rock and layers of ash and scoriæ, which probably belong to the Silurian age. These products of eruption have, however, not remained *in situ*, for the once horizontal deposits have been so powerfully crushed, twisted, or upheaved at various points, that the masses of rock of which the great Siggen Fjeld is composed have now a vertical inclination trending north. The accidental discovery in 1862 of a small nugget of pure gold embedded in white quartz, in the so-called Storhaugens mine on Bömmel Island, has attracted the attention of prospectors, and a French company has opened extensive works at Viksnes, where copper pyrites are found in considerable quantities. A fine specimen of the auriferous quartz of Bömmel Island may be seen in the museum of the Christiania University.

THE steamer *Germania* is on the point of sailing from Hamburg to Cumberland Sound, in order to bring home the staff of the German Polar station. Dr. F. Boas leaves with the *Germania* for the purpose of making ethnographical researches in Arctic America.

THE additions to the Zoological Society's Gardens during the past week include a Malbrouck Monkey (*Cercopithecus cynosurus* ♂) from West Africa, presented by Miss M. A. Waite; a Black-backed Jackal (*Canis mesomelas* ♀) from South Africa, presented by Mr. E. D. Thomas; a Philantomba Antelope (*Cephalophus maxwelli*), a Duyker-Bok (*Cephalophus mergens*) from South Africa, presented by Mrs. Macfarlane; five Martinican Doves (*Zenaida martinicana*), two Porto Rico Pigeons (*Columba corensis*) from the West Indies, presented by Mr. J. A Ward; a Ringed-necked Parrakeet (*Palæornis torquatus*) from India, presented by Mrs. Humphrey; a Macaque Monkey (*Macacus cynomolgus* ♀) from India, an Ocelot ((*Felis pardalis* ♂) from Demerara, a St. Thomas's Conure (*Conurus xantholæmus*) from St. Thomas, West Indies, deposited; thirteen Common Vipers (*Vipera berus*) from Hampshire, purchased; two Levaillant's Cynictis (*Cynictis penicillata*), two Wonga-Wonga Pigeons (*Leucosarcia picata*), bred in the Gardens.

OUR ASTRONOMICAL COLUMN

The Ellipticity of Uranus.—It may be remembered that Sir William Herschel, who was at first under the impression that the disk of Uranus presented a perfectly circular outline, was afterwards convinced that there was an appreciable elongation in the direction of the major-axis of the orbits of the satellites, though he has not recorded any measures to test this conclusion. On October 13, 1782, about eighteen months after the discovery of the planet, he writes: "I perceived no flattening of the polar regions." On March 5, 1792, he used "a newly polished mirror of an excellent figure: it showed the planet very well defined and without any suspicion of a ring." With powers 240–2400, all which his speculum bore with great distinctness, he formed a different opinion, and remarked, "I am pretty well convinced that the disk is flattened." On February 26, 1794, he has an observation thus recorded, "20-feet reflector, power 480. The planet seems to be a little lengthened out in the direction of the longer axis of the satellites' orbits." Further, in a paper communicated to the Royal Society in December, 1797, wherein he announces his supposed discovery of four additional satellites of Uranus, he says: "The flattening of the poles of the planet seems to be sufficiently ascertained by many observations. The 7-foot, 10-foot, and the 20-foot instruments equally confirm it, and the direction pointed out February 26, 1794, seems to be conformable to the analogies that may be drawn from the situation of the equator of Saturn and of Jupiter." This ellipticity being admitted, he inferred that Uranus had a rapid axial rotation.

In September, 1842, Mädler, remarking that notwithstanding the statement made by Sir W. Herschel no measures of the planet existed which would confirm it or otherwise, instituted a series with the filar-micrometer of the Dorpat refractor. The measures were made on five nights, and the diameter of the planet was determined at every 15° of the circumference, the mean of each set being made to fall nearly at the time of meridian passage. The nights (September 16, 17, 19, 20, and 21) were of exceptional clearness, and permitted of a power of 1000 being used. Mädler found the greater diameter of Uranus $4''\cdot249$ at the planet's mean distance, and the compression $\frac{1}{10\cdot85}$; the angle of the greater axis was 160° 40′ counted from north towards east. At this time Uranus was less than 11° from the descending node of the orbits of the satellites, as determined by Prof. Newcomb.

Between August 24 and October 20, 1843, Mädler repeated his measures on seven nights: his results from this year's series were—

Greater axis of projected ellipse ...	$4''\cdot3274$
Lesser axis ,, ,, ...	$3\cdot8910$
Compression	$\frac{1}{9\cdot92}$
Angle of greater axis with declination circle	$15° 26'\cdot1$

This ellipse is for September 28, 1843, when the distance of Uranus was 19·079. The greater axis for the mean distance of Uranus would be $4''\cdot304$.

An ellipticity comparable with that of the planet Saturn might have been expected to strike the generality of observers provided with the large instruments which have been available since the epoch of Mädler's measures; yet neither with the Pulkowa refractor, with the late Mr. Lassell's 4-foot reflector, employed by him and Mr. Marth in measures of Uranus at Malta in 1864–5, nor with the Washington 26-inch refractor, or many other instruments of adequate power, do we find that there has been any confirmation of the great inequality of diameters found by Mädler, up to 1877.

It now appears from a communication made by Prof. Safarik of Prague to the *Astronomische Nachrichten* in April last, that on March 12, 1877, he found Uranus "certainly elliptical, the greater axis in the parallel," and this impression he received on various occasions up to the date of his letter. On April 2 in the present year he records of the appearance of the planet: "Stets stark länglich; in den besten Momenten schätze ich die Elliptität stärker als jene Saturns"; the greater axis was at 190°. The instruments used were of very moderate capacity, being an achromatic of 11 cm. and a silver-on-glass speculum of 16 cm.

In consequence of a representation from Prof. Safarik, who laid stress upon the actual proximity of the planet to the ascending node of the orbits of the satellites, Prof. Schiaparelli has made, this year, an extensive series of measures of the diameter of Uranus, the results of which have appeared in No. 2526 of the above-named periodical. The measures are discussed on two methods giving for the ellipticity of the planet in the one case $\frac{1}{10\cdot98 \pm 0\cdot93}$, and in the other (perhaps the more preferable value), $\frac{1}{10\cdot94 \pm 0\cdot67}$. In addition to actual measures, Prof. Schiaparelli drew the outline of the planet, as it appeared to the eye, on thirteen nights, the drawings giving by measurement an ellipticity of $\frac{1}{11\cdot07}$. An assistant in the same way found $\frac{1}{10\cdot9}$. The Milan measures with the filar-micrometer were made between April 12 and June 7. For the equatorial diameter at the mean distance Prof. Schiaparelli found $3''\cdot911$.

PHYSICAL NOTES

In the current number of *Wiedemann's Annalen*, Prof. C. Christiansen of Copenhagen resumes his researches on the indices of refraction of coloured liquids. The methods adopted consisted in the examination of the liquid in hollow prisms of very small refracting angle; a few drops of the liquid being placed between two small pieces of glass touching each other at one side, but separated about half a degree. Another method consisted in inclosing the liquid between a piece of very thin glass and a biprism made of a glass the index of refraction of which was known, the index of the liquid being calculated by taking the refraction as the difference of the two separate refractions of the glass and the liquid. Prof. Christiansen gives tables of results for water, alcohol, turpentine, and nitrobenzol, and also for solutions of permanganate of potash of various degrees of concentration. For the latter substance the results agree with the determinations of Kundt, but are probably more exact.

Prof. G. M. Minchin has greatly improved the form of the absolute sine electrometer invented by him some months ago. The first of the new instruments constructed by Mr. Groves of Bolsover Street is now complete, and is to be sent out to Prof. Anthony of the enterprising and wealthy Cornell University. We hope shortly to illustrate and describe this beautiful instrument.

Prof. Ewing of Tokio prints in the *Proceedings of the Seismological Society of Japan* three valuable seismological notes. The first of these describes a duplex pendulum seismometer the principle of which is the following:—A common pendulum having its centre of gravity below the centre of suspension is stable; an inverted pendulum with pivoted supporting rod is unstable. By placing an inverted pendulum below a common one, and connecting the bobs so that any horizontal displacement must be common to both, the equilibrium of the jointed system may be made neutral or as nearly stable as is desired. A very sensitive seismograph is thus obtained. The instrument has not yet been put to the test of an actual earthquake.

Prof. Quincke has contributed to the *Proceedings of the Royal Prussian Academy of Sciences* an important memoir on the changes produced by hydrostatic pressure in the volume and refractive index of transparent liquids. The ratio of these changes exhibits, it appears, a definite relation. The compressibility in volume was measured by subjecting the liquids to pressure in glass vessels furnished with capillary tubes. The indices of refraction were measured by observing the number of interference bands in homogeneous light in an interferential refractometer. One of the most important results of this research is the light it throws on the disputed formula called the *constant of refraction*. According to Dale and Gladstone the name of *constant of refraction*, or *specific refractive power*, should be assigned to the quantity $\frac{\mu - 1}{s}$, where μ is the index of refraction and s the specific gravity of the substance. According, however, to Laplace the quantity $\frac{\mu^2 - 1}{s}$ is the true constant of refraction; whilst, according to Professors H. A. and L. Lorenz, that name should be given to the more complicated function $\frac{\mu^2 - 1}{(\mu^2 + 2)s}$. Now since with liquids that are subjected to pres-

sure the density varies proportionally with the pressure within certain limits, the true constant of refraction should be that function of the index of refraction and of the density which is independent of pressure. In point of fact Prof. Quincke's experiments confirm the formula of Dale and Gladstone, since $\frac{\mu - 1}{s} = \frac{\mu_1 - 1}{s_1}$, where s_1 is the density under any given pressure, and μ_1 the observed refractive index under the same pressure. To put the matter in simple phrase, *the decimals of the refractive index increase proportionately with the density*.

IN a further paper in *Wiedemann's Annalen*, Prof. Quincke has given some details concerning the experimental methods pursued in his investigations, together with figures of the apparatus and tables of results for a large number of liquids under different conditions.

M. BLEEKRODE has lately described in the *Journal de Physique* a very convenient form of apparatus for projecting galvanic experiments on a screen. It consists of a glass bath (6 cm. long, 5 cm. high, 1 cm. broad), at either end of which is a metallic support which not only makes contact with the two plates that are immersed in the bath, but also are attached to a flat galvanometer which is placed on the top of the bath. The galvanometer consists of a light ebonite framework the same size as the top of the bath and 1 cm. thick, upon which is wound two or three layers of insulated copper wire ·3 mm. thick. A single needle is used, supported on a pivot in the centre of the coil. The whole apparatus is of such a size as to be easily used in any lantern.

IN a recent number of *Carl's Repertorium*, Th. Edelmann describes a very simple means of determining the specific weight of a gas. His method consists in taking a column of gas which presses on a membrane, then observing the displacement of the membrane. This is a somewhat analogous action to the aneroid barometer. The absolute arrangement being to have the membrane strained on a metallic box about 30 cm. diameter, this box is in direct communication with a tube 2 m. long filled with gas. Upon the membrane rests a light lever which carries a mirror at its point of suspension; thus by raising a scale at a considerable distance the slightest movements can be observed and therefore the density taken with the greatest accuracy.

M. MORIN has lately brought out a new electric candle, one great advantage in it being that the light may be extinguished or relighted at any time. This is obtained by the attraction of a piece of soft iron by a flattened solenoid; fixed on the same axis as the soft iron is a cam, upon whose position the proximity of the carbon depends. This motion is easier and not so noisy as the electromagnet as used by Wilde and others.

M. TOMMASI has brought out a new regulator in which he uses selenium, whose resistance varies considerably with variations in the intensity of light. At present it has only been adapted to regulating the position of the light of a Jablochkoff candle.

THE latest idea brought out for making incandescent lamps is by Messrs. Boulton, Soward, and Probert. They electrolyse a carbonaceous gas between platinum electrodes, in a globe; as soon as an arch of carbon is formed the globe is exhausted and the lamp ready for use.

MESSRS. J. ELSTER AND H. GEITEL have found that a Zamboni pile can be made to work as an accumulator by charging it from a Holtz machine. After ten minutes they obtained a spark with the poles 1 mm. apart. Peroxide of lead does not work so well when used ready formed.

M. REYNIER has published some figures concerning the work done by a Leclanché battery when used on a telephonic exchange. Two batteries of three cells each were used for thirty days of seven hours' duration. The loss of weight of zinc during that time was 64·5 grms., which represents 63,235 coulombs. This is equal to a current of 0·084 ampere during the month. Taking the E.M.F. of a Leclanché cell at 1 volt, the total work done is 189,705 watts, which is equivalent to 1 h.p. every 52 minutes.

GEOGRAPHICAL NOTES

THE new number (No. 1 of vol. iv.) of the German African Society's *Mittheilungen* gives a table of magnetic observations and temperature made at different points of his route from Kakoma to Karema by Dr. E. Kaiser, who unhappily died last November on the bank of the Rikwa lake. A copious list follows of Dr. Kaiser's altitudes between Zanzibar and Kakoma. On the basis of English maps of the Niger and the Binuë, Dr. Kiepert traces Herr Ed. Robert Flegel's route from Eggan to Bida in September, 1881, and thence by way of Keffi to Loko in November and December of the same year. Summing up Herr Flegel's topographies, Herr Stück determines the latitude of Loko at 7° 58′ 16″ ± 7″ N., and of Keffi at 8° 49′ 22″ ± 3″ N. In an interesting letter from Ngaundere amid the sources of the Logone, dated August 22, 1882, Herr Flegel claims to have discovered the source of the Binuë, or at least an important part of the territory from which this river takes its source. On July 31 last Herr Flegel proceeded from Jola to the watershed between the tributaries of the Faro and the Binuë, and on August 17 reached the first fountain-brook of the Binuë, passing it and two further heads of the river on the 18th. Ascending a steep mountain chain, the watershed between the Binuë, Faro, Logone, and Old Calabar system, he beheld the last stream, by the inhabitants unanimously named the Binuë in contradistinction to the Guzun-Binuë (beginning of the Binuë) he had first passed. From the back of the mountains close by their encampment on the first *rimchi* (farm) of Ngaundere, the source of the Binuë was pointed out by the natives. If not *the* source, it was undoubtedly one of the main sources. After a stay of four months at Ngaundere Herr Flegel returned to Lokoja, whence, in a letter of February 21 last, he projects an early exploration of the lands yet unknown to the south of the Benuë and of the watershed crossed by him the previous year. He also contemplates opening up the territories where the Tsad and the Niger have their sources, and investigating the relations between these two water-systems, examining Barth's hypothesis of a direct water communication between the Tsad and the Niger by means of the Mao Kebbi and the Jubori swamps. He will further make inquiry into the political and ethnographical relations between the Tsad and Niger territories. Astronomical topographies are given of places visited by Lieut. Wissmann between Malange and Kimbundu. There are two interesting and instructive reports by Dr. Pogge and Lieut. Wissmann on their expedition through the south-east of the Congo basin, between Kimbundu and Nge Njangwe, from July 31, 1881, to April 17, 1882. The Kioque, inhabiting the country along the Luelle and the Chikapa, among whom the two travellers journeyed for a month and a half, are described as an intelligent and enterprising people, expert smiths, hunters, and far-travelling merchants. Carrying on a large trade in gum, and soon exhausting a district of its gum produce by their inconsiderate method of going to work, they are in a state of perpetual movement towards the north. Almost all the ivory which reaches Loanda is forwarded thither by the Kioque from the Tuschilange country. The Tuschilange (sing. Kaschilange) or Baschilange (sing. Muschalange) are a mixed people, composed of the aborigines and the Baluba, who have entered the country from the south. Of the three divisions of them the central is the *Bena Riamba, i.e.* sons of wild hemp, so called from their excessive addiction to smoking that herb, which is smoked more or less in almost the whole of Africa, and produces an intoxicating effect combined with coughing. The *Bena Riemba* are forbidden to keep goats or swine, and the travellers during their stay among them suffered from the want of animal food. Crossing the splendid river of Lubi, the travellers passed from the land of the Baschilange to that of the Bassonge, who, according to Lieut. Wissmann, occupy the highest industrial position he had ever seen negroes hold. Artistic working in iron and copper, weaving, basket-making, carving, and pottery are all highly advanced among them. Living in fair villages with large clean houses, under the shade of palms and bananas, the men cultivate their trim fields, and leave only the lighter work to their wives—a relation in marked contrast to that existing among the peoples they had hitherto visited.

THE July number of Hartleben's *Rundschau für Geographie und Statistik* contains, among numerous others, the following original papers:—Researches concerning Madagascar, by J. Audebert.—On the Bedouins of Palestine, by R. Ranipendahl.—On the three first German "Geographentage," by Dr. Sigm. Günther.—On the United States of Columbia; these are remarks accompanying a good map of the States in question.

THE commander of the *Willem Barents*, now on her fifth North Polar expedition, has sent news to Amsterdam from

Solombola. Nothing had been ascertained regarding the fate of the steamer *Varna* or her crew.

At the meeting of the Berlin Geographical Society on the 8th inst. some communications were made regarding the latest undertakings of the German explorers now at work:—Dr. Paul Güssfeldt had undertaken to ascend the Aconcagua, the highest peak of the Chili Cordilleras (6934 metres); he failed on account of the extreme cold, but succeeded in taking a number of interesting photographs. Dr. Steiner, a member of the Antarctic expedition had proceeded northward from Punta Arenas, and had drawn a remarkable geological map of the country he traversed. He intends to penetrate into Chile. Dr. Hettner is about to start on an exploring tour through Canada with a view of discovering coal deposits.

News of the German African traveller, Dr. Fischer, has just arrived from Zanzibar. He was at some days' distance from Ngaren Erobi, had 800 followers, and had forced his way through the Massai district. He thus seems to have joined other caravans, as he had started with only 350 men himself. Ngaren Erobi is to the west of the Kilima Ngaro, and under 36½° E. long., and 3° S. lat.

Lieut. Bove is just starting on a second expedition to Terra del Fuego. Thence he intends to penetrate into Graham's Land. The Italian Geographical Society bears the cost of this expedition, which will sail from Genoa and go by way of Monte Video.

Dr. Oscar Lenz is now writing an account of his second great African journey. It will be published by Brockhaus (Leipzig), and will be entitled "Timbuktu, Reise durch Marokko, die Sahara und den Sudan, ausgeführt im Auftrag der Deutschen Afrikanischen Gesellschaft."

SCIENTIFIC SERIALS

Bulletin de la Société d'Anthropologie de Paris, tome vi. fasc. 1, 1883.—Presidential address.—Conditions to be observed by the competitors for the annual "Godart Prize" of 500 francs, founded in 1862; and for the "Broca Prize" of 1500 francs for the best memoir on a question of human or comparative anatomy, or of physiology referring to anthropology. This prize was founded by Madame Broca in 1881, and is biennial.—Report by M. Pozzi of a highly ornamented so-called medical pipe, found in an ancient mound in Kentucky. This fine specimen of the workmanship of the prehistoric mound-builders of the New World is identical with those found in California, and supposed to have been used for producing blisters and moxas.—M. Ball described the post-mortem appearances of the brain of the Batignolles cretin, whose abnormal condition had been brought to the notice of the Society last year.—On social instinct, by Dr. Prat.—On supposed human imprints found in clay beds at Carson in Nevada, by Dr. W. Hoffman.—An interesting paper on the superstitions and faith in sorcery still persisting in South Italy, by M. Maricourt.—On an anomaly of the brachial biceps, by M. G. Hervé.—On M. Hamy's Case of anthropometric instruments, approved of by the Society, for the use of travellers engaged in Anthropological determinations.—A case of hydrocephalus in a child of ten years, by Dr. de Grandmont, considered specially in reference to the ophthalmic lesions associated with this condition, and their probable joint dependence among other causes on too near relationship between the parents, as intermarriage between first cousins of degenerate constitution.—The reproduction in man of a simian muscle, the scalenus intermedius of the anthropoid apes, by Dr. Testut.—Observations on polyandry in Kouloo and Ladak, by M. Ujfalvy, based on personal investigations during his travels in the Western Himalayas. In Kouloo polyandry and polygamy subsist side by side; in Ladak with similar physical and economic conditions, polygamy, which necessitates a certain degree of material prosperity, is less frequent. The prevalence of polyandry among savage tribes in ancient times, and the organisation of matriarchy, or maternal supremacy, in tribal and domestic rule, were considered by M. Rousselet in the discussion which followed the reading of M. Ujfalvy's important communication.—A discussion on the anthropological study of the crania of great criminals, chiefly in reference to the connection of criminality with any fixed cranial malformation, by M. Manouvrier.—Considerations of the nature of the arterial sulci of the encephalon in man, by M. Danilo.—On the development of the human skeleton, by M. de Merjkowsky, with special reference to the embryological affinities between the higher and lower animals, the author believing that in the human fœtus we have a reproduction of a simian form, which gives support to the theory of development as applied to man.—An anomalous formation of the first rib, by M. G. Hervé.—On the brain of an insane person, by M. Rey, in which the frontal and antero-posterior circumvolutions were extraordinarily developed, together with an excessive weight of the brain.—On a successful attempt to inoculate a monkey with matter taken from an indurated chancre, by M. Pozzi.—On the substance used by the North American Indians to poison their arrows, by Dr. Hoffman.

SOCIETIES AND ACADEMIES

London

Geological Society, June 20.—J. W. Hulke, F.R.S., president, in the chair.—Henry Yorke Lyell Brown, Edward St. F. Moore, John Henry Nichols, and Henry Parker, were elected Fellows, and Baron F. von Richthofen, of Berlin, a foreign correspondent of the Society.—The following communications were read:—On the discovery of *Ovibos moschatus* in the forest bed, and its range in space and time, by Prof. W. Boyd Dawkins, F.R.S. The specimen described by the author formed part of the collection of the late Rev. F. Buxton, and was obtained by a fisherman from the forest-bed of Trimingham, four miles from Cromer. The edges are sharp, and the red matrix adhered in places, so that the author regards its geological position as satisfactorily established. It is the posterior half of the upper surface of the skull of an adult female *Ovibos moschatus*. The author describes the range in space and time of this animal, mentioning the different instances in which its remains have been found in Britain. These are, in some cases, undoubtedly post-glacial; but he inclines to consider the lower brick-earth of the Thames Valley, where the musk-sheep has been found at Crayford, as anterior to the boulder clay, which occupies the district to the north. This deposit at Trimingham, however, is certainly pre-glacial, and so *Ovibos moschatus* belongs to a fauna which arrived in our country prior to the extreme refrigeration of climate which characterised the glacial epoch, and afterwards retreated northwards to its present haunts, showing, with other evidence, that this epoch did not form a hard and fast barrier between two faunas.—On the relative age of some valleys in Lincolnshire, by A. J. Jukes-Browne, B.A.—On the section at Hordwell cliffs, from the top of the Lower Headon to the base of the Upper Bagshot Sands, by the late E. B. Tawney, M.A., and H. Keeping, of the Woodwardian Museum. Communicated by the Rev. Osmond Fisher, M.A. The authors, after a brief sketch of the literature of the subject and of the method which they have adopted in measuring the beds in the Hordwell section, passed on to describe these, viz. the freshwater Lower Headon series, and the so-called Upper Bagshot Sands of the Geological Survey. They make the whole thickness of the former 83½ feet. The bed numbered thirty-two in their section they identified with the Howledge limestone on the other side of the Solent. It is almost the highest seen in the section, and underlies the true Middle Headon which is now no longer exposed. The authors pointed out that in their opinion the late Marchioness of Hastings and Dr. Wright have somewhat misapprehended the position of these several beds. Details were then given of the remainder of the section, and comparisons made with the details published by former authors; after which the authors described the underlying estuarine series, or Upper Bagshot Sands, which has a thickness of 17½ feet.—On some new or imperfectly known Madreporaria from the Coral Rag and Portland Oolite of the counties of Wilts, Oxford, Cambridge, and York, by R. F. Tomes, F.G.S.—The geology of Monte Somma and Vesuvius, being a study in vulcanology, by H. J. Johnston-Lavis, F.G.S. The author, after referring to the vast amount of literature which has appeared dealing with the same subject, stated that his object was to lay before the Society the results of his personal observations. The external form and general features of Monte Somma having been described, the origin of the present condition of the volcano was discussed in some detail, and the geological structure of the mountain and of the surrounding plain, as revealed by well-sections, was carefully considered. As the result of his observations the author believes that he is able to define eight successive phases in the history of the volcano; and the events which took place during these several periods, with the products of the eruption during each, were

discussed in detail. The earliest certainly recognised phase in the history of the mountain was distinguished by chronic activity exhibited in outflows of lava and the ejection of scoria and ash. Possibly, however, a still earlier and paroxysmal stage is indicated by some of the phenomena described. Phase II. was a period of inactivity and denudation, which was brought to a close by the violent paroxysms of Phase III., followed by the chronic activity of Phase IV. Phase V. marks the return of a period of inactivity and denudation, which was again followed by the paroxysms of Phase VI. and the less violent outbursts of Phase VII., the last subsiding into the chronic activity which is the characteristic of Phase VIII., the modern period of the history of the volcano. The products of each of these periods of eruption were described in great detail. The eruptive phenomena which are illustrated by these studies of Somma and Vesuvius were then considered, together with the nature and result of the denudation which alternated with eruptive action in originating the present form of the mountain. The paper concluded with a statement of fifty propositions on the subject of vulcanology which appear to the author to be established by the studies detailed in the paper.—Note on "cone-in-cone" structure, by John Young, F.G.S.—A geological sketch of Quidong, Manaro, Australia, by Alfred Morris, F.G.S.

Anthropological Institute, June 12.—Prof. Flower, F.R.S., president, in the chair.—Dr. E. B. Tylor, F.R.S., read a paper on old Scandinavian civilisation among the modern Esquimaux. Amongst other evidences of contact with European civilisation, the author made particular mention of the lamps used by the Esquimaux for cooking and for warming their dwellings: one of these primitive-looking lamps was exhibited by Dr. John Rae, F.R.S.; it consists of a flat semicircular dish of steatite or potstone about 18 inches in diameter and 2½ inches deep, with slightly sloping sides; in it the natives burn oil, using for wick fragments of sphagnum arranged along the edge of the lamp. Dr. Tylor considered that the metal lamps used in the south of Europe, and some of those used in Scotland at the present day, were exactly the same in principle as these Esquimaux lamps, and that they must all have been developed from the same original idea.—The director read a communication from Mr. J. H. Rivett-Carnac, describing some palæolithic stone implements found by himself and Mr. J. Cockburn in Banda, a hilly district of the North-Western Provinces of India. Specimens of these implements were exhibited, presented by Mr. Rivett-Carnac to the Institute.—Dr. E. B. Tylor read a paper by Mr. A. W. Howitt, on Australian beliefs.

June 19.—A special meeting was held at Piccadilly Hall, by invitation of Mr. Ribeiro, to view the Botocudo Indians brought over by him to this country. Mr. Hyde Clarke, vice-president, was in the chair, and Mr. A. H. Keane read a paper on the Botocudos. Mr. Ribeiro presented the Institute with a small collection of typical Botocudo weapons.

June 26.—Prof. Flower, F.R.S., president, in the chair.—The election of Ernest G. Ravenstein was announced.—Mr. Worthington G. Smith exhibited a collection of palæolithic implements from Leyton and Walthamstow.—Mr. R. B. White read a paper on the aboriginal races of the north-western provinces of South America. This paper referred to a strip of country about 600 miles in length by from 100 to 250 in width, bounded on the west by the Pacific Ocean, and extending from one degree north latitude to the eighth parallel. It is now embraced by the States of Cauca and Antioquia, two of the nine states of the Columbia Union, which was formerly called New Granada.—Mr. J. Park Harrison read a paper on the relative length of the first three toes of the human foot. The author adduced evidence to show (1) that a long second toe was a racial characteristic existing at the present day in Egypt (according to Pruner Bey), South-west Africa, and many of the Pacific Islands, including Tahiti. It appears also to have prevailed amongst the ancient Peruvians and Etruscans; (2) when met with in Europeans, excepting perhaps in Italy, it may be attributed mainly to narrow shoes, but sometimes to mixture of blood; (3) Mr. Harrison had ascertained by measurements that a second toe even slightly longer than the first was not, as generally supposed, common in statues of the best period of Greek art, nor in accordance with the rules laid down in Flaxman's lectures at the Royal Academy; (4) unfortunately the peculiarity was being perpetuated by casts of the feet of Roman or Græco-Roman statues, which in some cases, as for instance that of the left foot of the Farnese Apollo, were modern restorations. Travellers were asked to observe the respective lengths of the toes in foreign countries and especially in Italy.

EDINBURGH

Mathematical Society, July 13.—Mr. J. S. Mackay, president, in the chair.—Prof. C. G. Knott read a paper on quaternions, and Mr. D. Munn one on radical axes and centres of similitude.

SYDNEY

Linnean Society of New South Wales, May 30.—Rev. J. E. Tenison-Woods, F.L.S., vice-president, in the chair. —The following papers were read:—Notes on a lower jaw of *Palorchestes Azael*, by Charles W. De Vis, B.A.—Synonymy of Australian and Polynesian land and marine mollusca, by John Brazier, C.M.Z.S.—On some Mesozoic fossils from Central Australia, by the Rev. J. E. Tenison-Woods, F.G.S. The author describes the nature of the deposit from qualitative analysis and microscopic examination, noticing the occurrence of various fossils too imperfect for specific identification. The author describes also the two new species, *Trigonia mesembria*, a clearly Cretaceous form of the section "Glabræ," and *Pecten psila*, which the author considers may only be a variety of *P. socialis*, Moore. He also described a *Belemnites*, probably *B. australis*, Phillips, of a very aberrant type of the section "Hastati." In conclusion, he considered that, as many of Moore's Wollumbilla (Jurassic) fossils were found in this formation, there was either a confusion of type, or that the Wollumbilla beds were part of the lower Cretaceous formation of Central and North-East Australia.—Contribution to a knowledge of the fishes of New Guinea (No. 4), by William Macleay, F.L.S. One hundred and thirty species of fishes are here recorded, chiefly from the extreme south-east of New Guinea, making, with those enumerated in the three previous papers, 409 species in all, collected by Mr. Goldie on the island. One new genus (Tetracentrum) and 33 new species are described, chiefly from fresh water.—A second half-century of plants new to South Queensland, by the Rev. B. Scortechini, F.L.S. The author enumerates 50 plants not previously quoted from Southern Queensland, and either belonging to the tropical flora of Northern Australia, or indigenous to the southern and temperate portions of the continent. He also notices some of the changes of nomenclature resulting from the fusion of the genera *Pithecolobium*, *Calliandra*, and *Enterolobium* with *Albizza*.

PARIS

Academy of Sciences, July 16.—M. Blanchard, president, in the chair.—On the whirlwinds of dust observed by Colonel Prejevalsky in Central Asia, by M. Faye. Like those of Mexico, India, and the Sahara these sandstorms are shown to have the same origin and mechanical action as the tornadoes of the United States and all waterspouts. They are all alike spiral movements descending with vertical axis and invariably moving horizontally nearly in a straight line. The popular belief that the dust on land and water at sea ascends from the surface to the higher regions is due to an optical illusion.—Active or dynamic resistance of solids. Graphic representation of the laws of longitudinal thrust applied to one end of a prismatic rod, the other end of which is fixed, by MM. de Saint-Venant and Flamant.—On the cause of death in the case of freshwater animals plunged into salt water and *vice versâ*, by M. Paul Bert. In the case of freshwater animals the fatal effect is caused by the action of chloride of sodium, a conclusion already arrived at by M. de Varigny. In the opposite case death is caused by the absence of chloride of sodium, which it is found impossible to replace either by salts of soda or of magnesia, by glycerine, sugar, or any other substances calculated to give fresh water the consistency of the marine liquid. Several interesting attempts at acclimatisation are described.—On the puna, or "mountain sickness," experienced by travellers at great altitude, by M. A. d'Abbadie. The symptoms are fully described, but M. P. Bert enters a protest against some of the suggested remedies, especially blood-letting.—On some of the results already obtained by the submarine explorations of the *Talisman*, by M. A. Gaudry. Amongst these results are several new species of mollusks, sponges, and crustacea.—On the separation of gallium from various substances (continued); separation from molybdenum, by M. Lecoq de Boisbaudran.—A fresh contribution to the study of intra-vascular sanguineous concretions, by M. G. Hayem.—Brief description of an electric indicator (one illustration), by M. J. Cauderay.—On the observation made by M. Gonneriat of the great comet of 1882 (one illustration), by M. Ch. André.—On the changes produced in the duration of the Julian year by the variations of the quantities on which this

duration depends, by M. A. Gaillot.—On the longitudinal impact of a prismatic rod fixed at one end and acted on at the other, by M. J. Boussinesq.—Remarks on the calculus of a definite integral, by M. R. Radau.—On surfaces of the third order, by M. C. Le Paige.—On a new theorem of dynamic electricity, by M. L. Thévenin.—On the currents of emersion and the movement of a metal in a liquid and currents of emersion, by M. Krouchkoll.—A new pile made of oxide of copper, described by MM. F. de Lalande and G. Chaperon.—On the density of liquid oxygen, by M. S. Wroblewski.—The salts of protoxide of gold, by M. Ad. Carnot.—On the alcoholate of barytum, by M. de Forcrand.—The action of aldehyde on propylglycol, by M. Arnaud de Gramont.—Researches on the extraction of cinchonamine, by M. Arnaud.—On a new glycerine, "Mesitplenic Glycerine," $C_6H_3(CH_2.OH)_3$, by M. A. Colson.—On coal as a heat-generator and on the conversion of its azote into ammonia, by M. Scheurer-Kestner.—A contribution to the history of the development of the heart (four illustrations), by M. Vulpian.—A comparative study of echinoderms: on the organisation of crinoids, by M. Edm. Perrier.—On the structure and texture of the spleen in the common eel, by M. C. Phisalix.—Physiological researches on the secretion of the Morren glands in the earthworm, by M. Ch. Robinet.—Researches on the structure of the breathing apparatus in cephalopods, by M. P. Gorod.—Changes and migrations of plant-lice. Complete biological evolution of the *Tetraneura ulmi*, by M. J. Lichtenstein.—On the colouring function of the *Drosera rotundifolia*, by M. P. Duchartre.—On the physiological part played by the undulations of the lateral walls of the epidermis, by M. J. Vesque.—Cloudiness at Bourges, with meteorological tables of observations from 1867 to 1881, by M. Hervé Mangon.—On the culture of quinquinas in Bolivia, and on some other agricultural products of that country, by M. Sace.

Berlin

Physiological Society, July 13.—Dr. Martius spoke on the nature of the heart's systole, more particularly as to whether it was a simple or a tetanic contraction of the heart's muscle. For some time many experiments have been made on this subject with the neuromuscular apparatus, but no secondary tetanus having been produced by the application of this physiological electroscope, it was concluded that the systole was no tetanic but a merely simple contraction. It was, however soon observed that other contractions, unquestionably tetanic, such as the voluntary tetanus, the strychnine tetanus, &c., generated no secondary tetanus, or at all events not in every case. The absence of secondary tetanus in the case of the heart's systole was therefore no conclusive proof of the simple nature of this contraction. Dr. Martius accordingly sought a more decisive means of settling the question, through the aid, namely, of the capillary electrometer, having first, however, made sure of the capability of the instrument he employed to follow with ease and certainty undulations of current of much greater frequency than occur in the case of the natural tetanus and reaching as high as forty per second. The capillary electrometer having then, by means of two needles thrust into a normal rabbit's heart *in situ*, been circularly closed, it was found that each systole responded by a merely simple displacement of the meniscus. The systole was consequently determined to be no tetanic but a purely simple contraction. Dr. Martius further described the following method towards an exact enumeration of very frequent vibrations of the capillary electrometer, which to the eye present merely the vanishing rim of the quicksilver cup. Let one fasten to the lever of a chronometric electromagnetic tuning-fork, instead of the pencil, a square piece of paper performing a known high number of movements per second. The square piece of paper will then appear to stand still and to have a gray border on its upper and under side. Let one next place this gray border between the ocular of the microscope and the meniscus of the capillary electrometer. Does the meniscus make just as many movements per second as the square piece of paper, the quicksilver cup will appear to stand still. Does, however, the number of movements not tally, the difference between the two will then be apparent and easily counted, and the number of movements on the part of the paper being known, the actual number of the movements of the quicksilver is also determined.—Prof. Kronecker gave a report on the experiments made by Dr. Jastrebow as to the mode, rhythmus, and innervation of the movements of the vagina of rabbits.—These communications were at the close illustrated by demonstrations.

Vienna

Imperial Academy of Sciences, May 4.—R. Maly and R. Andreasch, studies on caffeine and theobromine (fifth paper).—A. F. Reibenschuh, on methyl-biguanidine and its compounds.—F. Emich, on ethyl-biguanidine and its compounds; contributions to a knowledge of biguanidine.—W. Biedermann, on the excitability of the spinal cord.—T. Gerst, on the method of determining the orbit from three complete observations.—St. Wolyncervicz, on the determination of the orbit of the *Isabella* planet (210).—S. Wroblewski and K. Olszewski, on the liquefaction of nitrogen and carbon monoxide.—M. Neumayer, on climatic zones during Jurassic and Cretaceous epochs.—T. F. Wolfbauer, on the chemical composition of the water of the Danube near Vienna in the year 1878.—E von Fleischl, on the distribution of the fibres of the optic nerve over the cones of the human retina.

May 10.—C. von Ettingshausen, contribution to knowledge of Tertiary flora of Sumatra.—Dr. Steir, to the morphology and systematics of culmian and carbon flora.—F. Anton, definitive determination of the orbit and ephemeris of the *Bertha* planet (154).—Zd. Skraup and A. Cobenzl, on two chinoline base, naphthochinolines, formed of naphthylamines.

May 25.—A. Adam Riewicz, on the theory of brain-pressure and on the pathology of brain-compression.—A. Delbovier, report on prophylaxis and therapeutics of typhus.—T. Kachler and F. V. Spitzer, on the formation of isomeric camphor bibromides.—G. Niederist, on Reichenbach's picamar.

May 30.—Anniversary meeting.—The meeting was opened by the substitute of the Curator, Herr von Schmerling.—An address was given by Prof. Zeissberg, of the Historical Class of the Academy, on the youth of Archduke Charles.—The reports of the past year were read by the General Secretary, Prof. Siegel, and the Secretary of the Mathematical Class, Prof. Stefan. Then the obituary notes on the members deceased during the past year were read by the secretaries.—In the Mathematical Class Prof. Senhofer (Innsbruck) was elected member, E. Mojsisowics (Vienna), corresponding member. Prof. Richard Owen (London), W. E. Weber (Göttingen) were elected honorary members, Julius Schmidt (Athens), Hermann von Abich (St. Petersburg), Prof. Ferdinand Zirkel (Leipsic), foreign correspondents.—The Baumgartner prize was awarded to Carl Exner for his paper on the scintillation of stars, and the Lieben prize to V. R. Ebner (Gratz), for his experiments on the causes of anisotropism of organic substances.

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THURSDAY, AUGUST 2, 1883

ZOOLOGY AT THE FISHERIES EXHIBITION[1]

II.—*Notes on the Vertebrata*

At the request of the editor of NATURE I have drawn up this very general report on the Vertebrate animals now exhibited in the natural history sections of the different Courts at the International Fisheries Exhibition. In its compilation I have principally used the notes taken during a month's pretty close attendance at the great piscatorial show in South Kensington, and whilst doing work on the two special juries who had to examine and report on such collections. The space and time conceded preclude entirely anything like a detailed account even of this small portion of the rich and varied exhibit, whilst on the other hand books of reference could not be consulted, and strict nomenclature and systematic arrangement must be partly sacrificed. I shall, however, be content if I succeed in giving a fair general account of this special part of the Fisheries Exhibition, which cannot but interest many of the readers of this periodical to whom the sight of the exhibits themselves, some of very great interest, is not possible. I may also add that to my knowledge one group, that of Birds, will be the subject of a special article, to be published shortly in a special journal by one of our leading ornithologists, whilst on the other hand the Cetacea and Pinnipeda will be reported on in the jurors' reports by such distinguished specialists as Prof. Flower and Mr. Clark. I do not know whether any special report on the all-important and largely represented group of Fishes be imminent; I fear not; but as several highly competent ichthyologists have carefully gone over such collections in the Exhibition, I trust that so important a subject will also be laid before the scientific world by a competent reporter.

Before commencing my special task, and before taking the reader through the Vertebrate collections in the International Fisheries Exhibition, for which purpose I consider preferable a zoological to a geographical arrangement, I shall say a few words on the relative importance of the exhibits in this section contributed by different countries. Besides Great Britain and her dependencies, colonies, and possessions, such as the Isle of Man, Heligoland, Canada and Nova Scotia, Newfoundland, British Columbia, the Bahamas, Jamaica, New South Wales, Tasmania, India, Ceylon, and the Straits Settlements, the following foreign countries have contributed to the Fisheries Exhibition: France (not officially), Belgium, the Netherlands, Germany (not officially), Denmark (not officially), Sweden and Norway, Russia, Austria and Hungary, Italy (not officially), Greece, Spain, Switzerland, the United States, Chili, Venezuela, Haiti, China, Japan, Morocco, and Hawaii. Of these, however, nearly a third, viz. France, Germany, Italy, Venezuela, Haiti, Morocco, Japan, and Hawaii have no exhibit to call for our attention, while another third show so little, and that of so small a value that they hardly deserve a passing notice. In the richness, value, and beauty of the Vertebrata exhibited, the foreign countries who compete for the palm are Sweden and the United States of America, far above all the rest in this respect. Great Britain is, on the other hand, singularly defective, none of her great public institutions having taken any part in the competition; this may be partly accounted for by the close proximity of the Buckland Collection of Economic Fishery, adjoining the Fisheries Exhibition, while not much further is the new Natural History Museum in the Cromwell Road, and in this case it is much to be regretted that, at a time when many interested in fish and ichthyology have been attracted from afar by the Fisheries Exhibition, the zoological collections, and more especially the ichthyological ones, are not in a condition to be open to public inspection. However, if Great Britain is, with a single exception, meagrely represented by a few private exhibits in the Vertebrate collections, it is not so with some of her colonies and possessions, and the Courts occupied by the exhibits of New South Wales, Tasmania, and India are rich in specimens of much interest and great scientific value, while the Dominion of Canada is (in respect of Vertebrates) not far behind them.

The mammals ought in this case to be divided into two groups, those which are fished and those which fish, but I prefer to classify them scientifically rather than popularly. Carnivora mostly belong, when aquatic, to the latter group; amongst the more abundant are of course the otters, and especially our European kind, of which many specimens are in the British Natural History Gallery; Canada, India, and Chili show specimens of those belonging to their waters, and I was pleased to see in the latter Court fine specimens of my old friend *Lutra felina*, whose marine habits and agility amongst the kelp-beds of Western Patagonia I witnessed many a time. A few Polar bears are also shown, a very large and fine specimen being in the Russian Court; while Musteline Carnivora of more or less fishing propensities are to be seen amongst the Canadian exhibits. Seals and *Otariæ* form of course a prominent feature in the Exhibition; foremost in beauty and rarity is no doubt *Histriophoca fasciata* of the Behring Sea; the *Vega* exhibit shows a skull and a rather inflated and dilapidated skin, whilst a magnificent specimen is exhibited by the National Museum, Washington; these are, I believe, the first specimens of that rare mammal ever seen in this country. Some good specimens of *Cystophora*, *Ph. barbata*, *Ph. grœnlandica*, *Ph. gryphus*, are to be seen in the Canadian and Newfoundland exhibits, the latter belonging mostly, I am told, to the Liverpool Museum. Tasmania shows a splendid specimen of *Stenorhynchus leptonyx*. A large but badly mounted walrus is in the British gallery; but the enormous tusks and cranium and the life-like head of the Pacific species (*T. obesus*), of whose specific distinctness I should however greatly doubt, call for special attention in the United States department; the very beautiful sketches from life of those unwieldy creatures and of the agile fur seals, drawn by Mr. Elliott in the Pribylov Islands, deserve much praise. The *Otariæ* are represented by a fine group of *O. ursina* ♂ and ♀, the principal source of the sealskin industry, in the United States exhibit, mounted very beautifully indeed; an interesting group of *Arctocephalus cinereus* is conspicuous in the New South Wales Court, in which is a young specimen of what appears to be a distinct species; Chili has

[1] Continued from p. 291.

several interesting specimens of an *Otaria* from Juan Fernandez, a true *Loboa dospelos*, which might be the rare *Otaria Philippii*.

The strange and uncouth Sirenia are represented by a grand specimen, one of the principal attractions to every naturalist in the entire Exhibition, the nearly complete skeleton of a *Rhytina Stelleri*, which, with other bones of that most interesting creature is exhibited by Baron Nordenskjöld, one of the many grand results of the *Vega* expedition; the National Museum of Washington shows a very fine skull of that peculiar, rare, and extinct Sirenoid. Very interesting, and more noticed by the general public, are the two fine mounted specimens, male and female, of the Dugong (*Halicore australis*), exhibited by the Australian Museum of Sydney.

The Cetacea contribute an important portion of the Vertebrate series, and now and then afford instruction of a novel and rather startling nature; thus the large skeleton of *Balænoptera musculus*, covered with luminous paint and set up in the Garden, shows some remarkable innovations in practical osteology, the natural asymmetry of the skeleton of these creatures is most vividly exaggerated, and we are shown various of the larger paired bones curiously displaced from right to left, and *vice versâ*; but this is not all, we are told that the whale before us, which by the way was noticed by no less a man than Prof. Flower, when cast upon these shores, is the Greenland Whale (*Balæna mysticetus*), and the large label thus headed further informs us that it grows to be 75 feet long, swims at the rate of four miles an hour, and possesses a tongue so thick and fleshy, that when the mouth is closed it envelops the upper jaw and all the horny laminæ (baleen plates) along it! Not far off a Berlin dealer in whalebone, Isaac Mann, shows a fine series of baleen plates belonging to several species, but he startles us with the announcement in large letters that "the whale can grow to the length of 200 feet, reach the age of 1000 years, the weight of 20 tons, and is therefore the largest of known fishes." But from the comical and amusing, let us return to more serious and interesting matter; amongst the mounted and entire specimens of Cetacea exhibited, I may mention the large and beautiful *Orca gladiator*, which forms a prominent feature in the Swedish Court, five young and fœtal porpoises preserved in alcohol, shown by the Gothenburg Museum, and by the Norwegians; the large *Beluga* in the Canadian exhibit, less life-like, however, than the beautiful cast of the same species shown by the National Museum of the United States; special notice ought to be taken of the rare *Orcælla brevirostris* from Singapore, in the Straits Settlements exhibit. Skeletons and crania of Cetaceans are more numerous, and for the high scientific value and beauty of specimens exhibited Sweden has in this respect by far the highest rank; the complete skeletons of *Orca Eschrichtii*, *Hyperoodon diodon*, and *Mesoplodon bidens*, will be examined by all zoologists with pleasure and profit, but of more special interest is that of *Ziphius Gervaisii*. This form, which differs principally from *Z. cavirostis* in the absence of the stony *mesorostral* bone, and in the size and shape of the two teeth at the apex of the mandible, is probably the female of the latter; whilst examining again that most valuable specimen yesterday, I was grieved to find that some unprincipled person had abstracted the two teeth, an act of ruthless vandalism or pseudo-scientific kleptomania much to be deplored and condemned.

Birds, of course, figure largely in the British and foreign exhibits; they are more or less aquatic, and may or may not fish or otherwise prove injurious to piscatorial interests. It is to be hoped that the public will not take for granted that every bird displayed in this Exhibition is the fisherman's natural enemy and therefore to be ruthlessly destroyed whenever the opportunity occurs. The dipper, for example, largely repays any occasional injury he may do to the fish spawn by destroying a vast number of insects which habitually feed on it.

Amongst the notable exhibits in this series in the British section is a fine collection of British waterfowl very nicely mounted, shown by T. E. Gunn of Norwich, in which a pair of hoodies attacking a wounded widgeon and a pike drawing under water a female mallard are very effective. Mr. Burton's collection of New Zealand waterfowl is also good; and especially worthy of praise is a set of beautiful photographs illustrating bird-life, and more especially the gannets on the Bass Rock and Fern Islands, exhibited by W. P. Carr of Berwick. India, Australia, and the United States show a fair exhibit of their waterfowl, especially Anatidæ, Ardeidæ, Laridæ, Procellaridæ, and Spheniscidæ; but by far the most important exhibits in this class are the rare Arctic birds from the Behring Sea and Alaska in the Swedish and United States Courts. Ornithologists will look with unmitigated delight on the splendid specimens of *Eurinorhynchus pygmæus*, *Colymbus Adamsii*, and *Rhodostethia Rossii* in the *Vega* exhibit; and on the magnificent *Bernicla canagica*, *Somateria Fisheri*, and *Somateria V-nigrum*, shown both in the *Vega* and in the National Museum of Washington exhibits; some of these species are seen, I believe, for the first time in this country. A large collection of water-birds of North America, some three hundred species, has besides been sent over in skins by the National Museum of Washington; these, however, have not been exhibited for want of space.

Reptiles contribute a small but not uninteresting series to the Fisheries Exhibition. Amongst the Chelonians the most noticeable are a fine *Sphargis coriacea* shown by the Australian Museum of Sydney; a large specimen of *Chelonia imbricata* in the Spanish exhibit from the Philippines; several large turtles, *Emys* and *Trionyx*, in the Indian show, where may also be seen several large crocodiles and a set of snakes, amidst which several species of that most difficult but interesting group the Hydrophidæ, from Karachi. The United States National Museum shows some fine casts of turtles, tortoises, snakes, and lizards, amongst the latter a very fine one of the recently described poisonous lizard of Arizona (*Heloderma*).

The Amphibia are represented by a complete set of the North American Urodela exhibited in the United States section, while a few Anura are shown by India, and in the Chilian Court may be seen a few more, amongst which is the curious *Calyptocephalus Gayi*.

Fish naturally contribute the larger portion of the Vertebrata exhibited; in the British gallery may be seen a very great number of the common freshwater game and food fishes exhibited principally by anglers and by angling clubs, mostly mounted dry, and of little or no scientific interest. A small set of freshwater and marine British

fishes shown by T. E. Gunn and Mr. Carr are noticeable; but of very great interest is the large and nearly complete collection of British fishes exhibited by Dr. Francis Day, they are of special value as being a set of types used by Dr. Day for his work on the fishes of Great Britain and Ireland at present in course of publication. A few interesting Mediterranean species of fish may be seen in the magnificent series of Invertebrata shown by Prof. Anton Dohrn, founder and head of the Zoological Station at Naples; amongst them are two specimens of *Callionymus partenopœus*, Gigl., the young of *Scymnus lichia*, *Centrina Salviani*, *Scyllium stellare*, and *Myliobatis bovina*; a *Fierasfer imberbis* is shown in the act of getting into a large *Holothuria*, whilst a specimen of the rare *Fierasfer dentatus* is of special interest. Good skeletons in alcohol of *Ceratodus Forsteri* and *Cestracion Philippii* are exhibited by Mr. Gerard, jun., and some well mounted disarticulated crania of fish are shown by Mr. Moore.

Besides a large set of the admirable casts of the more conspicuous of their food-fishes, and a splendid series of large photographs of many typical forms of their rich ichthyo-fauna, the United States (National Museum and Fisheries Commission of Washington, both under the able and energetic direction of Prof. Baird) exhibit a most interesting and complete series of type representatives of the freshwater genera of North America; the series embraces 173 species, amongst which the Ganoids, so well represented in that region, as *Amia*, *Lepidosteus*, *Spatularia*, *Scaphirhynchus*, and *Acipenser* deserve special notice. A collection of thirty-eight nominal species of American Salmonoids are also exhibited, and an interesting set they are; these two sets are mostly represented by specimens preserved in alcohol. The National Museum of Washington has also sent over a fine and highly interesting collection of the fishes of Alaska and another of those of the Gulf of Mexico and East Florida, all alcoholic specimens, and not exhibited from want of space. Prof. Brown Goode kindly showed me some of them; the former contains about 100 species, the latter 159. The Alaskan collection is of special interest, and contains many species recently described by Goode, Bean, and other ichthyologists.

In the Canadian Court a numerous series of mounted and alcoholic fish is exhibited, mostly freshwater and well known food-fishes; large specimens of Salmonids, Clupeidæ, Esocidæ, Sturgeons, and Halibuts may be seen, and a curious *Læmargus borealis* and a very large *Orcynus thynnus* deserve notice. Some very large Cod may be mentioned in the Newfoundland Court, whilst on the other side of the equator in the new continent, Chili shows a collection of food-fishes, principally marine and from Juan Fernandez, the highly esteemed "Peje Rey" (*Atherinichthys*) and *Heliastes crusma*, a large representive of our interesting Mediterranean species, may be recorded.

Sweden shows a magnificent collection of her Salmonidæ, large and beautiful specimens wonderfully preserved in alcohol in the finest of glass jars; an interesting series of types and embryos and larval fish is besides shown by the Gothenburg Museum, but of special interest is the *Vega* collection from the Arctic seaboard. In the Russian Court a good collection of mounted fish is exhibited, amongst which are to be noticed a large *Silurus glanis*, a fine *Hippoglossus*, very fine and large specimens of *Lucioperca sandra*, an excellent food-fish, which with greater profit than the black bass of America might, I believe, be introduced into British waters; besides a fine set of the various species of sturgeon which abound in Russian waters, and lastly some good enlarged wax models illustrating the development of *Acipenser* and *Petromyzon*.

Norway again deserves notice as exhibiting some very fine specimens of rare fish preserved in alcohol or mounted; I may particularly mention *Argentina silus*, *Argyropelecus Olfersii*, *Sebastes norvegicus*, with embryos taken alive from the female, *Raja niderosiensis* (the type), *Scymnus microcephalus*, and a fine Opah (*Lampris guttatus*).

New South Wales (the Australian Museum of Sydney) has one of the very best ichthyological exhibits; besides very beautifully mounted specimens, and very well preserved alcoholic ones, a set of splendid coloured drawings from nature and of natural size, and a large series of photographs of fish are exhibited. Most of the remarkable forms and of the peculiar species of the fish-fauna of Australia are represented. I may specially mention those living fossils *Ceratodus* and *Cestracion*, both represented by two species, the former *C. Forsteri* and *C. miolepis*, the latter *C. Philippii* and *C. galeatus*; *Ceratodus miolepis*, exhibited in a dry skin, is the companion specimen to the type. Amongst others of the many interesting species exhibited may be mentioned *Galocerdo Rayneri*, *Carcharodon Rondeletii*, *Crossorhinus barbatus*, *Rhinobatis granulosus*, *Odontaspis taurus*, *Trygonorhina fasciata*, *Myliobatis australis*, *Rhina squatina*, *Temnodon saltator*, the singular *Glaucosoma*, with its mussel-like opercular appendage, &c. Some of the freshwater food-fish, as *Oligorus*, *Ctenolabris*, and *Therapon*, are noticeable. A remarkable sun-fish is also exhibited; it differs from our species in shape, in the size and form of the caudal rays, and lastly in being covered with small carinate horny scales, which appear to cover the osseous granulations of the dermis; I am inclined to think that it differs from our *O. mola*, belongs to the southern hemisphere, and if so, might go by the name of *Orthragoriscus Ramsayi*, as a just acknowledgment to Mr. E. P. Ramsay, Curator of the Australian Museum, who brought it over, and to whose intelligence and activity the splendid exhibit of the New South Wales Court is entirely due.

Tasmania shows a collection of stuffed and alcoholic fishes, some very interesting. A fine *Lophotes cepedianus* deserves special notice, as also specimens of *Galaxias*, *Retropinna Richardsoni*, *Histiopterus recurvirostris*, *Phyllopteryx foliatus*, and *Pristiophorus cirratus*.

India exhibits a very large collection of mounted and spirit specimens, from Madras and Bombay principally; worthy of special mention are fine specimens of *Histiophorus gladius*, *H. belone* (?), *Cybium guttatus*, *C. Kuhli*, *Caranx sansun*, *Megalops indicus*, *Drepane punctata*, *Corinemus lysan* (so like our *Lichia vadigo* in appearance), *Polyphemus plebejus*, *Thynnus thunnina*, the beautiful *Murœna tessellata*, *Barbustor*, *Catla Buchanani*, *Wallago attu*, *Macrones seenghala*, and other peculiar freshwater forms; some interesting Elasmobranchs, as *Rhynchobatis djeddensis*, *Stegostoma tigri*, *Trygon uarnak*, and a *Dicerobatis*, very like the Mediterranean species.

Scientifically, however, the most important ichthyological collection exhibited in the Indian department is beyond doubt that shown by Dr. Day—fine specimens in alcohol of several hundred species illustrated in his great work, "The Fishes of India." Dr. Day also exhibits a set of his coloured drawings of Indian fish.

The Straits Settlements exhibit a fair sample of the sea-fish of that region, unfortunately unnamed; there are also a few freshwater fishes from Singapore.

China has a rich and interesting collection of fish, and also some very good drawings of them. Unfortunately they also are unnamed. The fishes exhibited are principally in alcohol, and come mostly from Swatow; some are very rare, and others appear to be new to science; amongst those of some interest I may mention: *Elacate niger*, *Rhynchobatis ancylostomus*, *Zygæna malleus*, *Cestracion zebra*, and some fine species of *Pteroplatea*, *Trygon*, *Raja*. One fish of special importance is *Polyodon gladius*, from Tchang.

I have now finished, and hope I have been successful in giving a fair general sketch of the Vertebrata shown in the International Fisheries Exhibition; some of the contributions might, no doubt, have been better, but on the whole we may well be content with the opportunity thus given of seeing many good things.

London, July 17 HENRY H. GIGLIOLI

STELLAR NAVIGATION

Stellar Navigation, with New A, B, *and* C *Tables for Finding Latitude, Longitude, and Azimuth by Easy Methods.* By W. H. Rosser. (Published by Norie and Wilson, 1883.)

THERE can be no doubt that star observations, when the horizon is clear and well defined, are the best means by which the position of a ship at sea can be ascertained; as, by altitudes of two or more stars, in suitable positions with regard to the observer, the latitude and longitude can be obtained at the same moment, whereas single observations of heavenly bodies only give one element, and consequently it is not possible to obtain simultaneous observations for both elements during the day, unless either the moon, Venus, or Jupiter passes the meridian whilst the sun is above the horizon.

It is true that when the azimuth of the sun is changing rapidly the latitude as well as the longitude can be obtained from two sets of observations, taken at a given interval of time, provided the alteration in the position of the ship, during that interval, can be accurately determined; but this supposes a knowledge, not only of the course and distance traversed during the interval, but also of the tidal stream or current affecting the ship, which is usually uncertain.

Any writer or teacher, therefore, who impresses on navigators and students the importance of obtaining star observations is deserving of praise, for it is impossible to take too much precaution in ascertaining the position of a ship; cloudy or foggy weather may set in at any moment, and an opportunity lost can never be recovered.

Mr. Rosser has in the *Nautical Magazine* drawn attention to the value of Sumner's method of working out simultaneous observations of two or more stars, and there is little doubt that it is the best, as it is the only method by which results obtained from simultaneous observations of three or more heavenly bodies can be readily combined. It has been for years constantly used by the naval officers employed on surveying service, and in fact by most navigators, though, perhaps, they seldom take observations of more than two stars at the same time. We however prefer three for precisely the same reason we prefer three to two chronometers.

Sumner's method may be thus briefly described. As at a given moment of time each heavenly body is at the zenith at some points on the earth's surface, so at that moment circles may be described on which its altitude will be 80°, 70°, 60°, &c. If then the altitudes of two stars are obtained at the same instant, and the Greenwich time be known, the two circles of altitude may be drawn on the earth's surface with the points where the stars are in the zenith as centres, and the point where these circles cut will be the position of the observers. In actual practice it is not necessary to draw the circles, it is merely necessary to be able to draw the arc of a small portion of each circle; for the position of the observer being generally known to within twenty miles, the arc of the circle of altitude on which he is situated can be readily drawn.

The method of obtaining this arc of altitude formerly practised was to calculate the longitude with two latitudes, using the same two latitudes for each star, which gave four resulting longitudes; then, by plotting these four longitudes on the two parallels, and drawing lines joining the longitudes given by each star, two circles of altitude were obtained, which either cut in a given point, or would do when produced, which point was the position of the observer.

This method of calculation was however quickly discarded for a more simple one, where one latitude only was used; for as the azimuth of a heavenly body can be readily calculated at the same time as its hour angle, and the azimuth being the bearings of the place where the star is at the zenith from the observer, it is evident that a line drawn at right angles to the azimuth will be the arc of the circle of altitude on which the observer is situated, as practically the arc is, for so short a distance as twenty or thirty miles, a straight line. The two longitudes on one parallel with the azimuth enable the two arcs of altitude to be plotted as before.

The importance of Sumner's method has not as yet been pointed out in any treatise on navigation, principally because since the time of Lieut. Raper, R.N., no treatise has been written by a practical navigator. It is true the method is mentioned in Riddle's "Navigation," and was taught by him many years since, though not in the form now adopted, and we think Mr. Rosser has done good service by urging its importance and the importance of stellar navigation generally. All navigators should in our opinion obtain star observations every night and morning, during twilight, as constant practice will alone render them expert in these observations, and familiar with the positions of the stars.

The extra work entailed by such observations will be amply repaid if, when standing in towards the land, after three or four days' thick weather, a partial break in the clouds enables the expert navigator to secure a couple of star observations which give him his position and enable

him to direct his course with confidence towards his point of destination. Whilst, however, giving Mr. Rosser credit for his advocacy, we cannot but regret he has thought it necessary to pad his pamphlet with problems which are in every good treatise on navigation, and with tables which are either useless or are to be found in a more complete form elsewhere.

In "Stellar Navigation," Problems I. to X. are simply repetitions from works already published, and we notice that in the examples given of obtaining hour angle and azimuth (pp. 9, 10, and 11), Mr. Rosser seems to be unaware of the existence of Raper's tables of logarithms of the log. sine square. Problem XI. is an example of Sumner's method, and is well explained, excepting that we think it far better and quite as quick a process to calculate the azimuth with the hour angle rather than refer to another set of tables. Problem XII. is what is called the new navigation, and is merely another, and in our opinion less simple, way of arriving at the same result as Sumner. Problem XIII., or Paget's method, is merely to attain the position by calculation instead of by plotting on a chart the two circles of altitude, and as this can be done by two plane triangles we should hardly have thought it required explanation. Problem XIV., to compute the altitude of a heavenly body, will be found in all treatises on navigation.

The Tables A and B are useless, for they are merely a complicated method of finding the error of longitude due to an error of one mile of latitude, which can be readily ascertained by the ordinary traverse-table. Table C., on azimuths, may be, as before stated, as readily and more accurately calculated at the same time as the hour angle. Table D is a combination of two tables invariably given in all treatises on navigation.

Table I., or mean places of stars, is given in the *Nautical Almanac*, which every navigator possesses; Table II. is given more elaborately in Jean's handbook for the stars, which every navigator should possess; and Tables III. and IV. are given in the *Nautical Almanac*.

THE STUDENT'S MECHANICS

The Student's Mechanics. By W. R. Browne. (London: C. Griffin and Co., 1883.)

THIS work, we are told in the Preface, "differs from the many previous works on the subject mainly in the fulness and care with which the foundations" (of mechanics) "have been considered," and it aims at such a treatment of the subject that the student may apply its principles "confidently in attacking questions of practical importance."

The book is characterised by a considerable amount of original and independent thought, especially in the earlier portion treating of First Principles. This is largely due to the definition of matter which is given:—"Matter consists of a collection of centres of force distributed in space, &c." We are not aware of any writer who has employed this hypothesis to deduce and explain the fundamental laws of mechanics in an elementary treatise. Nor does it seem to us at all well adapted to elementary students. It is so very important that they should see that mechanics depends, at every stage, in the establishment of its fundamental laws, on experiment, and also that they should know what the experiments are and in what way they serve to establish the laws, that the deductive method adopted by Mr. Browne, which does not sufficiently exhibit this connection, would seem to be unsuitable for the purpose he has in view. For though he explicitly states, once or twice, that the science of mechanics rests on experimental evidence, he does not point out the way in which it so rests, nor where the necessity for experiments comes in. As a specimen of his purely deductive method and, at the same time, of poor logic, we have a proof given on p. 9 which reads thus:—"We have defined a force as a cause of motion. Hence we see that, if a force has produced motion, it will be represented to us by the motion it has produced. . . . But motion is measured in terms of velocity. Hence, other things being equal, forces are measured by the velocities which they cause or generate." By the expression "other things being equal" must be understood (Art. 30) that "the things they act upon must be equal" (in what respect—of weight, volume, or mass—is not stated, although, from an illustration previously given, we are, presumably, to infer that their weights must be equal). If we substitute for *force*, *amplitude of vibration*, and for *motion generated*, *intensity of illumination*, all through the above proof, the reasoning will be equally plausible, and the conclusion false. Of course all that can be inferred from the fact of force having caused motion, apart from experiment, would be that the velocity might be expressed as a function of the force.

A possible source of much confusion to the student exists in the old-fashioned division of forces adopted in this book into statical, moving, and accelerating forces. The confusion will be increased by the introduction, in addition, of the more modern word "acceleration." In Art. 348 we have f called the acceleration in the formula $P = Mf$, whilst g is called the accelerating force of gravity; whilst in Art. 422 the actual tractive force P exerted by an engine on the following train is called an accelerating force.

The proof in Art. 359 is incomplete, owing to its not recognising the fact that the sum of an infinite series of vanishing quantities may be a finite quantity.

A valuable feature of the book is the prominence that is given to, and the early introduction of, the theory of the conservation of energy. The friction of machines is deduced from this principle in a very simple manner. The theorems of statics are very clearly put before the reader, and much that is suggestive and valuable is contained in the articles on elasticity and on the action of railway-brakes.

The book is one which may be read with profit by a student who is already familiar with elementary mechanics and is not liable to be confused by the peculiarities alluded to above, but does not seem to be adapted to students who approach the subject for the first time.

OUR BOOK SHELF

Manual of Taxidermy. A Complete Guide in Collecting and Preserving Birds and Mammals. By C. J. Maynard. Illustrated. (Boston: S. E. Cassino and Company, 1883; London: Trübner and Co.)

THIS small volume of 100 pages of thick paper contains the ordinary instructions for skinning, preserving, and

mounting birds and mammals, given very briefly, but probably with sufficient detail to serve as a guide to beginners. The author appears to be a dealer in natural history accessories, and the book has rather the aspect of a trade advertisement from its recommending the almost exclusive use of a "preservative" prepared and sold by the author, the composition of which he keeps secret. As a practical guide to English collectors in foreign countries it is very inferior to Mr. Ward's "Sportsman's Handbook," which was reviewed in NATURE last year (vol. xxvii. p. 146). A. R. W.

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications.*

[*The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to insure the appearance even of communications containing interesting and novel facts.*]

The Meteorological Council and Falmouth Observatory

THE Meteorological Council contemplate closing on December 31 next the Primary Observatories at Glasgow, Armagh, Stonyhurst, and Falmouth, which have been in full operation since 1868, and continuing only those at Kew, Aberdeen, and Valentia.

The Falmouth Observatory has a geographical position which insures it the first record from the south, and the position of the instruments is considered satisfactory by scientific men. It is superintended and managed by the Royal Cornwall Polytechnic Society, who for the small sum of 250*l.* per annum provide suitable buildings, an observer, assistant observer, gas, and the other necessary outgoings, thus supplementing by local effort the Treasury grant.

The Meteorological Office have been satisfied with the manner in which the Observatory has been managed. The accompanying report, which Prof. J. Couch Adams of Cambridge sent to the Meteorological Council at their own request, deprecates, on scientific grounds, the retrograde step contemplated by the Council, and I am requested by my Committee to invite through you the assistance of scientific men generally to prevent the discontinuance of so important an observatory as the one at Falmouth. EDWARD KITTO,

Secretary to the Royal Cornwall Polytechnic Society

Falmouth, July 30

Copy of the Document submitted to the Meteorological Council by Prof. J. Couch Adams, F.R.S., on July 5, 1883.

To the Members of the Meteorological Council.

In compliance with the wish expressed by some members of the Council at the interview of June 27, I have great pleasure in explaining my view on the matter then discussed more fully and clearly than I was able to do *vivâ voce*.

1. First I will say a few words about the relative value from a scientific point of view of a continuous record of meteorological phenomena when compared with occasional observations of the same phenomena.

In my opinion the continuous record would be in this case incomparably the more valuable. When we know the laws of variation of an observed quantity, occasional observations at intervals which may be settled beforehand are sufficient to determine all the constant quantities which enter into the expression of the law. On the other hand, when the law of variation is in a great measure or altogether unknown, as is the case with most meteorological phenomena, a continuous record may throw more light on the law or laws of variation than would be afforded by any amount of occasional observations.

I have no hesitation in expressing my belief that if we ever attain to a knowledge of the principal laws which regulate the weather, it will be as a result from continuous records, and not from occasional observations.

2. In the second place, in order to study the laws of variation of any particular phenomena, it is important to have continuous observations at different places which are not so far distant from each other as to make the circumstances of the phenomena at the different stations differ too widely from one another.

In this way only will it be practicable to study and trace the progress of a wave of disturbance of any kind across a given country. From this point of view I do not think that seven stations judiciously distributed over the surface of the British Isles are at all too many. Hence I should regard the proposed abandonment of four out of these seven stations as a retrograde step which is greatly to be deprecated.

3. In the first place I come to the circumstances which relate to the Falmouth Observatory in particular The unique situation of Falmouth, nearly at the mouth of the English Channel, and considerably to the south-west of any of the other meteorological stations will render continuous observations made there peculiarly valuable. Most of our storms and other atmospheric disturbances come from the south-west, and therefore they would first affect and be recorded by the instruments at Falmouth. Valentia is the only other station which can compare with Falmouth in this respect, and I should consider the observations at Falmouth more valuable, as its more southerly situation enables us better to trace the progress of any disturbance across the southern and the central parts of England by comparison with other observations in those parts, while Valentia is too much to the north to answer this purpose.

4. Next I will consider the objection which has been brought against further continuing these observations, viz. that they have already been continued for twelve years, and nothing of importance has been deduced from them. Considering the complicated nature of the phenomena we are concerned with, it is not to be wondered at that little or no progress has been made in twelve years in unravelling their laws. Even in astronomy, if the fate of the Greenwich Observatory had depended on the results deduced during the first twelve years of its existence from the observations made there, the consequences to the progress of the science might have been disastrous. The fact that we already have twelve years' continuous observations at a given place makes any additional observations at the same place much more valuable. Thus twenty-four years' continuous observations at the same place would be much more valuable for any theoretical deductions than twelve years' observations at one place and other twelve years' observations at a different place.

5. There can be no doubt that one of the principal astronomical conditions by which meteorological phenomena are affected consists in the varying motion of the moon in declination, and this again depends on the position of the moon's node, which takes between eighteen and nineteen years to perform a complete revolution.

Hence it would be desirable that meteorological observations should be continued at the same place during one or more revolutions of the moon's node.

This is already well recognised to be necessary in the case of tidal observations. And here I may incidentally remark, though it does not directly affect the Meteorological Council, that Falmouth would be a very important station for making continuous observations of the tides.

6. If the present grant were withdrawn from the Falmouth Observatory, the Cornwall Polytechnic Society have not the means of keeping it up, and the abandonment of the Observatory would be a heavy blow to the cultivation of meteorological science in Cornwall and the West of England generally, where there are many local stations which regard Falmouth as their scientific centre. This is a matter which ought not to be indifferent to the Meteorological Council. No doubt it is no part of the duty of the Council to subsidise local efforts, unless indeed by means of such efforts the objects of the Council can be better and more economically carried out than would otherwise be done. I submit that this is the case in the present instance. The difference between the expenditure at Valentia, where the Meteorological Office has to defray the whole cost of the establishment, and the expenditure at Falmouth affords some indication of the advantages to be derived from local efforts.

7. Lastly, if it is absolutely necessary to reduce the expenditure on some branches of the work undertaken by the Meteorological Office, it may be inferred from what I have already said that in my opinion the continuous records are almost the last branch in which any reduction should take place.

(Signed) J. C. ADAMS

Determination of "*H*"

IT has occurred to me that the following notes of a rough determination of the value of the horizontal component of the

earth's magnetism, according to the method described by Mr. Andrew Gray (NATURE, vol. xxvii. p. 32), might not be without interest to some readers, as showing the amount of accuracy which can be obtained. The experiments were made by one of my students at this College about four months ago.

The form of reflecting galvanometer which lends itself best to these experiments is one devised by Prof. Stuart, in which the needle is centrally situated between two rectangular pieces of wood carrying the coils. To the sides of these, two boards can be easily attached by brackets, so as to be in the same plane as the needle, and quite horizontal, and in this position do not interfere with the light falling upon or reflected from the mirror. The reflecting magnet is then north or south of the needle, and perpendicular to it.

The magnets were made from thin knitting needles (about No. 19, B.W.G.), cut to the proper length, and made glass hard. They were made in two lengths, 8·5 and 12·5 cm., but the longer ones were slightly warped in hardening, and did not give concordant results. The scale was at a uniform distance of 62·5 cm. from the mirror, and in reading the deflections four observations were made and again repeated after noting the times of oscillation, as described by Mr. Gray. Each of the deflections given below is therefore the mean of *eight* observations.

The following are the details of the experiments :—

Denomination of magnet.	A.	B.	C.
Length	8·5 cm.	8·5 cm.	8·5 cm.
Weight	0·6760 grm.	0·6924 grm.	0·6900 grm.
Time of oscillation	4·88 sec.	4·71 sec.	4·76 sec.
Deflection at 15 cm.	7·1 cm.	7·7 cm.	7·6 cm.
,, 13 ,,	10·6 cm.	11·5 cm.	11·2 cm.

From these results we obtain, by aid of the formula :—

$$H = \sqrt{\frac{4}{3} \frac{\pi^2 l^2 w}{(r^2 + l^2)^{\frac{3}{2}} T^2 \tan \theta}},$$

the following values for H:—

0·17705
0·17635
0·17828
0·17754
0·17725
0·17770

Mean = 0·17736 ± 0·00048,

showing an amount of accuracy which may, I think, be compared with that obtained with much more expensive and delicate apparatus.

T. S. HUMPIDGE

University College of Wales, Aberystwyth, June 27

The Lachine Aërolite

THE most remarkable fall of an aërolite that has yet been recorded took place at Lachine, about eight miles from Montreal, on Saturday, July 7, 1883. I give the following account from the Montreal *Daily Star* of July 11 :—

"The fall of the aërolite transpired during a rain shower on the forenoon of Saturday, and there were no premonitory indications to show that the air was more than usually charged with electricity. The person who witnessed the fall of the aërolite more clearly than any one else was Mrs. Popham, wife of Mr. John Popham, insurance agent. Mrs. Popham was seated in her house up stairs sewing, when all of a sudden the apartment became illuminated with a blinding flash of light. The lady instantly glanced out of the window, when to her astonishment she beheld a huge mass of fire descending towards the earth in a diagonal direction. This brilliant body had a solid nucleus that appeared to the eye about four feet square, and a strange, indescribable noise was caused by its flight through the air. Simultaneously, as it seemed to Mrs. Popham, she received a paralysing shock that affected her from head to foot, as if the entire contents of a highly-charged battery had been discharged into her body at once. The astonishing brilliancy of the meteor caused a temporary loss of sight, and it was fully half an hour before the lady could distinguish surrounding objects. When Mrs. Popham first beheld the falling mass she fancied that it was about to strike the house, and is still of the opinion that it must have passed alarmingly close. The lady took several hours to recover from the shock, and when Mr. Popham returned home several hours after he found her partially prostrated from its effects.

"Mr. McNaughton, a brother of Mrs. Popham, was sitting down stairs reading when the flash came. He jumped up, and, looking out of the window under the trees towards the river, he plainly saw the fiery ball strike the water at a little distance from the shore, causing a mountainous upheaval and sending splashes in every direction.

"Mr. Horace Baby also saw the glare caused by the flight of the meteor, although he did not actually see the body itself. He said that he felt a tremendous shock, and that he could feel the electricity oozing out of his finger-ends for some time after.

"Mr. C. P. Davidson, Q.C., was sitting down to lunch at the time, and describes the crash as being tremendous. The Rawlings family also felt the shock severely, as indeed did half the village. Mr. Popham's cottage stands about seventy feet from the water's edge at Stony Point, and it is thought that the aërolite fell into the stream about twenty or thirty yards from the shore, in about twenty feet of water. Owing to the high winds since the occurrence the water has been so muddy that it has been impossible to locate the whereabouts of the meteor. An attempt, however, will shortly be made to bring it to the surface."

I will send further details when they come to hand.

E. W. CLAYPOLE

New Bloomfield, Perry Co., Pennsylvania, July 15

Cold and Sunspots

YOUR correspondent, Mr. C. J. B. Williams, is wrong in the statement he makes in NATURE, vol. xxviii. p. 103, concerning the cold in California in the month of March. The month was the warmest March we have had for some years, the mean temperature being 3°·5 above the average, and 2°·8 above the average for the whole of the Pacific coast. February, on the contrary, was a very cold month, the mean temperature being 3°6 below the average. I believe it will be found that the mean temperature of a hemisphere is not affected by sunspots. That the seasons, however, are influenced by the state of the sun's surface I have no doubt, but this only in a secondary manner. In a paper read before the California Academy of Sciences in 1870 (see *Proceedings*, vol. iv. p. 128), I pointed out that our extreme seasonal climates were caused by the prevalence of broad belts of north and south winds which would extend continuously from east to west for 1500 or 2000 miles, and would blow over the same surface for months together, causing extreme seasons with temperatures above the average where the south current prevailed, and cold winters where there was a northerly current.

As a general rule when there is a cold winter on the Pacific coast the winter in the Eastern States is mild. The following figures taken from the U.S. Meteorological Reports will illustrate what I mean :—

Mean Temperature for February 1883

Below the Average	°	Above the Average	°
North Pacific States ...	−4·3	North Atlantic States	+2·2
Middle Pacific region	−4·3	Middle Atlantic States	+4·3
South Pacific region ...	−2·1	Florida	+6·3

Thus while on the whole of the Pacific coast the temperature of the whole was from 4°·3 to 2°·1 below the average, on the Atlantic coast the temperature was from 6°·3 to 2°·2 above the average.

Towards the end of February the north current that had been prevailing over the western regions of the continent during the whole of the winter shifted to the east, and this change of longitude was accompanied by some of the worst cyclones that have visited the central and middle States for years.

During the month of March, whilst we were under the *régime* of a south current, the temperature in the Eastern States was low, the temperature in Massachusetts for March being 7°·3 below the average.

My own belief is that the connection between the character of our seasons and sunspots will have to be worked out through the influence of the sun on the regional distribution of air currents.

San Francisco, Cal., July 3 JAMES BLAKE

Intelligence in Animals—Can a Viper Commit Suicide?

HAVING occasionally caught a viper, and kept it for a time in a glass case, one of the platelayers called me last Thursday and said "there was a fine 'Long Cripple' (a local name for a

serpent of any kind) lying on the bank a few yards down the line." I went to the place indicated, and there was a very large viper basking in the sun, but when I got near, it began to move away, and to prevent its escape I gently pressed a stick across it while I sent the man to fetch a glass jar to secure it in; but when it found its progress arrested, it began in a very spiteful manner to dart its nose forward, striking at the stick and stones and anything that was within its reach, but I could not see that it opened its mouth to make a real bite; but when it found with all its wriggling and twisting it was unable to free itself, it turned its head round upon itself, and about four inches from the head it opened its jaws and gave itself a bite, and when the fangs were well into the skin, it gave an extra squeeze, as if it intended to make sure that the operation should be thoroughly and effectively performed. It then deliberately withdrew its fangs, and in so doing it turned its head first one way and then the other, so as to withdraw one fang at a time.

Its head then went forward, and its body and tail became straight, and there lay the viper apparently lifeless, but I noticed a slight tremor in the skin and scales, which gradually passed from the head to the end of the tail. I took it up with my hand and placed it in the glass jar, and stood the jar in the window where the rays of the sun were hot, and in twenty five minutes the viper began to show signs of life, and in an hour it was as lively as if nothing had happened.

I should be glad to know whether it has come to the knowledge of any of the readers of NATURE that any human being or any animal has died from the bite of a viper. In my boyhood I have known sheep being bitten in the under jaw near the lip, and the animal's head has swollen very large, but invariably the sheep were well again when seen early on the following morning.

Some twenty years ago I saw a man who had been bitten in the hand by a viper, and his arm swelled and turned purple in places, and he was sick and faint for some hours, but he told me he was as well twenty-four hours after the bite as he was before.

R. LANGDON

Silverton Station, Cullumpton, Devon, July 28

A Cat and a Chicken

THE account I extract below was given in a local paper dated May 30 last:—

"*Strange Attachment.*—A curious instance of the above was brought to our knowledge by Mr. Hibbs, of the 'White House,' Swanage. A hen sitting on thirteen eggs hatched out twelve chickens on the 15th inst., but during her sitting four stray eggs had been laid in her nest, and as the eggs had not been marked these could not be removed. The hen with her little brood were not taken from the nest till two days later, when one of the stray eggs was found to be just bursting its shell. Mrs. Hibbs, in trying to assist the little stranger by removing the shell, somewhat injured it, and thinking it would die, and not liking to kill it herself, she thought that her cat (which happened to have a kitten a few days' old) would make short work of it. Strange to say the cat commenced to remove all the shell from the hatching chick, and then to shelter it with her kitten; since which she has carefully looked after it, and it is certainly a pleasing and unusual sight to see the little chick nestling between the forepaws of its foster mother with the kitten in close proximity. Mr. Hibbs tried to put the chicken with the rest of the brood, but the cat was so uneasy until the chicken was restored to her, that Mr. Hibbs has decided to let her have her own way, and bring them up together."

I kept the paper by me, intending, if I could verify the incident, to send the report of it to you. But under pressure of other writing it was not till a week ago that I addressed a letter to Mr. Hibbs. Last night I received from Mr. James Andrews of Swanage the following reply:—

"*Faircross, Wyke Regis, Weymouth, July* 24, 1883

"DEAR SIR,—David Hibbs of Swanage has forwarded me your letter of the 19th inst., asking me to reply to it. This he has done, I presume, as I had put his paragraph to the paper a little into 'shipshape' for him

"I am a resident at Swanage, and the bank manager there, and can vouch for the details of the 'Strange Attachment' just as recorded. I went round at Hibbs's request when the chicken was four days old. The old cat was lying down—the kitten asleep—and the little chick nestling with the cat, who would lift up her foreleg whenever the chick came near, to allow the chick to nestle under its arm, when it would close its arm around it in a most amusing and affectionate way, and seemed to be much more anxious about it than her own kitten. They began feeding the little chick at the first by sprinkling sop on the hair of the cat, which the chick would pick off. I do not know whether Hibbs has replied to you as well, as he did not say, but I hope the above will be sufficient.—JAMES ANDREWS."

It is to be noted that these aberrations from inherited habit—to which we have given the convenient name of instinct—occur almost invariably under the strong solvent of the maternal στοργή; but that they should occur at all points strongly towards the essential oneness and common origin of all life—however widely it may have deviated later along its ancestral lines of descent.

HENRY CECIL

Bregner, Bournemouth, July 25

Primæval Man and Working-Men Students

I RECEIVED a letter with great pleasure a fortnight ago from four new correspondents, who said they were working-men of Plaistow who had read my notes on Primæval Man in NATURE, had studied the Pitt-Rivers collection, and wished to show me their finds in Essex and have the North-East London position personally explained to them. Sunday having been mentioned as a convenient day, and this being approved by me, my correspondents (Messrs. W. H. Smith, Amos Herring, W. Swain, and Philip Thornhill) came here on Sunday morning, July 29. The stones brought were of great interest, mostly belonging to the Essex positions published by me. One example was a superb, rather large, wedge-shaped, pointed, slightly abraded, and ochreous implement found at Leyton; two were from Plaistow, a locality almost unrepresented in collections; one from West Ham, and other pieces from Wanstead. A somewhat small ovate specimen of great interest was found by one of my correspondents in the gravel excavated for the New Albert Dock, the extension of the Victoria Dock. The object of the greatest interest was a rude scraper-like tool made from a somewhat large piece of tabular flint, and found in gravel excavated between Loughton Railway Station and the "Robin Hood" Tavern, undoubtedly artificial and palæolithic; this ancient gravel is I think usually placed in the Glacial series; the find must be accepted as genuine. I may say here that on the 23rd of this month I found another implement and six flakes in gravel brought from Ware.

After my friends had looked over the collection here, listened to a few hints, and received a gift each of an implement from my own store in pleasant remembrance of the visit, we went to see some of the small excavations still open near Stoke Newington Common, in one of which the line of the "Palæolithic Floor" was distinctly visible, covered with about two feet of "trail and warp" and surmounted by humus. We then went into the Lea Valley, the meaning of the wide and deep excavation since palæolithic times being well understood by my visitors.

38, Kyverdale Road, N. WORTHINGTON G. SMITH

A Remarkable Form of Cloud

THE peculiar cloud formation observed by Mr. Hopkins and communicated to NATURE, vol. xxviii. p. 299, was also seen by me on Sunday, July 22, at 10.35 p.m. What I saw accords almost perfectly with the description given by Mr. Hopkins; but there was one rather important exception. Starting from a little above the horizon in the north-north-west I observed the position of another arch of cloud, clearly defined, strictly parallel to the principal arch, and ending somewhat abruptly about 20° from the zenith. The main streak was separated from it by about three times its width, and the intermediate space was quite clear. Both clouds appeared comparatively dense, and were situated at a moderate elevation. I did not notice any change in their appearance, nor did I see them break up.

It seems not improbable that currents of air from the north-north-west, passing through an otherwise tranquil but vapour-laden atmosphere of a much lower temperature than the surrounding air, may have originated these streaky bands of cloud by condensing the aqueous vapour suspended along their course into definite form.

ARTHUR EBBELS

Clapham, July 31

WITH reference to Mr. Hopkins's letter in NATURE last week (p. 299), I may say that I observed the bow-like band of

cloud, and noticed that it had what I may compare to a bowstring stretched from end to end. On Thursday, July 19, from 11 to 12 p.m., the whole sky was divided by such bands converging east and west. This was noticed by many persons in Essex, where I was staying. E. C. WALLIS

31, Meadow Road, S.W.

ON MOUNTING AND PHOTOGRAPHING MICROSCOPIC OBJECTS[1]

II.

THE prepared slide fixed in a clip should be placed on a hob or in a cool oven (not above 50° C.) for two days, by which time the excess of balsam round the edge of the cover will have become brittle, and can be removed with the point of a scalpel or penknife. Any balsam still remaining can be cleaned off with methylated spirit and a clean soft rag. The final cleaning of the slide may be done with soap and water. As the balsam itself serves to secure the cover to the slide, no cement or varnish is needed, and it remains only to label the object.

After successfully mounting this object, no difficulty will be experienced in applying the same methods to other small insects and parts of insects, such as antennæ, spiracles, feet, wings, ovipositors, corneas, tracheæ, &c. The two last cases, however, require careful dissection.

Animal hairs are best mounted in balsam, and the only special treatment they require is soaking for a short time in ether to remove grease.

The siliceous skeletons of diatoms and spiculæ of sponges and Holothuriæ require cleaning from extraneous matter by treatment with strong acids, but space will not allow a description of the details of their preparation.

The mounting of the organs and tissues of the higher animals and plants should not be attempted until tolerable facility has been acquired in the preparation of the simpler objects previously mentioned, as their structure is usually revealed only by the somewhat difficult process of cutting thin sections of them.

Most animal substances require hardening before they can be cut. Hardening may be thus effected. The perfectly fresh tissue is to be cut into pieces about the size of Spanish nuts, and soaked in ten times its bulk of a solution, consisting of one part of methylated spirit, and two parts of a ½ per cent. solution of chromic acid. At the end of twenty-four hours, and again after every third day, the solution is to be changed. After a week or fortnight the pieces should be well washed in methylated spirit, and will then be hard enough for cutting.

The next process is to embed the tissue in some substance firm enough to afford it support, yet soft enough to be readily cut with it. A good material for this purpose is a mixture of three or four parts of solid paraffin (paraffin candles), three of lard, and one of paraffin oil. It should be heated just sufficiently to keep it fluid, and the hardened tissue from which the excess of alcohol has been drained should be soaked in it for a quarter of an hour if of moderately close texture. If of very open texture—lung or testis, for instance—it must be soaked for about half an hour in rectified alcohol, and for a like period in absolute alcohol, to remove all traces of water. Then after displacing the alcohol by a quarter or half an hour's immersion in oil of turpentine, the tissue may be placed in the melted wax, which being readily miscible with the turpentine, will gain access to all the interstices of the substance.

A mould must then be prepared by gumming a piece of paper round a cork or cylinder of wood, the paper being allowed to project about three-quarters of an inch. Into this mould the substance is to be put, and the space filled up with some of the melted wax. When quite cold the paper may be stripped off, and the preparation will be ready for cutting with a razor, wetted with spirit to prevent adhesion of the sections.

The sections as they are cut are to be floated off the razor into methylated spirit, from which they may be transferred to a staining fluid.

The object of staining is in most cases not simply to impart a general colour to the object, but to take advantage of the fact that different parts are affected in different degrees by the same dye and are thereby clearly discriminated. Thus if an ammoniacal solution of carmine be employed, the structures which are first and most deeply stained are nuclei, axis cylinders of nerves, and ganglion corpuscles. To a less extent it stains the protoplasm of gland-cells and connective tissue corpuscles. But if the action be too long continued, the whole will be deeply and uniformly stained. and the selective power will be lost.

Carmine solution may be prepared by dissolving with the aid of gentle heat 2 grammes of carmine in 4 c.c. of ammonia and 48 c.c. of distilled water. Continue the heat or expose to the air until the smell of ammonia has almost disappeared, and then keep in a well-corked bottle. When required for use, a few drops of this solution should be added to a watch-glass full of water.

Logwood resembles carmine in its action and is by many preferred to it. It may be prepared as follows:—12 grammes of extract of logwood and 36 grammes of alum, both in fine powder, are to be mixed with 60 c.c. of distilled water, stirred well with a glass rod and filtered. Add to filtrate 5 c.c. rectified alcohol. Dilute with two or three times its volume of distilled water when used. When the tissue has been hardened with chromic acid, the sections should be soaked for a few minutes in a 1 per cent. solution of sodic bicarbonate to neutralise the acid before staining in logwood.

No general rule can be given for the length of time the section must remain in the staining fluid. It will vary from a few minutes to as many hours, and the section must be removed and examined with the microscope from time to time to see when the process has gone far enough.

When sufficiently stained, the excess of staining fluid is to be drained off and the section passed through rectified spirit 60 O.P., oil of cloves, and oil of turpentine, remaining about five minutes in each, and may then be mounted in balsam as already described.

For displaying tesselated epithelium in mesenteries, lungs, and blood-vessels, nothing can be more beautiful than staining by oxide of silver reduced from the nitrate. The perfectly fresh membrane or the section of hardened tissue as the case may be must be well washed with distilled water and then soaked for five minutes in a 5 per cent. solution of nitrate of silver. It is then again to be washed and exposed in distilled water to sunlight until it assumes a brown colour. The necessary exposure will vary from ten minutes to an hour or more. After a final wash in distilled water, it may be treated like objects stained by other methods. By this treatment the tissue assumes a general pale brown tint and the outline of every cell is sharply marked out by a deep brown deposit of argentic oxide in the intercellular substance.

Many vegetable tissues, such as cork, pith, succulent leaves, and some fruits, tubers, and roots, can be cut without previous preparation, and for such as are too soft to be cut in the fresh state the process of hardening is simpler than that employed for animal substances. Dehydration by simply soaking for a day or two in methylated spirit usually suffices.

Stems of plants usually require softening before cutting, and this softening can be effected if the wood is young by two or three days' immersion in methylated spirit to remove resinous matter, followed by maceration for from four days to a week in water. When the wood is old or unusually hard, the maceration must be prolonged or the

[1] Concluded from p. 303.

specimen may be boiled for a short time. Longitudinal sections may be cut by gluing the piece of stem to a cork to afford a hold upon it.

The preparation of sections of minerals and rocks is usually considered a very difficult matter, but much may be accomplished without the aid of the usual lapidaries' wheel for cutting and the revolving lap for grinding the sections, if the microscopist provides himself with a flat piece of lead six to ten inches square, and two pieces of boiler plate of the same size, planed on one side. A chip of the rock may be ground flat on the leaden lap, charged with coarse emery and water, and the process continued with emery of moderate grain on one of the iron plates, and the finest flour emery on the other. The flat side being then cemented with balsam (undiluted) to a piece of plate-glass about an inch square, the process of grinding may be repeated on the other side of the chip, until it becomes perfectly transparent. It may then be detached from the glass by soaking in benzole, and mounted in balsam in the usual way.

When the sections are to be mounted dry, which is very rarely the case, the fine scratches left by the flour emery must be removed by giving the section a final polish on a hard and flat oilstone reserved for the purpose and wetted with clean water only.

When it is desired to preserve the natural colour of objects, especially of such as contain chlorophyll, the necessary preliminary treatment with alcohol raises an objection to the balsam process, and another objection is that some tissues are rendered too transparent, and many of their finer features are obliterated by the highly refractive balsam.

In these cases the object must be mounted in some aqueous medium, the best and most convenient being a preparation of glycerine and gelatine, which forms a transparent jelly when cold, but is easily liquefied by heat. It is best to buy this "glycerine jelly," as it is troublesome to make on a small scale. When required for use it must be liquefied by standing it in a cup of hot water.

In general, objects to be mounted in glycerine jelly should not be embedded, but if any support is needed in cutting, this should take the form of two pieces of cork hollowed out to the shape of the object.

Water in the objects no longer presents any difficulty in this method of mounting, but air has still to be contended with, and the methods adopted for its elimination in the balsam process are no longer applicable. Some objects may be freed from air by boiling in water for a few minutes, but many would be spoilt by such treatment. Recourse must then be had to the air-pump, or, if this instrument is not accessible, to a very simple process depending on the great solubility of air in water.[1]

A wide-mouthed bottle of about four ounces capacity, with a closely fitting *solid* stopper, is completely filled with water, which at the time is, and for half an hour previously has been, boiling, in order to expel all traces of dissolved air. The stopper being then inserted without inclosing a single air-bubble, the bottle is set aside until cool enough to receive the sections, which are then to be put into it. A few drops of boiling water are then to be added to make good the inevitable loss in removing the stopper; the bottle is to be again closed, wiped dry, and securely sealed with melted paraffin. After twelve hours it may be opened, and the whole contents turned into a white porcelain shallow dish. The sections can then be easily seen, and picked out with a section-lifter, and should be soaked for half an hour in a 50 per cent. solution of glycerine before mounting.

The process of transferring the object to the slide, applying the liquefied jelly, and lowering the cover, are exactly the same as in the balsam method, and the slide should be set aside in a clip for a few hours for the jelly to solidify. In cold weather it is advisable to warm the brass table by means of a spirit lamp, or the jelly may viscify too quickly.

When quite cold and set, the excess of jelly may be cleaned from the edges of the cover glass, and the slide may then be ringed with asphalte while running in the turntable. Two or three subsequent coats of asphalte and the attachment of labels will complete the slide.

The objects for which glycerine jelly is most suitable are the lower forms of vegetable life—Algæ, Desmidiaceæ, Characeæ, Hepaticæ, Fungi, Lichens, Mosses, &c., and cuticles and sections of plants of all kinds. Many animal tissues are also better seen in it than in balsam.

It is but seldom that other preservative media are required, and it will be found that almost all objects may be suitably preserved by one of the three methods here described.

Closely related to the preparation of objects for microscopic examination is their delineation by photography, an art of the greatest value on account of its freedom from bias and personal equation, and as a means of lecture illustration with the aid of the lantern it must be appreciated by the numbers who have experienced the difficulty of demonstrating microscopic structure to many persons.

This application of photography, which is almost as old as the photographic art itself, extending back to the days of Daguerreotype, owes its recent development and simplification mainly to the introduction of gelatine plates, and the object of Mr. Malley's work[1] (which, however, should be called Photomicrography, for it does not treat of the production of microscopic photographs, as its name would imply) is to show how in an ordinary room, with an ordinary microscope, photographic camera, and paraffin lamp, photographs can be taken which will bear comparison with those obtained in the old days by the aid of sunlight reflected from expensive heliostats, electric arcs, magnesium and lime light, microscopes of special construction, and rooms specially set apart for the work. It therefore appeals to a large class of persons—those who would wish to practice the art, but lack either the sunlight hours or the expensive illuminators and apparatus formerly considered necessary.

The microscope, camera, and dark room, with their accessories, and the method of working with the Swan incandescent lamp and sunlight are described in detail, but the reader is perplexed by references to an illustration which cannot be found in the book. Four Woodburytype reproductions of photomicrographs of Aulacodiscus, Pleurosigma, and Surirella, scales of Lepisma, and Bacilli in human lung, accompany the work.

The instructions for taking negatives by the wet collodion and gelatino-bromide processes and the production of positives, enlargements, &c., are clear and concise, but we must enter an emphatic protest against the author's opinion that in object-glasses for photomicrography, depth of focus or penetration is to be sacrificed to angular aperture. Penetration and flatness of field are really of greater importance in lenses for photographic than for visual purposes, for in viewing an object under the microscope the observer has the power of focusing in rapid succession, and by imperceptible gradations, points at different depths and different distances from the centre of the field; but a photograph represents only such structures as were in focus at the time of exposure, and once taken, the focus is unalterable. It is therefore desirable to secure as great a depth of focus and as flat a field as possible—qualities which are incompatible with large apertures.

Mr. Malley very properly advises his readers not to walk about during the exposure of a plate, but the extension of the prohibition to speaking also is surely an unnecessary restraint.

[1] The writer cannot remember where he has seen this process described, but he can testify to its efficiency.

[1] "Micro-Photography," by A. Cowley Malley, B.A., M.B., &c. (Lewis, Gower Street.)

PROPOSED ZOOLOGICAL STATION AT GRANTON, NEAR EDINBURGH

AT the half-yearly meeting of the Scottish Meteorological Society held at Edinburgh on Thursday last week, Mr. John Murray, convener of the Society's Fisheries Committee, submitted the following Report:—

"The Fisheries Committee of the Council appointed in February last have had under their careful consideration the matters remitted to them by the Council, viz. the carrying out of investigations in accordance with the terms of the grant of 1500*l.* made to the Society by the Executive Committee of the International Fisheries Exhibition held in Edinburgh in 1882. The Committee recommends (1) to continue and extend the river observations and the observations made by the District Fishery officers through the Scottish Fishery Board, and to discuss all observations made to the end of the fishing season of 1883, which are yet undiscussed.

"2. To obtain the assistance of a few naturalists in making observations at several of the chief fishing centres and principal inland lakes. Prof. Herdman has consented to reside at Loch Fyne for a month, and to arrange for observations for a year. Mr. Hoyle is in like manner to go to Peterhead, and Mr. Beddard to Eyemouth. The Rev. Dr. Norman has during the present month been engaged in examining a large number of the Scottish lochs. Instructions have been drawn up for the guidance of these gentlemen, and a sum not exceeding 50*l.* has been placed at the disposal of each for the expenses immediately connected with the investigations. These observations are of a strictly tentative character, but will certainly lead to additions to knowledge, and are, moreover, necessary as a basis for further investigations.

"3. The Committee have had under consideration the recommendation of the Executive Committee of the Fisheries Exhibition as to the foundation of a zoological station. A number of the members of Committee have examined the capabilities of the old Granton Quarry, which has been for many years in direct communication with the sea, as a suitable position for a zoological station.

"The convener has drafted the following scheme, which in the opinion of the Committee would, if carried out, afford excellent facilities for biological researches and meteorological observations bearing upon these inquiries:—It is proposed to inclose the Granton quarry, which has an area at high water of about ten acres, and depths varying to sixty feet, so as to regulate the inflow and outflow of the tide in such a manner that while admitting abundance of sea water at each tide, fish and other animals will be prevented from escaping out of the inclosure. This will be done by means of stakes and wire with other kinds of netting. The quarry will then be stocked with all kinds of fish and marine invertebrates. When it is desired to separate fish or other animals for special study this will be done by floating or fixed wire and wood cages.

"A barge, about 64 feet by 27 feet, of great stability, will be moored in the inclosure; upon this will be built a house with laboratories, workrooms, and a library; it will also be furnished with a small windmill to pump up sea water into a tank on the roof. The water in this tank will be conveyed by pipes to the various tiled tables, glass jars, and aquaria of the establishment. A small cottage will be built on the shore for the accommodation of the keeper and engineer, with one or two spare rooms. A steam pinnace for dredging and making observations in the Firth of Forth and the North Sea will be attached to the station.

"A naturalist will be appointed whose duty will be to make continuous observations and experiments, assisted by the engineer and keeper. There will be ample accommodation for four other naturalists to work at the station and carry on investigations; and, so far as the accommodation will permit, British and foreign naturalists will be invited to make use of the station free of charge.

"Towards the carrying out of this scheme the Duke of Buccleuch has liberally granted a lease of the quarry at a nominal rent, with permission to erect a cottage on the shore; and Mr. Howkins, his Grace's local commissioner, has promised all the assistance in his power to further the undertaking. A gentleman who takes a warm interest in the progress of research in Scotland has offered 1000*l.* to construct the barge and fit it up with laboratories and workrooms. Mr. John Henderson (of Messrs. D. and W. Henderson, shipbuilders, Glasgow) has undertaken to provide the plans and specifications of the barge and laboratories gratuitously; Mr. J. Y. Buchanan has promised to fit up one of the rooms on the barge as a chemical laboratory suited to the requirements of the station; Mr. Thomas Stevenson, the Society's Honorary Secretary, has agreed to give his professional services in inclosing the quarry gratuitously; and Mr. John Anderson, of Denham Green, has undertaken to provide the station with a salmon and trout hatchery. The convener will furnish the laboratories with apparatus, and place his large zoological library at the service of workers. A number of gentlemen have promised to support the undertaking when once commenced; and the convener believes that within a few months he will be able to announce that the station has been presented with a steam pinnace and with funds for the erection of a cottage on the shore—the only desiderata to complete the scheme.

"In these circumstances the Committee, believing that this scheme deserves their hearty support, recommend, for the year ending November 1, 1884, a grant from the Fishery Fund not exceeding 300*l.*, and 250*l.* for each of the two subsequent years, towards the expenses of the station, on the conditions that the biological and meteorological observations and the investigations above referred to, relative to the Scottish fisheries, be carried on, and that a report on the work done be annually furnished to the Council of the Society."

The above grants were agreed to, and it was announced that the works at Granton would be commenced at once. It is expected that by the beginning of November the proper work of the station will be begun. Already, we understand, several distinguished naturalists have signified their intention to avail themselves of the altogether unique facilities which will be afforded by this zoological station for the successful prosecution of biological research. It is gratifying to observe the heartiness with which the funds required for carrying out this admirable scheme are being provided, and it cannot be doubted that the 800*l.* still required for the steam pinnace, the 500*l.* for the cottage, and the 200*l.* for inclosing the quarry will also be soon provided by some of our more generous patrons of science.

ELEVATION AND SUBSIDENCE; OR, THE PERMANENCE OF OCEANS AND CONTINENTS

IT has been observed, and with increasing frequency within the last few years, that wherever considerable weight is added on any part of the earth's surface, a corresponding subsidence of its crust almost invariably follows. It is generally admitted that nearly the whole of the sedimentary rocks, enormous as their known thickness is, were deposited in shallow water, and therefore in slowly subsiding areas. The Palæozoic rocks consist mainly of sandy and muddy sediment, with occasional intercalated zones of limestone. They everywhere bear witness to comparatively shallow water and the proximity of land. Their frequent alternations of sandstone, shale, conglomerate, and other detrital materials, their abundant

rippled and sun-cracked surfaces, marked often with burrows and trails of worms, as well as the prevalent character of their organic remains, show that they must have been deposited in areas of slow subsidence, bordering continental or insular masses of land.[1] Vast thicknesses of strata have been continuously deposited at or near the sea-level. The coal-measures present a series of alternating layers of vegetable matter and brackish water sediments, reaching in the South Wales district a thickness of upwards of 10,000 feet, whose accumulation must have been accompanied almost foot by foot by a corresponding subsidence. The Cambrian sediments accumulated in the British area to a thickness of 23,000 feet apparently without any great change in the depth of the sea in which they were formed; and throughout the deposition of the whole of the Silurians, subsidence seems to have kept pace with sedimentation. The Permian again furnishes many instances of sedimentation at or near the sea level sustained throughout great thicknesses, and of frequent alternation of marine and freshwater deposits. Among Mesozoic rocks, the New Red Sandstone furnishes an example of isolated basins of deposit to which the sea found repeated access, though a thickness in places of 3000 feet had accumulated in them. The German Triassic basin is for the greater part of its thickness a succession of terrestrial deposits containing plant-remains. The Jurassic and Cretaceous systems were deposited during interrupted depression of the sea bottom, while the Tertiaries abound with local instances in which subsidence has kept pace with sedimentation.

The washing of sea-coasts and removal of material shown by the discoloration of the sea for miles round the shore in stormy weather, shows that the process of accumulation of sediment still progresses on a very large scale.[2] It has been ascertained that nearly the whole of this must be redeposited within a distance of thirty miles. If the waves have no disturbing power at a greater depth than 40 feet, and could therefore neither deepen the sea-bed nor prevent its silting up to within that depth, our shores should be surrounded by enormous tracts of shoal water, whose bottom might be grooved or deepened by local currents, but whose average depth would not exceed 40 feet, or even less, since many rocks are so protected by seaweed that their further degradation when once below the reach of surf must be inappreciable. There is no cause therefore capable of generally deepening the sea round coasts beyond some such limit as this, except subsidence, and this can only be ascribed with any semblance of probability to the accumulating weight of sediment. The prevailing tendency on sea-margins is and must be towards depression, and there are few residents on the sea-coast who would be unable to contribute valuable observations on this point. It must however be remembered that while raised beaches are conspicuous objects, depressed beaches could obviously hardly ever attract attention, even if the shingle had not been removed by the surf, and further, most, if not all, of the existing raised beaches may have been formed during the general elevation of the land that took place at the close of the glacial period.[3] Other observers have recorded numerous submerged forests on the coasts of Cornwall, Devon, Somersetshire, and Wales. An elevation of the coast may, on the other hand, be sometimes accounted for when due consideration is given to the surrounding conditions.[1] It may, for instance, conceivably be produced on any shores where considerable sediments are forming at some distance out to sea or where masses of cliff are being washed away.

The extreme sensitiveness of the earth's crust to any changes in the distribution of weight on its surface is, however, best exemplified by those local depositions and removals of matter which have attracted more general attention at the present day. The chief of these is the transfer of matter by river action from large tracts, and its accumulation in such limited areas as plains, estuaries, and deltas. Borings of 400 and 500 feet have shown that these often consist of long successions of silts, which alternating layers of shells and of vegetable matter prove to have been deposited at or near the sea-level, and the Wealden and Eocene formations in the British area show that such accumulations may exceed 1000 feet in thickness. In the case of deltas, subsidence must keep pace almost foot by foot with the accumulation, and be confined to the area over which the sediment is being deposited, for any more rapid subsidence would check its growth and convert it into an estuary. This sinking is apparently of universal occurrence.

A similar instance of the transfer of weight from larger areas and its precipitation on a very circumscribed area[2] is seen in coral atolls and reefs. The explanation of their formation given by Darwin requires a gradual subsidence keeping pace with their growth, which takes place within twenty fathoms of the surface only. This theory, simple and admirable as it is, accounting satisfactorily for all the observed phenomena of coral growth, has been contested by Mr. Murray, who has shown that atolls might be merely incrustations of volcanic peaks. But his theory seems improbable by contrast, for it demands 290 volcanic peaks at the sea-level in the Pacific coral area alone, every foot of which has been completely concealed by coral growth, though few volcanic craters are known so near the sea-level outside this area. We seem thus to have in coral growths another evidence of subsidence keeping pace with the increase of weight, sometimes, as soundings prove, to a depth of 1000 feet or more. The replacement of a column of sea water 100 fathoms in depth, by a column of limestone, would increase the pressure per square fathom from $619\frac{1}{2}$ tons to 1487 tons, so that it is easy to realise how vast must be the increased pressure on such an area as that occupied by the great reef of Australia, 1250 miles long and 10 to 90 miles broad.

The sands, gravels, and clays, with marine shells and erratic boulders, prove that a great submergence took place during the Glacial Period, while Europe was under an ice sheet 6000 feet thick in Norway, and diminishing to 1500 in Central Germany. The extent of the submergence has been perhaps understated at 600 feet in Scandinavia, and was at least 1350 feet in Wales. A corresponding re-elevation accompanied the disappearance of the ice. It has often been supposed that the sinking of the west coast of Greenland is similarly due to its ice-cap.

It is probable that great outflows of lava may in like manner occasion subsidence—though it is by no means

[1] "Text-Book of Geology," Geikie. 1882, p. 647.

[2] M. Marchal has estimated that the sea deposits annually 600,000 cubic metres of sediments in the Bay of Mont St. Michel, and 10,000,000 on the coasts of Flanders, Zealand, and Norfolk.

[3] My own experience on the south coast is that the weight of evidence points to a general sinking, for vestiges of submerged land vegetation and traditions of submergence are very frequent. At Bournemouth I have seen heath plants and roots, fresh-looking except for the incipient formation of pyrites, cast up from a tract of moorland now below the sea-level. Poole Harbour would long since have been left by the sea if there were no subsidence, and a landing-stage with rings found below low-water mark furnish valuable data as to the amount that has taken place in historic times. The Solent must have been originally a harbour like that of Poole, continually silting, sinking, and enlarging, the depression travelling west and cutting one river after another from the sea until the western channel was at last opened. The immense accumulations of mud in its channel seem to have dragged the land into a sort of trough with raised sides, so that the Yar, Medina, and Brading Rivers flow inland instead of out to sea. On its margins we find here have been oscillations of level, caused perhaps by alterations in the position of the sediments through changing currents. The inroads of the sea at Pagham and Selsea show the downward movement to have extended along the whole of the Hampshire coast.

[1] An elevation, for instance, has taken place on the Kentish coast which has closed the Stour to navigation and caused the sea to retreat from Stourmouth, Richborough, and Sandwich, and which is also marked by the great exposures of Eocene along this part of the coast above low-water mark, and which could hardly exist where exposed to strong tidal and wave action, unless the abrading process were counteracted. The immense deposits, taking place at a distance from shore, brought down by the Thames, must lead to considerable subsidence in its estuary and consequently some corresponding elevation along its shores. The Thames sediment is of unknown depth, but on its margins at Sheerness the alluvial mud is 80 feet thick, and at Upchurch, opposite Queenborough, 75 feet.

[2] But $3\frac{1}{2}$ per cent. of solids preexisted in the water displaced by the rock.

so well ascertained a fact. I have, however, observed in Iceland that lava-streams very frequently terminate in or flow under lakes or gulfs of the sea, though water presents no obstacle to their continued progress. Lakes have been filled in solid by outpourings of lava, and had those I observed existed previous to the flows the lava must have entered them in a more abrupt manner, and it seems therefore likely that they are depressions caused by the weight of the lava. But there are also instances in which the actual depression produced by the weight of lava-streams can be seen. A great lava flow has at some period debouched from Skjaldbreith, and from two other nameless craters to the south-east, on to the historic plain of Thingvállir, forty square miles of which is water. At its northern end the lava is still in its original position upon the slopes, but the whole central mass in the plain has torn itself away from the sides and sunk a hundred feet, leaving vertical cliffs of solid lava of that height on its flanks.

Again, near Mývatn there is an immense tract of lava, the latest contribution to which, estimated by Mr. Lock at 31,000,000,000 cubic feet, welled out in 1875. This tract, known as the Öræfi, presents a somewhat analogous instanee, for the centre of the flow has also broken away from its flanks and sunk. Mývatn, a lake of some thirty miles in extent appears to have been formed by the weight of lavas which have poured on to the plain from nearly every side. Another recent stream in the same neighbourhood, whose source and age are unknown, follows the course of the Skjálfandafljót to the sea and terminates in a deep gulf. It appears that, as long as lava-flows occur in mountainous regions or in narrow valleys, any subsidence occasioned by the additional weight is difficult to detect, but as soon as it enters on to plains the subsidence is marked. It may also be that valleys in undulated or folded strata, being inverted arches, would resist pressure, while even in valleys of erosion much of the pressure must be exerted obliquely against the mountain masses instead of wholly vertically as on a flat surface. It has been suggested that the discharge of masses of lava at the surface would leave cavities in the interior and thus occasion subsidence, and it has even been anticipated that Iceland would bodily disappear from this cause, like the island of Friesland, from the maps; but there is no evidence of any such cavities having existed in old basaltic formations or in volcanic districts, and it is far more probable that the escaped matter is pressed out by other lava which immediately replaces it.

Dr. Fisher believes that all plains in proximity to mountain chains, upon which the material provided by their denudation is spread out, sink under the weight of the material and cause a compensating elevation of the neighbouring mountains. The sub-Himalayan range consists of subaërial deposits from 12,000 to 15,000 feet thick brought down by torrents, and which must have been deposited on a level and continuously sinking plain. "The conclusion seems irresistible that corresponding to the long, though occasionally interrupted, depression of these plains, a correlative elevation of the great range which has supplied the deposits has been going on."[1] If, as in the Himalayas, the region be one of approximate equilibrium, and much sediment is brought off the mountains and spread over the plains, the mountains become after a while too light and the plains too heavy,[2] and accordingly the mountains rise and the plains sink to restore the contour. This appears to be what has happened.

These comprise nearly all observable instances in which weight has been transported from elsewhere to areas where it did not previously exist, and are sufficient to prove that in such cases a subsidence more or less equalling in amount the vertical thickness of such added matter—except in the case of ice, which is of a much lower specific gravity—nearly invariably follows. Can it be reasonably maintained that these subsidences and the reelevations, which seem invariably to accompany the removal of weight, whether by melting of ice, as in the glacial period, or by denudation, are not the result of the increase or diminution in pressure? If the accumulation of sediment were due to the subsidence, instead of the subsidence to the accumulated sediment, as recently suggested by Dr. Geikie, it would be most improbable that they would so frequently bear such near proportion the one to the other. In none of these instances has the subsidence exceeded the accumulation, as must sometimes have been the case if the sediment merely accumulated because a subsidence quite independent of it happened to be in progress.

Such subsidences would only be possible with a substratum somewhere of viscous matter. Professors Shaler and Le Conte and Mr. Fisher, and many other very able geologists, have advocated the existence of a fluid or viscous layer between a solid interior of great density and a consolidated crust. If, Mr. Fisher maintains, it requires great pressure to solidify the materials at the temperature of the solid interior, a melting temperature may exist at some depth before the pressure is sufficient to solidify. Although Prof. Geikie and many other geologists do not admit the continuous existence of such a layer, it is difficult to see how they escape the conclusion. In his text-book[1] Prof. Geikie states that "from the rate of increment of temperature downwards it is obvious that at no great depth the rocks must be at the temperature of boiling water, and that further down, but still at a distance which relatively to the earth's radius is small, they must reach and exceed the temperatures at which they would fuse at the surface." Further on he explains that the crystalline rocks of the Highlands of Scotland and of the Green Mountains of New England are mechanical sediments metamorphosed chiefly where they are most highly contorted, or have been subjected to the greatest pressure. Strata of sedimentary origin which have accumulated to thousands of feet in thickness may be depressed deep beneath the surface and brought within the influence of metamorphosis,[2] and be eventually reduced to a soft and pasty condition, and protruded into some of the overlying less metamorphosed masses in the form of granite veins, or be erupted to the surface in the form of lava. This is an absolute admission that at some depth, relatively not great, pressure converts solid into viscous or fluid strata. He further states that "There can be no doubt that the lines of equal internal temperature (isogeothermal lines) for a considerable depth downward, follow approximately the contours of the surface, curving up and down as the surface rises into mountains, or sinks into plains;"[3] so that it seems difficult to understand why the particular line of temperature or of pressure at which most rocks melt, should not be continuous.[4] Like conditions must produce like results, and if the mere pressure of overlying strata can anywhere or at any depth render rocks molten or fluid, they will become molten or fluid wherever the required pressure occurs. A nucleus kept solid at a temperature higher than its melting-point, through excess of pressure, cannot pass into a crust whose solidity is due to lowness of temperature, through absence of pressure, without the existence of that intermediate stage of pressure or temperature requisite to produce a melted zone or layer. Prof. Geikie in fact himself admits "that the nucleus though practically solid, is at such a temperature and pressure that any diminution of the pressure by corrugation of the crust or otherwise, will cause the subjacent portion of the nucleus to melt."[5] But as the pressure diminishes gradually throughout the crust from the enormous amount on the solid nucleus to the merely atmospheric pressure

[1] "Physics of the Earth's Crust," p. 81. [2] *Ib.*, p. 83.

[1] *L.c.*, p. 289.
[2] Geikie, "Text-Book of Geology," p. 587. [3] *L.c.*, p. 287.
[4] Fisher maintains that mountain chains have solid roots, far exceeding their bulk above ground, projecting into the liquid layer. [5] *L.c.*, p. 265.

at the surface, how can the conclusion be avoided that there is everywhere a point in the earth's crust at which it must be just sufficient to keep rocks at the melting point. It seems utterly impossible, if it is once conceded that pressure does render rocks fluid, to avoid the conclusion that there everywhere exists a viscous substratum following to some extent the contour of the earth's surface.

Such a condition is precisely that which will alone explain the undoubted fact that the addition or removal of even comparatively small weights produces corresponding changes in the previous level of the earth's surface. "The deposition of 1000 feet of rock will, of course, cause a corresponding rise in the isogeotherms,"[1] that is, of the liquid layer, and "denudation of the land must lead to a depression of the isogeotherms, and a consequent cooling of the upper layers of the crust." Dr. Fisher[2] proves mathematically, in fact, that a liquid layer and no other condition can explain the movements that have taken place in the earth's crust, and satisfactorily account for volcanic action.

It appears in the present state of knowledge almost impossible to estimate the depth at which a viscous layer could exist. The estimates that have been made vary from 1000 miles to only 50,000 feet. On the one hand, however, if it is a fact, as Dr. Geikie surmises, that sedimentary formations of Silurian age have been fused and rendered viscous mainly by the mere superincumbent pressure of more recent sedimentary formations, the depth at which a viscous layer can exist must be less than the lowest estimate yet formed. On the other, the observed increase of temperature, not exceeding at most 1° F. for every 50 feet of depth, the melting temperature of rock, 2000° to 3000° F., would not be reached at a less depth than 100,000 feet. This is obviously too great a depth to account for some of the observed facts of geology, and is without any allowance for the increasing density of rocks at great depths, or for the many unknown agencies which may contribute at such depths to lower their melting temperature. The inert weight of 25,000 feet of rock of the density of slate, the thickness which according to Dr. Geikie has reduced rock to a viscous state, is about 2000 tons to the foot. I am not aware that any estimate has been made of the actual amount of heat that would be produced under such conditions. If, as must be the case, any relatively small increase of pressure produces a displacement in the molten layer, a compensating elevation must take place elsewhere, and if its effects are so considerable when the weights are relatively small, the results of pressure applied to oceanic basins must be infinitely great. The theory that oceanic basins have been permanent has been embraced by many of the ablest geologists, and since sediment has been forming in them uninterruptedly, at however slow a rate, since Eozoic times, its aggregate vertical thickness by now must be colossal. The pressure of water alone upon the rocks forming the bed of the greatest depths of the ocean (say 4000 fathoms) would equal 6195½ tons upon a square yard, and this pressure exists on a bottom which is at or near the freezing-point. The effect would be as if on land the pressure of the first 7000 or 8000 feet of rock generated no heat whatever, or rather as if the heat were intercepted by an icy layer, which also might conduct it away, with the result that the molten layer would rest under a greater weight under the ocean, where the rocks have been observed to be denser, than it does under the land. This extra weight, even if small, would tend to render the greatest depths of the ocean permanent, but lines of current, where sedimentation was less rapid, would present lines of relatively less resistance, which, becoming more and more elevated, would in time form submarine ridges or banks or dry land, until the extreme tension might possibly become relieved by the eruption of volcanic matter. The lines of absolutely least resistance would, however, most frequently perhaps coincide with sea-margins, because these would often be the nearest lines free from the pressure of accumulating sediment. While, therefore, actual shore-lines may be depressed by local sedimentation, there may be inland a far more important tendency to elevation. The recent mountain chains, whether volcanic or otherwise, follow at a distance the contours of coasts, and it is likely that such apparent exceptions as the Alps, Urals, and Himalayas were in proximity to coast-lines at the time of their formation.

To this extent, I believe, the permanence of ocean basins can be maintained, but the past and present distribution of both plants and pulmonate mollusca, which alone in terrestrial life seem to have any antiquity as species, appear to be wholly against any further extension of it.

That such considerations, theoretical as they seem, may have a practical value to geologists, a recent journey to Iceland abundantly proved. There is not only there, but in the Faroes, evidence of a period of quiescence between two great basaltic formations, during which plants grew and lignite was formed. It even appears that this quiescent period extended synchronously from Ireland to Greenland. During this time the Lower Eocene flora, splendidly represented at Reading, seems to have migrated through increasing temperature as far north as Greenland, for the Reading plants are almost wholly those which were thought to be characteristic of northern Tertiary floras and distinctive of Miocene time. A connection between Europe and America in these high latitudes has also been inferred on many grounds to have existed at about the same period. Does it not seem as if the elevation of land (which permitted these floras thus to migrate, and which probably raised the Eocene temperature by excluding the Arctic Ocean from the Atlantic) caused these stupendous eruptions of basalt to cease—for elevation on such a scale must mean relief from tension—and that its submergence during the Miocene led to, or was caused by, the renewal of the basaltic flow? The horizon in Iceland is marked by only very scattered sedimentary tuffs and lignites, and is far less marked than in the Faroes, but in the region of Akreyri it can be traced even from a distance by the highly-laminated beds of light-coloured trachyte, which seem to have ushered in the new volcanic activity. It would be impossible on internal evidence to assign most of them to any definite age, and it is only perhaps on broad considerations such as these that their geological position may hereafter be fixed, though their immense antiquity may be inferred by the denudation which has furrowed, since their deposition, nearly the whole surface of the island into deep troughs and high ridges, out of what were formerly continuous tabular sheets of basalt.

It would be interesting to ascertain whether the great basaltic outpours of Oregon and the Deccan preceded or accompanied any marked changes of level in adjoining areas.

The question has been asked as an objection to this theory, What possible mechanical properties can we attribute to the upper strata of the earth which will permit them to sustain the whole of the Himalayan plateau and North and South America above the sea-level, and yet will cause a continuous subsidence in an estuary in which sediment is being deposited? Such subsidence, it is maintained, could only occur with a substratum somewhere of viscous matter, and if such viscous matter exists, why does it not flow under the stresses due to the weight of continents and mountains?

It is difficult to meet this objection except by appealing to the facts. It is apparent that continents, and especially mountain masses, have been upheaved from below

[1] Geikie, *l.c.*, p. 287, 1° F. for every 50 feet.
[2] "Physics of the Earth's Crust."

through a pressure which the earth's crust is not rigid enough to resist, and that so long as this pressure is sustained they must remain at least stationary. There is no proof anywhere that the pressure that caused the elevation is now removed, but there are frequently indications, such as earthquakes and landslips in mountain chains, that it exists and is even on the increase. On the other hand, there is no evidence of any kind to show that some, especially of the older mountain chains, are not sinking, though subsidence in such cases would be very difficult to detect. Besides this it is conceivable that when the force which has squeezed the crust into folds has ceased to be exerted it is not flexible enough to regain its original horizontal position, but will remain in folds, and as there is no increased thickness, and consequently no addition of weight, but on the contrary a continual loss from denudation, there is no reason why they should not retain their position upon the hypothesis of a continuous molten layer subjected to greatest pressure at its lowest levels. Dr. Fisher even assumes that the mere removal of weight from them by denudation, and its accumulation on their flanks, would suffice to cause a continuous upheaval. The deflection of the plumbline has shown that the density of the crust beneath mountains must be less than that below the plains, and the relatively slow rate at which heat increases in boring through them shows also that the pressure there cannot be so great. Though strata are compressed into a smaller area through the folding, it is doubtful whether the aggregrate pressure on the liquid layer in such regions is at all increased, while in elevated plains it obviously cannot be so, as there is in that case no direct increase of weight. It thus seems as if it were as necessary that the crust of the earth should yield to increasing pressure as that the sea should roughen under the wind, and the apparently arbitrary upheavals and depressions are brought under a definite law. The greatest depths of the ocean would ever deepen and its superficial area tend to diminish, while that of the dry land would increase, and its mountain chains reach higher elevations. The theory appears in harmony with the truths of geology and of astronomy, for the records of Palæozoic times show neither evidence of great depth of sea nor mountainous elevation on land, the organic remains pointing to a little varied surface. The highest mountains are geologically the most recent, and evidence of deep seas increase towards the Cretaceous period, while our satellite, whose evolution may have progressed more quickly than ours, has relatively far greater, more numerous, and more abrupt elevations than the earth.

Somewhat similar conclusions to these have been arrived at in the "Physics of the Earth's Crust," by Dr. Fisher. Without presuming to compare the present superficial treatment of the subject with that great and philosophical work, some important differences will be observed between the views there expressed and these, as well as some entirely new observations and extensions of the theory. The views advocated are still so far from being generally accepted by geologists that their publication in NATURE will doubtless put many in possession of facts and inferences which are in a general way only accessible to those who have leisure to gather them from less popular publications. J. STARKIE GARDNER

THE ISCHIA EARTHQUAKE

ONE of the most disastrous earthquakes on record occurred in the little Island of Ischia, in the Bay of Naples, on the evening of July 28. It was only in March 1881 that a similar catastrophe occurred at the same place. The island is a favourite summer resort of Romans and Neapolitans, and Casamicciola, where the destruction was greatest, was crowded with strangers. The full extent of the loss of life has not yet been ascertained; but up to the present it is estimated that at least 4000 have been killed, and very large numbers wounded. The earthquake occurred at half-past nine, when strangers and natives were enjoying themselves in various ways under a cloudless sky with not a breath of air stirring. Not the slightest warning seems to have preceded what occurred; in the space of fifteen seconds Casamicciola was a heap of ruins, while a similar fate overtook the smaller towns of Forio, Laco Armino, and Fontana Serrata. At present we can only record the facts of the case; when further details are to hand it may be possible to throw some light on the real cause of the catastrophe. Besides the first shock, which lasted fifteen seconds, other two were noticed immediately after. Prof. Palmieri is stated to have expressed the opinion that the catastrophe was caused by a sinking in of the level, and not by an earthquake. On the 31st there was another slight shock; while Vesuvius is in a state of active eruption. A Rhenish journal states that on Saturday night, about the time when the Ischia earthquake occurred, a tremendous motion of the earth was distinctly felt at Wiesbaden. On the morning of the 31st also, it may be noted here, a shock of earthquake was felt in Oporto, lasting two seconds, with direction east and west; it naturally caused great consternation. Two shocks are reported to have occurred on the same day at Gilroy, California. With regard to the volcanic Monte Epomeo in the Island of Ischia, we may say that its last recorded eruption took place in 1302.

We are glad to learn that Dr. Dohrn, director of the Naples Zoological Station, who was in Ischia at the time, escaped unhurt.

THE AGRAM EARTHQUAKE[1]

IN connection with the Ischia Earthquake, the official report of the Agram Earthquake of three years ago may not be without interest. The detailed report by Herr Hantken von Prudnik contains all the information which he had been able to collect regarding the severe earthquake with which the district surrounding the town of Agram in Croatia was visited on November 9, 1880. Herr von Prudnik gives not only an exhaustive narrative of his own observations of the effects of the earthquake, made a few days after its occurrence, but also some account of careful observations made by inhabitants of the district where the earthquake actually took place; and his memoir is full of most interesting matter to seismologists. The district is situated in an area within which earthquakes are of very frequent occurrence, for Herr von Prudnik gives a long list with descriptive notes and dates, beginning with March 26, 1502, and coming down to Nov. 9, 1880, but most of them within the present century, of earthquake-shocks, some of which seem to have been severe, which have been felt in the locality. A few self-registering seismographs erected in suitable places in the district would yield, we think, much valuable information and would detect many of the smaller motions, partaking rather of the nature of tremors, which are no doubt frequent, but which, although of great seismological importance, remain unnoticed where such appliances are not in use.

With regard to the earthquake of November 9 itself, the shock seems to have been very severe, causing as it did, besides loss of life, a vast amount of damage to public and private buildings, especially churches. The details of the damage done, given by Herr von Prudnik, are very interesting, and illustrate very clearly the conclusions which have already been arrived at by seismologists as to the effects of the conformation of the ground in the neighbourhood of a building, and of the structure of the building itself, in diminishing or in aggravating the

[1] "Das Erdbeben von Agram in Jahre 1880." Bericht an das k. ung. Ministerium für Ackerbau, Industrie, und Handel, eingereicht von Max Hantken von Prudnik, gewesenem Director der k. ung. geologischen Anstalt. (London: Trübner and Co., 1882.)

destructive action of an earthquake. For example, we find that in some cases well-built and substantial churches and houses suffered severely, while crazy erections, considered to be almost on the point of falling to pieces, received little or no disturbance. This apparent paradox is of course explained by the fact that the sudden backward and forward motions of the ground on which a building stands, although they may be, and in general are, of limited extent, bring very severe stresses to bear on high masses of masonry, which although it may be of the very best construction has little strength to resist the strains produced; while more loosely put together, and, in ordinary circumstances, insecure structures are capable of yielding to the necessary extent and escape unharmed. Again, when an earthquake consists of approximately periodic movements of the ground, buildings or parts of buildings, whose natural period of free oscillation coincides with, or is some multiple of the period of the disturbance, yielding to the repeated and conspiring impulses, oscillate with increasing range, until return to the equilibrium position is no longer possible and they collapse in ruins.

A phenomenon observed in connection with many other earthquakes, the rotation of upright pillars such as gravestones and monuments, on their bases, was very remarkable in this. Herr von Prudnik does not accept the explanation which has been offered by Mallet and others that the rotation is due to vorticose movements of the earth's surface; and he offers an explanation which, though not quite clearly put dynamically, seems to point to the true theory. The cause of the phenomenon no doubt is that the first sufficiently severe shock causes the body to tilt over in the direction from which the shock proceeded, and immediately after, the shock, although rectilinear in direction, makes the body turn round on the corner or portion of an edge on which it for the moment rests. This explanation has been tested with model gravestones and obelisks placed on a table, which could be shaken so as to imitate the motions of the ground during an earthquake, and found to answer perfectly.[1] The circumstance that in the earthquake at Agram, as elsewhere, the gravestones at one particular place were for the most part rotated in one direction accords well with this explanation, as no doubt the gravestones there were all set so as to face in one direction.

Herr von Prudnik is not of opinion that the earthquake was due to volcanic agency, but thinks that it was produced by the yielding to mutual stresses of the materials underlying the Slamen mountain, which lies along the middle of the area in which the destructive effects were most marked. This mountain occupies an area roughly elliptical in shape, about 4·5 kilometres (6 Meilen) long by 3 kilometres broad, and is composed for the most part of slate, limestone, and dolomite surrounded with strata consisting mainly of marl. To this mountain all the effects point as the locality in which the earthquake originated; but here again we think the use of self-registering seismographs would be of great service in giving definite information. This would also give most valuable information as to the velocities of propagation of earthquake motions in strata of different materials. In the present case the disturbance travelled from Agram to Vienna in twelve seconds, which gives a velocity of propagation of 2·2 kilometres per second. It is not stated, however, how the exact times were observed.

Among the details of the many interesting phenomena, we find a very careful account of an outbreak of "mud volcanoes" at Reznik, a place about 8 kilometres west-south-west of Agram; but for details as to this and many other important points, we can only refer our readers who are interested in seismology to the memoir, which will well repay perusal.

[1] *Vide* Milne and Gray on "Earthquake Observations and Experiments," *Phil. Mag.*, November 1881.

NOTES

We are enabled to give the text of the telegram received in Stockholm this week from the Swedish circumpolar observation party, which has wintered at Spitzbergen. The news is the first received from the expedition since October last:—"Cape Thordsen, July 4th, 1883. This message will be forwarded to-morrow to Capt. Startschin with the boat fetching our first mail this year. The wintering of the expedition has in every respect been attended with success, particularly as the scientific researches have throughout been carried on exactly in accordance with the regulations formulated by the International Polar Commission. Hydrographical and magnetic studies have also been pursued on the ice in the Ice Fjord, as well as parallax measurements of clouds, and observations as to the temperature of the air, the snow, and the earth. The winter has on the whole been mild; the greatest cold occurring on January 2, when the thermometer registered 35·5° C. below freezing point. Storms have been few. Since September last the following buildings have been erected:—A hut on a mountain at an elevation of 270 metres, containing the anemometer and the wind-fan, which were read by a self-registering electrical apparatus; two astronomical observatories; another magnetic hut; a bath-house, a forge, and a wood storehouse. The dwelling house and working room have also been enlarged. The following game was shot during the winter: 61 ptarmigans, 9 reindeer, 18 wild geese, 20 foxes, and some wild fowl. With continuous labour, plenty of food and drink, and frequent baths, the members of the expedition have throughout enjoyed excellent health. Descriptions of the nature, our labour and life here during the wintering will follow."

At the meeting of the Scottish Meteorological Society held on Thursday last week it was announced that upwards of 4500*l.* had been already subscribed to establish the Meteorological Observatory on the top of Ben Nevis. The subscriptions vary in amount from 200*l.* to one penny, and the subscribers include Her Majesty the Queen and all classes of her subjects, and town councils and other corporate bodies in all parts of the United Kingdom. The road to the top of Ben Nevis is nearly half finished. The building will be commenced early this month, and it is contemplated that the portion to be completed this season will be ready at the end of October for the three observers, who will begin their regular observations on November 1.

Mr. Mundella in presenting his educational budget the other night had nothing but essential progress to report. The cry of overworking the children was introduced by some of the speakers, but Sir John Lubbock pointed out that monotony and not overwork was the real weakness of the present system, and that the tendency was to cultivate the memory at the expense of the observing faculty. The real remedy, as he pointed out, is to introduce greater variety into the elementary course, and above all to make practical science teaching an essential part of the curriculum.

From a statement issued with reference to the Rolleston Memorial we learn that the total sum subscribed is 1183*l.* 5*s.* 0*d.*, to which is added 59*l.* 7*s.* 5*d.*, dividends paid on sums invested from time to time in Consols before the list was closed. From this total have been deducted secretaries' expenses, charges for printing, advertising, &c., 36*l.* 16*s.* 9*d.*, leaving a capital sum of 1205*l.* 15*s.* 8*d.* invested in 1200*l.* Three per Cent. Consols. This sum has now been transferred to the chancellor, masters, and scholars of the University of Oxford, and accepted by them as the Rolleston Memorial Fund. The fund, it has been decided, will be expended in the institution of a prize to be awarded every two years for original research in any subject comprised under the following heads:—Animal and Vegetable Morphology, Physiology and Pathology, and Anthropology, to be selected by

the candidates themselves. The period during which this prize may be obtained by a candidate is limited to ten years after the date of matriculation; and with a view to render the prize as widely associated with Prof. Rolleston's name as possible, it is open to the members of the Universities of Oxford and Cambridge.

A CORRESPONDENT writes to us that he has received from a resident at Zagazig, in Egypt, a curious fact concerning cholera, which, if not noticed before, may be of interest. The resident stated that the town of Zagazig was perfectly healthy, and that the swallows and sparrows were flying about as usual, and so long as they remained he considered they were quite secure from any attack, but when they left he would not be long before he followed them. He remarked further that the birds had been observed by old hands to depart before the approach of cholera during the last four epidemics. Our correspondent asks what can be the cause of this, and we shall be glad if any of our readers can answer the question.

A CORRESPONDENT makes the following statement:—"Kentish men who drink chalk water are large boned, whilst those people who drink soft water are the reverse. At Glasgow, where the water is supposed to be very soft, there are said to be more bandy-legged children than at any other place." Is this so?

M. PASTEUR has written to the *Voltaire* a letter justifying the step taken by him in advising the Government to send a mission to Egypt in order to study the generation of cholera. He believes that this plague is produced by some description of microzyme; but he admits that this minute organism has not been discovered yet.

M. BARTHÉLEMY ST. HILAIRE has just finished the printing of his translation of the "Natural History of Animals," by Aristotle, which will be published in a very few days; it consists of four large octavo volumes.

THE managing committee of the Vienna International Electric Exhibition, which recently announced that, in consequence of the delay in the arrival of exhibits, the opening of the Exhibition, originally arranged for the 1st inst., would have to be postponed, has now fixed the ceremony for the 16th.

THE International Medical Congress of the present year will open at Amsterdam on September 4, and will be attended by a number of the most distinguished physicians and medical men of Great Britain, France, Belgium, and Germany. Amongst the British physicians papers or addresses have been promised by Sir Joseph Fayrer, M.D., and Dr. J. Ewart, on the treatment of imported and tropical diseases in countries belonging to the temperate zone; Dr. F. de Chaumont, of Netley Hospital, on the best measures of quarantine; Dr. E. Waring, of London, on the remedies used by the natives of tropical countries against the most dangerous epidemics; Dr. J. B. Scriven, on quinine injections and malaria fevers; Dr. Norman Chevers, late Professor at Calcutta, on tropical epidemics and the influence of tropical climates upon them; and Dr. Dyce Duckworth, of London, on the education of physicians for the Colonies.

THE fifty-first meeting of the British Medical Association began on Tuesday at Liverpool with the address of the President (Dr. A. T. H. Waters of Liverpool). On Wednesday the Council met to consider invitations for 1884 and to nominate a President Elect.

THE Gardens of the Zoological Society of Philadelphia in Fairmount Park are, we believe, the most nearly complete and best organised Zoological Gardens on the American Continent. Their eleventh annual report, now before us, shows a considerable amount of progress since their last anniversary. The number of visitors to the Gardens in the twelve months ending on the last day of February 1883 was 252,866, being nearly 10,000 more than in the preceding corresponding months. The income of the Society during the same period was rather over $50,000, while the expenditure seems to have been some $8000 less. During the same twelve months 423 living specimens were added to the collection, the total number of animals in the gardens at the date of the report being estimated at 687, of which 306 were Mammals, 338 Birds, and 43 Reptiles and Batrachians. These figures, no doubt, cannot rival those of the Zoological Society of London. But it must be recollected that our Society has been founded upwards of fifty years, is supported by some 3300 members, and has a population of 4,000,000 to draw upon for its visitors, not to count the strangers who are perpetually seeing the "sights of London." Among the special additions to the menagerie to which attention is invited in the report is an example of the Coast Fox (*Vulpes littoralis*) received from Yucatan, and stated to be probably the first to be exhibited in a living state. This rare fox has, we believe, never been obtained by the Zoological Society of London, and we rather doubt whether there is any example of it in the British Museum.

THE Committee of the Sunday Society have resolved to petition the Prince of Wales to use his influence as President to have the Fisheries Exhibition open to the public on a few Sundays before the final close of the collection.

AMONG a number of very munificent bequests that have been left to Paisley by the late Mr. Brough, we, says the *British Medical Journal*, observe that he has directed that 300*l*. is to be spent annually in establishing and maintaining a science lectureship in that town, with all the necessary adjuncts and accessories. The subjects to be taught are left to the trustees to fix, but the testator himself recommends that one of them should be physiology.

A VIOLENT shock of earthquake was felt at Catanzaro, in Calabria, on the morning of July 25.

WITH reference to the volcanic eruption on Krakatan Island off the coast of Java, brief reports of which were received by telegraph, and then noticed in NATURE, the following particulars have since been received. During Sunday, May 20, and Monday, May 21, the eruption was very heavily felt at Batavia, also more or less on Tuesday, May 22; but the earthquake shocks have since ceased, although the mountain is still apparently vomiting fire and smoke. The following report is from Anjer, dated May 23, 3.47 a.m.:—On Sunday morning last, from six to ten o'clock, there was a tremendous eruption, with continuous earthquakes and heavy rain of ashes. On Sunday evening and Monday morning it was continued. The eruption was distinctly seen here till nine o'clock this morning, and smoke was seen until twelve o'clock; afterwards it cleared up a little, and at this moment the air is clouded again. Capt. Ross reports from Anjer that on May 22 he was sailing near Java's first point and tried to get Prinsen Island in sight, but found that it was surrounded by clouds. Then he steered for Krakatan, but found it to be the same there. The captain observed that the lower island or mountain situated on the north side of Krakatan was totally surrounded by smoke, and from time to time flames arose with loud reports. Fire had broken out in several places, and it is very likely that the trees in the neighbourhood have caught fire. The mountain of Krakatan has been covered all over on the north side with ashes. The captain could not make out the condition of the mountain, as he kept away as far as possible, being afraid of the wind falling, and the vessel being drifted on to the island. The strongest fire was seen on the evening of May 22, with heavy explosions and detonations. The fire was also seen at that time at Anjer, but on account of the heavy smoke nothing could be perceived, as

all the islands remained clouded. The captain did not experience any shower of ashes. The master of the steamer *Conrad*, which arrived at Batavia on May 24, reports having passed Krakatan on the north side the previous night, and met with heavy rains of ashes, covering the decks, &c., with about 1½ inch of ashes. He also had to cut his way through nearly 1½ metres of pumice-stone, which occasioned a delay of almost five hours.

WE have already referred in NATURE to the excellent scientific work being done by the French in the Indo-Chinese peninsula, as evinced by the large number of scientific missions which have been despatched from France to those regions. As a farther example of the pains taken in France to obtain a thorough knowledge of the country in which she seems destined to play so large a part, we may refer to a periodical published by the Government of Saigon, entitled *Cochin-chine Française: Excursions et Reconnaissances.* The fifteenth part is now before us, and as each part contains about two hundred pages the amount of information accumulated in these volumes is considerable. Speaking broadly, and slightly altering a well-known Latin maxim, it may be said that nothing relating to the vast territory between the mouths of the Brahmaputra and the Canton river, between the Bay of Bengal and the China Sea, is outside the scope of this journal. As a rule the papers are of a highly scholarly and scientific kind. Thus the last number contains the second part of a long and richly illustrated paper on the coins and medals of Annam and French Cochin China, by M. Silvestre, inspector of native affairs in Saigon; a short history of the Portuguese in Cambodia; an account of the typhoon of last November at Hué, the capital of Annam, with barometrical tables, by the surgeon to the French Legation there; a long paper on the vegetation and forest administration of British Burmah; and finally one of a series of very interesting papers on the customs and popular superstitions of the Annamites. The present instalment deals with marriage customs. The efforts of the Colonial Government to sustain and encourage the study of Indo-China does not, however, close with the publication of this excellent journal, for we observe the advertisements of a large number of works relating to that country in the magazine under review. Among these are a weekly journal for the natives, an annual summary of facts relating to Cochin China, various maps, medical reports, &c. Whatever may be thought from other points of view of the action of France in Annam and Tonkin, there can be no doubt that the increase of French power there carries with it a large increase to knowledge, for the Colonial Government of France appears to know how to organise and stimulate research in the countries over which it exercises rule.

THE telegraph has made another step in advance in China. It has had the honour of being mentioned in a memorial to the throne. Li Hung Chang recently mentioned in a report to the Emperor that he received certain information by telegraph. And, more wonderful still, that mysterious and awe-inspiring document, an Imperial decree, written with the vermilion pencil, has actually been despatched by telegraph, for the Viceroy of Canton reports recently in a memorial that a decree had been conveyed to him in this way.

THE German system of *privat docenten*, or University teaching by outsiders, is to be tried in France. A decree provides that any doctor of letters or sciences, or correspondent or member of the Institute, may apply to the Minister of Education for permission to lecture on his respective subject, the license being renewable annually. The lectures may be public or private, at the professor's option, and the expense falls on him, while he can charge the students what he pleases. The same system is applied to the medical school.

WE have received the *Transactions of the Norfolk and Norwich Naturalists' Society* for 1882–83. In the first paper, on the scenery of Norfolk, Mr. Horace Woodward gives a history of the geological strata of the county, shows how the scenery was influenced by the action of water and the introduction of various forms of life, and how affected by the artificial changes brought about by man. There is also an interesting paper by Mr. Stevenson on the dusky petrel, and a paper by Mr. Southwell on the bottle-nosed whale and the history of the seal fishery. Mr. Clement Reed's paper on the discovery of Lithoglyphus in the Weybourn Crag is very interesting, from the fact that this freshwater shell is found now in Europe only in the Danube. Mr. Young gives his observations on the habits of the bearded tit, which birds he had kept in confinement for twelve years. Mr. Bidwell's list of British birds in whose nest the egg of the cuckoo has been found is the most complete yet published. The President contributes part x. of the fauna and flora of Norfolk, a list of the marine algæ.

THE exhibition of the Society of Agriculture and Insectology of Paris has just come to an end with a ministerial visit and distribution of prizes at the Palais de l'Industrie. Thousands of visitors have flocked to this hall in order to visit the interesting collection. A special building will be erected for the Society in the Park de Montsouris, and a sum of 32,000 francs has been already voted for this purpose by the city of Paris. A menagerie of living insects is to be established.

ACCORDING to the Austrian *Monatschrift für den Orient* the production of tin in the protected state of Perak, in the Malay Peninsula, for the year 1882 was 7000 tons, about equivalent to that of Cornwall. Forty thousand Chinese are employed in the Malacca tin mines.

THE additions to the Zoological Society's Gardens during the past week include a Macaque Monkey (*Macacus cynomolgus* ♂) from India, presented by Mr. J. W. Lucking; a White-throated Capuchin (*Cebus hypoleucus* ♂) from Central America, presented by Mr. F. Hoey; a Leopard (*Felis pardus*) from Somali Land, East Africa, presented by Mr. Frederick Holmwood; four Babiroussas (*Babirussa alfurus* ♂ ♂ ♀ ♀) from Celebes, presented by Dr. F. H. Bauer, C.M.Z.S.; a Two-spotted Paradoxure (*Nandinia binotata* ♂), a Royal Python (*Python regius*) from West Africa, presented by Dr. D. Hume Hart; two Short-headed Phalangers (*Belideus breviceps* ♂ ♀), two Crested Pigeons (*Ocyphaps lophotes* ♂ ♀), a Modest Grass Finch (*Amadina modesta*) from Australia, two Bicheno's Finches (*Estrelda bichenovii*) from Queensland, a Funereal Cockatoo (*Calyptorhynchus funereus*) from New South Wales, a Saisset's Parrakeet (*Cyanorhamphus saisseti*) from New Caledonia, a New Zealand Parrakeet (*Cyanorhamphus novæ-zealandiæ*) from New Zealand, presented by Mr. T. H. Bowyer Bower, F.Z.S.; an Australian Cassowary (*Casuarius australis*) from Australia, presented by Capt. Mann; four Black Guillemots (*Uria grylle*) from Ireland, presented by Mr. H. Becher; a South American Rat Snake (*Spilotes variabilis*) from Brazil, presented by Mr. C. A. Craven, C.M.Z.S.; two Peacock Pheasants (*Polyplectron chinquis* ♂ ♂) from British Burmah, deposited.

WEATHER PROGNOSTICS AND WEATHER TYPES[1]

THE object of the first paper was to explain the best known popular prognostics by means of the most recent discoveries in meteorological science.

A great advance has been made in meteorology during the last twenty years owing to the introduction of daily synoptic charts of the distribution of atmospheric pressure, temperature,

[1] Abstract of two papers read before the Meteorological Society: "On Weather Prognostics," by Hon. Ralph Abercromby and W. Marriott; "On certain Types of British Weather," by Hon. R. Abercromby. (*Quarterly Journal of the Meteorological Society*, vol. ix. No. 45.)

wind, rain, &c. From these it is evident that there is a distinct relation existing between the distribution of pressure and the direction and force of the wind, and temperature and weather generally. A glance at a number of the charts shows that there is nearly always present either an area of low pressure called a cyclone, usually having a circular form, and as a rule moving in an easterly or north-easterly direction; or an area of high pressure, called an anticyclone, also nearly circular in form but almost stationary in position. The wind in all cases also blows nearly parallel with the isobars, having the region of lowest pressure on the left hand. This has given rise to the following simple law propounded by Dr. Buys Ballot for the northern hemisphere,

Fig. 1.—Cyclone Prognostics.

viz. "Stand with your back to the wind, and the barometer will be lower on your left hand than on your right." In cyclones the wind circulates round the isobars in the opposite way to which the hands of a watch move, but exhibits usually a little indraft; while in anticyclones the wind circulates round them in the same way as the hands of a watch, but exhibits usually a little outward motion. The velocity of the wind in all cases depends mainly upon the closeness of the isobars; for the closer the isobars the greater is the difference in pressure, and consequently the stronger the wind.

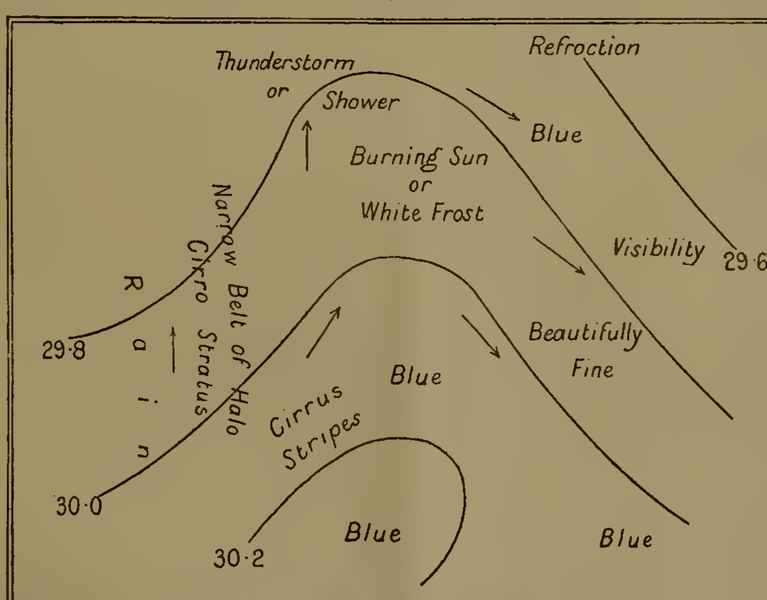


Fig. 2.—Wedge-Shaped Isobar Prognostics.

Since therefore nearly all our weather is of the cyclonic or anticyclonic type, and is entirely dependent upon the form and closeness of the isobars, it is by the aid of isobaric charts that the authors have attempted to explain a number of popular prognostics, and to associate them with certain kinds of weather.

The method of research actually adopted has been for many years past to take notes of any good observation of any prognostic and put them by in a portfolio with the nearest synoptic chart available; or preferably with the nearest both before and after. When a sufficient number had been collected they were

analysed, and the remarkable result has been arrived at that the greater number of prognostics are simply descriptive of the weather and appearance of the sky in the different portions of the various shapes of isobars seen on synoptic charts; and that they indicate foul or fair weather just as they precede the shifting areas of rain or blue sky which are mapped out by the isobaric lines.

These charts not only show the success of the prognostics, but also explain wherein they sometimes fail, by tracing the changes of each particular condition of weather. Hitherto the only prog-

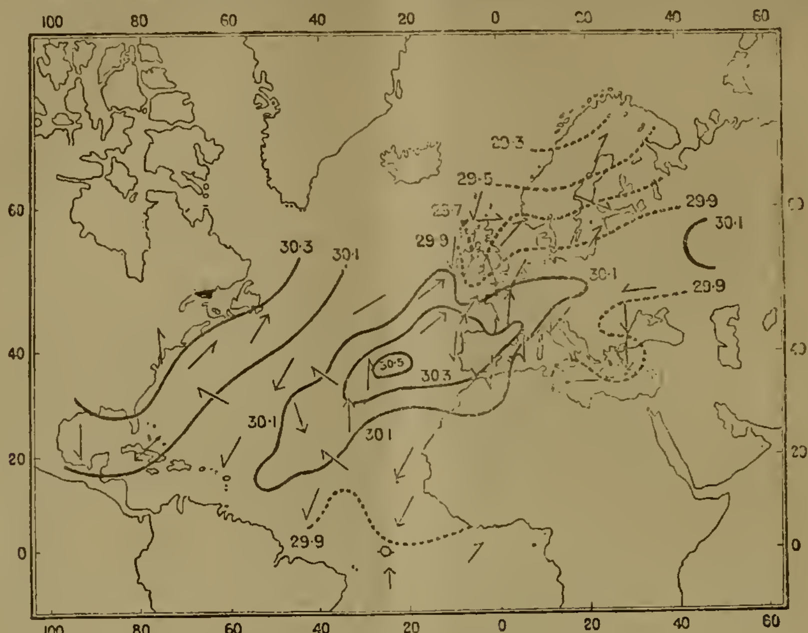


Fig. 3.

nostics which have been accounted for have been those due to excessive damp, but by means of isobaric charts many others can be readily explained. It must not be supposed that the modern methods diminish the value of prognostics, for even in forecasting weather from synoptic charts they are of great value, and will always be exceedingly useful to solitary observers who have only a single barometer to depend upon besides these prognostics, as for instance on board ship.

Though this way of treating prognostics is a great advance on the older methods, still there remains what may be called a

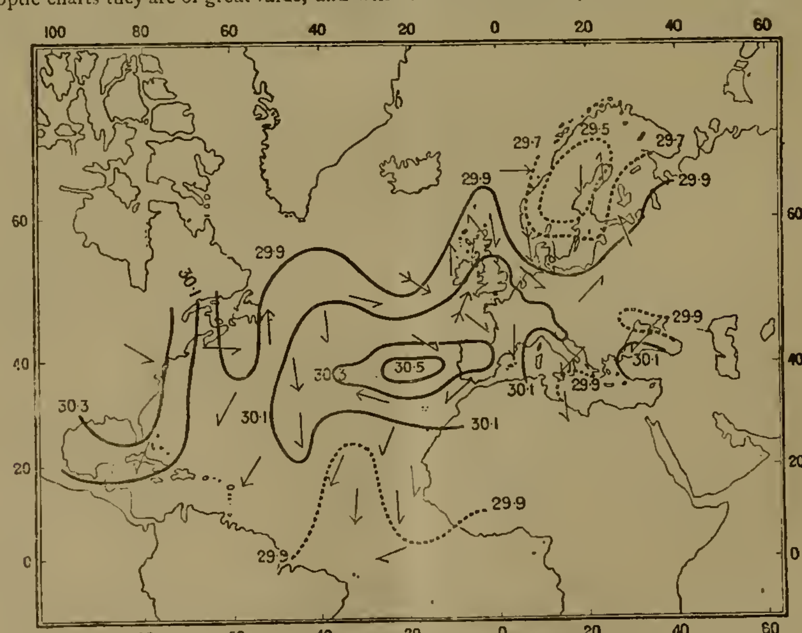


Fig. 4.

higher line of explanation. There is no doubt that the different shapes of isobars are the product of different phases of atmospheric circulation, just like the eddies and backwaters of a river, and that the appearance of the weather is the product of the complex vertical and lateral movements thus set up. For instance, there is no doubt that the principal cause of rain in a cyclone is the condensation of the ascentional current of air round its centre, while Ley and others have shown that many of the well-known forms of clouds are due to the action of upper currents moving in a different direction to those on the surface, and with a different

velocity. Similarly the bright dry weather of an anticyclone is certainly due to the descending current found round its centre, and so on for every shape of isobars. Any reference to these movements was, however, intentionally omitted by the authors, as these movements are still to a certain extent only partially understood, and it was their desire to rest the explanations which they gave exclusively on observation without reference to any theoretical considerations.

In a cyclone the broad features of the weather are a patch of rain near the centre, surrounded by a ring of cloud. But if we write down on a diagram, as in Fig. 1, the details of weather and kind of cloud in the different portions of the cyclone, we find that many of the best-known prognostics owe their value to the fact that they are characteristic of the front of a cyclone, and that after they have been observed, the rainy portion must pass over the observer before the sky becomes clear again. Sometimes a cyclone, after crossing a portion of the British Isles, dies out, and then the prognostics will fail in some districts.

The prognostics of settled fine weather are shown to be characteristic of anticyclones, which are nearly stationary for several days, and even weeks, together.

Though the bulk of British weather is made up of cyclones and anticyclones, there are two other distributions of pressure, marked out by wedge-shaped isobars and straight isobars respectively, which are associated with many well-known sayings. The chief interest in these prognostics consists in the contrast which they present to cyclone prognostics, as in many cases they are associated with fine and dry weather as opposed to the damp of an approaching cyclone.

In the front of wedge-shaped isobars (which are frequently found between a retreating and advancing cyclone) the weather is beautifully fine, of the sort of which we should say that it was "too fine to last"; or, if it lasted a whole day, we should call it a "pet day."

During the day the sun is hot, at night white frost forms. Great visibility, with a blue sky, and unusual refraction, are often observed.

On the west side of the wedge-shaped area, as the new cyclone comes on, the blue sky gradually assumes a dirty appearance, accompanied by a halo, and gathers into cloud, and later on rain begins to fall; while in the southern portion the rain is often preceded by cirrus stripes, either lying with the wind, or sometimes at right angles to it.

"Cirrus at right angles to the wind is a sign of rain."

These are all shown in the diagram (Fig. 2).

Some very interesting rain prognostics are also associated with straight isobars. While those in a cyclone are preceded by an almost ominous calm, and a dirty, murky sky, these are associated

FIG. 5.

with a hard sky and blustery wind, of which it would be ordinarily remarked that "the wind keeps down the rain," or that, "when the wind falls, it will rain." While also the prognostics which precede cyclone rain hold good for the reason that they are seen in front of the rainy portion, those associated with straight isobars hold good because, though there is little rain actually with them, the area which they cover to-day will probably be covered by a cyclone to-morrow—the conditions being favourable for the passage or formation of cyclones.

Altogether, about 100 prognostics are associated with these four shapes of isobars.

The use and position of prognostics relative to forecasting from synoptic charts was stated thus:—

Theoretically, when the isobars are well-defined, we ought to be able to write down the prognostics which might be visible, but practically we cannot do so. Besides, there are sometimes cases of isobars which have no well-defined shape, but with which thunderstorms or heavy showers often occur. These, as is well known, hardly affect the barometer, but are abundantly forewarned by the commonest prognostics, and as the rainfall is usually heavy in them, the failure of the forecast which omits to notice them is very conspicuous.

The scope of the paper precluded entering into the complicated question of the non-cyclonic rainfalls in this country. It was only stated that the prognostics which precede them are rather those associated with broken weather, such as bright sunrise or heavy clouds banking up without the barometer falling, than the muggy, dirty weather of a cyclone front. The warning they give is also much shorter, rarely more than three or four hours, if so long.

The other paper is an attempt to classify certain types of British weather.

It is familiar to many observers that the weather in this country frequently occurs in spells of several weeks' duration, during which there is a remarkable persistence of the general type of weather overriding both a considerable fluctuation from day to day, and a considerable local variation from place to place.

For instance, the wind will often back to some point of south with a high temperature, a dull sky and rain, and then veer to some point of west with a cooler air and brighter sky; and after a day or so of fine weather it will back again to the south with bad weather, perhaps this time rising to the intensity of a gale, and subsequently veer towards the west with finer weather, and so on for weeks together.

The changes only vary in intensity and detail, not in general

character, while the feel of the weather and the look of the sky remain through all of them what are customarily associated with westerly winds.

Similarly the wind will often blow persistently from some point of east, fluctuating between south-east for fouler weather and north-east for finer weather, and back again with many variations for several weeks, during which the predominant features of the weather are always characteristic of east winds. The frequent recurrence of particular types of weather at particular seasons of the year is also a matter of common observation; the north-east winds of March, the cold north winds of the middle of June, and the wet west winds of September are well-known instances.

If we examine a large number of synoptic charts we find that relatively to Europe the general position of the great areas of high pressure frequently remained constant for a lengthened period. Further examination shows that the constancy of these positions coincides with persistent types of weather similar to those above mentioned, the fluctuation of type being due to the passage of cyclones, while the local variation depends on the position of the cyclone centres and on the innumerable local conditions which modify any general type.

Over the North Atlantic and Europe the distribution of atmospheric pressure presents certain constant features, namely—

1. An equatorial belt of nearly uniform low pressure.

2. A tropical belt of high pressure rising at intervals into great irregular elevations or anticyclones.

3. A temperate and Arctic region of generally low pressure, but in which occasionally areas of high pressure appear for a considerable period.

The equatorial belt constantly covers the Sahara and the Amazon valley, and always narrows over the Atlantic at about 30° west longitude, where it often does not reach higher than 10° north latitude. The shape and depth of this area are tolerably constant.

The tropical belt comprises a region of high pressure rising at variable intervals into great anticyclones. Their position is generally variable, with the exception of one, which is always found over the central Atlantic. This anticyclone forms a very important factor of the weather of western Europe, and will be constantly referred to as "the Atlantic anticyclone." Its extension south and west is tolerably constant, while towards north and east it is variable, sometimes rising as far as 60° north and stretching over Great Britain and continental Europe.

The temperate and Arctic region extends from the tropical high pressure belt to the pole. The pressure, though ordinarily low, is perpetually fluctuating by reason of the incessant passage of cyclones; yet occasionally persistent areas of high pressure appear in certain portions of it.

With reference to western Europe there are at least four persistent types of weather—

1. The southerly, in which an anticyclone lies to the east or south-east of Great Britain, while cyclones coming in from the Atlantic either beat up against it or pass towards north-east.

2. The westerly, in which a tropical belt of anticyclones is found to the south of Great Britain, and the cyclones which are formed in the central Atlantic pass towards east or north-east.

3. The northerly, in which the Atlantic anticyclone stretches far to the west and north-west of Great Britain, roughly covering the ocean. In this case cyclones spring up on the north or east side, and either work round the anticyclone to the south-east, or leave it and travel rapidly towards the east.

4. The easterly, in which an apparently non-tropical anticyclone (or one disconnected with the tropical high-pressure belt) appears in the north-east of Europe, rarely extending beyond the coast-line, while the Atlantic anticyclone is occasionally totally absent from the Bay of Biscay. The cyclones, then, either come in from the Atlantic and pass south-east between the two anticyclones, or else, their progress being impeded, they are arrested or deflected by the north-east anticyclone. Sometimes they are formed to the south of the north-east anticyclone, and advance slowly towards the east, or in very rare instances towards the west.

The details of the southerly and westerly types are given in the paper. Here we can only reproduce the three diagrams of the westerly type, Figs. 3, 4, and 5, in which the general characteristics of the type, just mentioned, are readily seen.

The value of the recognition of type groups is shown in the following ways:—

1. They explain many phenomena of weather, and many popular prognostics.

For instance, besides showing the nature of spells of good, bad, dry weather, &c., they explain by reason of their persistence such prognostics as why "grouse coming down into farmyards are a sign of snow." Also why the prognostics, "When a river like the Tweed rises without any rain having fallen," or "Irregular tides are signs of rain," have a significance for the future; for though both are caused by past bad weather at a distance, yet the persistent type will almost certainly sooner or later bring more bad weather over the place of observation.

Then the recurrence of hot and cold periods, many of them well known, are shown to be due to the recurrence of a similar type of pressure distribution about the same season of the year. Particulars of seventeen such are given, and the manner in which the knowledge of them can be utilised in forecasting is stated thus: that though the forecaster is not justified in stating that any period will occur absolutely, still when about the time of its usual recurrence the synoptic charts show signs of the expected type, then the forecasts for a few days ahead can be issued with greater confidence. For instance, suppose that about November 6—a cold period—the charts begin to show traces of the northerly type, then, but not before, there would be good grounds for saying that a period of cold weather, which usually occurs at this season, has already set in, and may be expected to last for five or six days, the forecaster being thus enabled to issue a much longer forecast than can as a rule be safely attempted.

2. Type groups are of the utmost value in forecasting, for when the existence of the type is fairly recognised then the general features of the weather are at once given, as well as the probable motion of the cyclones which are formed during the continuance of the type. Unfortunately in many cases no certain indications can be given of an approaching change of type.

3. Statistical results can be corrected by their means, for they give a true test of identity of recurrent weather, which no single item, such as heat, cold, rain, &c., can do.

4. They enable geological questions to be treated, such as the influence of changing distribution of land and sea on climate, in a more satisfactory manner than any other method.

The general principles of prognostics and types hold all over the world, but the details in these papers apply to Great Britain only.

RALPH ABERCROMBY

OUR ASTRONOMICAL COLUMN

THE GREAT COMET OF 1882.—It appears quite possible that as the moon draws away from the morning sky towards the end of the present month, this comet may be again observed with our larger instruments. Its distance from the earth has been increasing from soon after perihelion passage in September last, and a maximum takes place at the beginning of September next, when the distance is 5·988; the earth then for a time overtakes the comet, and the distance diminishes to 5·709 on December 1. The intensity of light, however, is greatest at the end of August, and the comet then rises at a sufficient interval before the sun to render observation feasible. It will at least be of much interest to ascertain if the comet can be reached with our most powerful telescopes. The only comet which has been hitherto observed under similar conditions is the celebrated one of 1811, which, it may be remembered, was observed by Wisniewsky at Neu-Tscherkask, in August 1812.

The following places are deduced from the elliptical elements calculated by W. Fabritius of Kiev (*Astron. Nach.*, No. 2514), from a wider arc of observation than any other orbit yet published:—

At Greenwich Midnight

	R.A.	N.P.D.	Log. distance from Earth.	Log. distance from Sun.
	h. m. s.	° ′		
Aug. 28	7 25 58	98 32·0	0·7773	0·7306
30	7 26 44	98 41·6		
Sept. 1	7 27 28	98 51·4	0·7773	0·7339
3	7 28 10	99 1·4		
5	7 28 49	99 11·5	0·7771	0·7372
7	7 29 26	99 21·7		
9	7 30 0	99 32·1	0·7768	0·7405

Dr. Julius Schmidt last saw the comet at Athens on April 28; in a letter addressed to NATURE, Mr. A. S. Atkinson of Nelson, N.Z., states that with a 4-inch refractor he saw it with certainty on May 6. Assuming the theoretical intensity of light on the latter date to be unity, the intensity on August 28 is 0·35.

THE ASTRONOMISCHE GESELLSCHAFT.—The next meeting of this society will be held at Vienna, in the apartments of the Academy of Sciences, from September 14–17, under the presi-

dency of Prof. Auwers ; the secretary is Prof. Schœnfeld, director of the Observatory at Bonn.

The last part of the *Vierteljahrsschrift* contains reports of the proceedings during the year 1882, from twenty-eight continental observatories, public and private. Also a portrait of the late Prof. Plantamour of Geneva.

EPHEMERIDES OF THE SATELLITES.—The last number of the *Monthly Notices of the Royal Astronomical Society* contains Mr. Marth's extensive ephemerides of the satellites of Saturn (excepting *Hyperion*), Uranus, and Neptune for their next oppositions, as well as data to facilitate the reduction of physical observations of Jupiter. *Hyperion* will have been omitted from want of reliable elements. Prof. Newcomb, however, is in possession of manuscript tables, which he has utilised in the *American Ephemeris* for 1883 ; we extract the early portion of his table: I represents inferior, and S superior, conjunction ; E, east, and W, west elongation ; the times are for the meridian of Washington (5h. 8m. west of Greenwich) :—

	h.			h.			h.
Aug. 18,	2·9 E	...	Sept. 8,	10·6 E	...	Sept. 29,	17·0 E
23,	10·9 I	...	13,	18·4 I	...	Oct. 5,	0·6 I
28,	18·8 W	...	19,	1·9 W	...	10,	8·0 W
Sept. 3,	2·7 S	...	24,	9·5 S	...	15,	15·5 S

SCIENTIFIC SERIALS

Journal de Physique Théorique et Appliqué, July, 1883.—On the theory of electromagnetic machines, by J. Joubert.—Experiments on the aurora borealis in Lapland, by S. Lemström.—Note on a spectroscope with inclined slit, by M. Garbe.—A differential thermometer for class demonstration, by H. Dufour.—An addition to Atwood's machine, by A. Béquié.—The determination of the ohm by dynamometric methods, translated by M. Brillouin.—Electrochemical figure, with diagram, translated by Adrien Guébhard.

Rendiconti of the *Royal Lombard Institute of Sciences and Letters*, June 28, 1883.—On the theory of the potential, by Prof. E. Beltrami.—Note on the latitude of Milan, deduced from calculations of distances from the zenith observed near the meridian, by M. E. G. Celoria. In this concluding paper the author fixes the exact latitude of Milan (centre of the large tower of the observatory), at 45° 27′ 59″·34 ± 0″·09. . .A_1.—On the kinematic significance of wave surface, by Dr. G. A. Maggi.—Observations on the figure of the planet Uranus, by E. G. V. Schiaparelli. Besides calculating its ellipticity, which agrees with the conclusions of Mädler and Shafarik, the author determines the presence of spots and changes of colour on the surface of Uranus.—Results of a microscopic analysis of the drinking water at Cadempino, Canton of Ticino, Switzerland, by Prof. L. Maggi.—A case of policheiria (abnormal number of claws) in a freshwater crab (*Astacus fluviatilis*, Rond.), by Dr. E. Cantoni. Appended to the paper is a bibliography of crustacean teratology.—Remarkable results obtained by the treatment of pulmonary tuberculosis with iodoform, by Prof. G. Sormani.—On a Russian scheme of international exchanges, by Prof. E. Vidardi.

SOCIETIES AND ACADEMIES

LONDON

Royal Society, June 21.—"Supplement to former Paper entitled—'Experimental Inquiry into the Composition of some of the Animals Fed and Slaughtered as Human Food'—*Composition of the Ash of the Entire Animals and of certain Separated Parts.*" By Sir John Bennet Lawes, Bart., LL.D., F.R.S., F.C.S., and Joseph Henry Gilbert, Ph.D., LL.D., F.R.S., V.P.C.S.

In a former paper (*Phil. Trans.*, Part II. 1859) the authors had given the actual weights, and the percentage proportion in the entire body, of the individual organs, and of certain more arbitrarily separated parts, of 326 animals—oxen, sheep, and pigs—in different conditions as to age, maturity, fatness, &c. They called particular attention to the wide difference in the proportion by weight of the stomachs and intestines in the three descriptions of animal ; the proportion of stomach and contents being very much the highest in oxen, considerably less in sheep, and little more than one-tenth as much in pigs as in oxen. On the other hand, the intestines and contents contributed a less proportion to the weight of the body in oxen than in either sheep or pigs ; the percentage by weight in pigs being nearly twice as high as in sheep, and more than twice as high as in oxen. With these very characteristic differences in the proportion of the receptacles and first laboratories of the food the other internal organs collectively, as also the blood, contributed a pretty equal proportion by weight of the entire body, in the three descriptions of animal.

Ten animals had been selected for the determination of the chemical composition, namely—a fat calf, a half-fat ox, and a fat ox ; a fat lamb, a store sheep, a half-fat sheep, a fat sheep, and a very fat sheep ; a store pig, and a fat pig. In these, in the collective carcass parts, in the collective offal parts, and in the entire bodies, the total nitrogenous substance, the total fat, the total mineral matter, the total dry substance, and the water, were determined ; and the results were recorded and discussed in detail.

It was shown that, as the animal fattened, the percentage of nitrogenous substance decreased considerably, whilst that of the fat and of the total dry matter increased in a much greater degree. It was estimated that the portions of well fattened animals which would be consumed as human food would contain three, four, and even more times as much fat as dry nitrogenous substance : and comparing such animal food with wheat-flour bread, it was concluded that, taking into consideration the much higher capacity for oxidation of a given weight of fat than of starch, such animal food contributed a much higher proportion of non-nitrogenous substance, reckoned as starch, to one of nitrogenous substance than bread. In fact the introduction of our staple animal foods to supplement our otherwise mainly farinaceous diet did not increase, but reduced the relation of the flesh-forming material to the respiratory and fat-forming capacity of the food.

Finally, the actual amount and the percentage of total ash in most of the internal organs and some other separated parts were given. It was shown that the percentage of total mineral matter, like that of the nitrogenous substance, decreased not only in the entire body, but especially in the collective carcass parts, as the animals matured. It was the object of the present communication to record the results of the complete analysis of the ashes of the collective carcass parts, of the collective offal parts, and of all parts of each of the ten animals. Forty complete ash analyses had been made.

As was to be expected, more than four-fifths of the ashes consisted of phosphoric acid, lime, and magnesia ; these making up the largest amount in the ash of the oxen, less in that of sheep, and less still in that of pigs. Potash and soda were also prominent constituents. Assuming, for the purposes of illustration merely, that one of phosphoric acid was combined with three of fixed base, the ashes of the ruminants showed an excess of base ; whereas, according to the same mode of calculation, the ashes of the pigs showed no such excess.

It was, unfortunately, only in the case of the offal parts of the pigs that the ash of the chiefly bony and that of the chiefly soft parts had been analysed separately. The results showed a considerable excess of acid, especially phosphoric, in the ash of the non-bony portions ; presumably, in part at any rate, due to the oxidation of phosphorus in the incineration. In further reference to the point in question it may be stated that, although the oxen and sheep show a higher percentage of total nitrogenous substance than the pigs, yet, owing to the relatively small proportion of bone in the pigs, the amount of ash yielded from the non-bony parts is higher in proportion to that from the bones in their case than in that of the ruminants.

Comparing the percentage composition of the ashes of the entire bodies of the different animals, the chief points of distinction were that in the ash of the pigs there is a lower percentage of lime and a higher percentage of potash and soda than in the corresponding ash of the ruminants ; there is a somewhat higher percentage of phosphoric acid in the ash of the pigs and of the oxen than in that of the sheep ; and there is a higher percentage of sulphuric acid (and somewhat of chlorine also) in the ash of the pigs than in that of the other animals.

A table showing the quantities of total ash, and of each individual mineral constituent, in each of the ten animals analysed was given. Not much stress was laid on the amounts in the particular animals analysed, as the actual weights and condition of animals coming under similar designations may vary considerably.

It was of more interest to consider the amounts of the mineral constituents in carcass parts, in offal parts, and in all parts per 1000 lbs. fasted live-weight, of each description of animal.

It was shown that a given live-weight of oxen carried off much

more mineral matter than the same weight of sheep, and a given weight of sheep much more than the same weight of pigs. With each description of animal the amounts of phosphoric acid, lime, and magnesia, are less in a given live-weight of the fatter than of the comparable leaner individuals. Of both potash and soda, again, the quantity is less in a given live-weight of the fatter animals. The same may be said of the sulphuric acid and the chlorine; in fact, in a greater or less degree, of every one of the mineral constituents.

It was estimated that the loss to the farm of mineral constituents by the production and sale of mere fattening increase was very small. It was greater of course in the case of growing than of only fattening animals. In illustration, the amounts of some of the most important mineral constituents removed annually from an acre of fair average pasture and arable land in various products were compared. Such estimates could obviously be only approximate, and the quantities will vary considerably. With this reservation it may be stated that, of phosphoric acid, an acre would lose more in milk, and four or five times as much in wheat or barley grain, or in hay, as in the fattening increase of oxen or sheep. Of lime, the land would lose about twice as much in the animal increase as in milk, or in wheat or barley grain; but perhaps not more than one-tenth as much as in hay. Of potash, again, an acre would yield only a fraction of a pound in animal increase, six or eight times as much in milk, twenty or thirty times as much in wheat or barley grain, and more than 100 times as much in hay.

From the point of view of the physiologist, it would doubtless have been desirable that the selection of parts for the preparation and analysis of the ash should have been different, and more detailed. The agricultural aspects of the subject had, however, necessarily influenced the course of the inquiry; and the extent of the essential work had enforced the limitation which had been adopted. The results must be accepted as a substantial contribution to the chemical statistics of the feeding of the animals of the farm for human food.

PARIS

Academy of Sciences, July 23.—M. Blanchard, president, in the chair.—Historic importance of Nicolas Leblanc's discovery of the method of extracting artificial soda from marine salt, by M. Dumas. To this great discovery, which the author compares with that of the steam-engine by Watt, is traced the vast development of the chemical industries during the last hundred years. The present annual consumption of the carbonate of soda resulting from Leblanc's process is estimated at from 700,000,000 to 800,000,000 kilograms in Europe and America. Yet the name of the discoverer had almost been forgotten till recently revived by the municipality of his birthplace, Issoudun, which now proposes to erect a monument to his memory.—Active or dynamic resistance of solids (continued). Graphic representation of the laws of longitudinal thrust applied to one end of a prismatic rod, the other end of which is fixed, by MM. de Saint-Venant and Flamant.—Method of distributing the heat developed in the process of forging, by M. Tresca.—Description of the new apparatus about to be fitted up in the Paris Observatory for the purpose of studying the movements of the sun, by M. C. Wolf. This mechanism, which is based on the same principle as that adopted by G. and H. Darwin in the Cavendish laboratory, Cambridge, is intended more especially for the observation of solar oscillations and deviations from the vertical.—On the present outbreak of cholera in Egypt, and on the probability of Europe escaping its ravages, by M. A. Fauvel. Every day tended to diminish the chance of an invasion, and should the epidemic be staved off for the next four or five weeks there would be little cause for further apprehension, as it was expected from past experiences that Egypt itself would be entirely free within six weeks at the outside. With regard to the prediction confidently made in many quarters, that the epidemic would reach the mainland through England, the author remarked that on the contrary it had on all previous occasions found its way to England from the Baltic ports on the mainland. He regarded Greece and Spain as in any case free from danger, and thought that in case it appeared on the French seaboard it might easily be prevented from spreading inland by carefully isolating the patients. He considered that the two cities most exposed to its attacks were Constantinople and Trieste, the former through Syria and Asia Minor, the latter through the arrival of immigrants escaping from Egypt. Notwithstanding the recent disclosures made on the spot, he still holds the view that the cholera was originally introduced into Egypt from Bombay in consequence of the suspension of the precautionary measures formerly adopted by the Egyptian Government against the epidemic.—On the origin of the nitrogen existing in combination on the surface of the earth, by MM. A. Müntz and E. Aubin. Nitrogenous combinations are due in the first instance to the electric phenomena of which the terrestrial atmosphere is the seat. These phenomena appear to have been much more intense in remote geological epochs than since the appearance of animal and vegetable life on the earth. Hence it would seem that we are now depending on a constantly diminishing stock of combined nitrogen, and the process of diminution must go on unless atmospheric electricity prove to be a source of sufficient reparation.—On the adaptation to viticulture of the sandy tracts of the Landes and Gironde in the south-west of France, by M. A. Robinson.—Experimental researches on the action of a liquid introduced by a special process into the tissues of the vine for the purpose of destroying phylloxera, by M. P. de Lafitte. Sulphate of copper diluted in water is recommended as best answering all the conditions, and consequently as the surest antidote to the evil.—On some linear differential equations of the fourth order, by M. Halphen.—On certain special solutions of the problem of the three bodies, by M. H. Poincaré.—On some recently observed solar perturbations, by Admiral Mouchez.—On a universal galvanometer without oscillatory action, adapted for the measurement of currents of great intensity or of high tension, with illustration, by M. Ducretet.—On the nitric derivatives of hydride of ethylene, by M. Berthelot.—On some derivatives of mannitic hexylene, by M. Wurtz.—On the products derived from the bacterian fermentation of albuminoids, by MM. Arm. Gautier and A. Etard.—On the supposed transformation of brucine into strychnine, by M. Hanriot.—On the heat-generating power of coal, by M. Scheurer-Kestner.—On the physiological properties of the bark of the dundaké (a West African shrub) and of dundakine, by MM. Bochefontaine, B. Feris, and Marcus.—On the nervous chords in the foot of the heliotides, by M. H. Wegmann.—On the temperatures of the sea observed at Concarneau and Douarnenez, by M. Goez.—A reply to M. Certes on the subject of the method proposed by him for examining corpuscles held in suspension in water, by M. Eug. Marchand.

CONTENTS

THURSDAY, AUGUST 9, 1883

TWO "EMINENT SCOTSMEN"

James Nasmyth, Engineer. An Autobiography. Edited by Samuel Smiles, LL.D. (London: Murray, 1883.)

The Life of John Duncan, Scotch Weaver and Botanist, with Sketches of his Friends and Notices of his Times. By William Jolly. (London: Kegan Paul and Co., 1883.)

WE do not know in what particular direction Dr. Smiles has exercised his editorial functions in the charming autobiography of Mr. Nasmyth. The "pruning-knife" which the latter advised him to use freely was surely not needed; the inventor of the steam-hammer gossips so delightfully about himself that we should have been glad had he gone on to a much greater length. On the other hand it is a pity that Mr. Jolly had not obtained the services of some judicious editorial pruner. He himself has evidently not had the leisure to write briefly, and his book is therefore a somewhat heterogeneous collection of materials much in want of rearrangement and cutting down.

Mr. Nasmyth's autobiography, we venture to think, is likely to become a classic in the section of literature to which it belongs. The genial simplicity, the unconscious and perfectly just self-appreciation with which the great engineer and student of science talks of his career and his work, enlists from the first the reader's sympathy and interest. His father, Alexander Nasmyth, a painter of high rank and the founder of the Scottish landscape school, was himself a genius in mechanics; and an atmosphere of mechanical invention pervaded his happy home in Edinburgh. He was one of the select party on board Symington's steamer on Dalswinton Loch in 1788; and among his fellow-passengers was Robert Burns, a fact new to us. Mr. Nasmyth gives us a delightful sketch of his father and his happy family and the simple Edinburgh life of the time. He himself was born in 1808, and educated at the High School of Edinburgh. From his earliest years he delighted in mechanical invention, and was great at making "peeries" and toy cannon. He naturally, as his father's son, learned the use of the pencil, and insists strongly on the great value of drawing to a mechanical engineer. He himself, throughout life, has made almost daily use of his skill in this art, and by the facility with which he could record his ideas and incipient inventions in this form, saved himself much writing, and preserved much that would otherwise have been lost. He left the High School in 1820, wh-n only twelve years of age, though afterwards he attended classes at Edinburgh University. At this early period he says of himself:—

"I was constantly busy; mind, hands, and body were kept in a state of delightful and instructive activity. When not drawing, I occupied myself in my father's workshop at the lathe, the furnace, or the bench. I gradually became initiated into every variety of mechanical and chemical manipulation. I made my own tools and constructed my chemical apparatus, as far as lay in my power. With respect to the latter, I constructed a very handy and effective blowpipe apparatus, consisting of a small air force-pump, connected with a cylindrical vessel of tin plate. By means of an occasional use of the handy pump, it yielded such a fine steady blowpipe blast, as enabled me to bend glass tubes and blow bulbs for thermometers, to analyse metals or mineral substances, or to do any other work for which intense heat was necessary. My natural aptitude for manipulation, whether in mechanical or chemical operations, proved very serviceable to myself as well as to others; and (as will be shown hereafter) it gained for me the friendship of many distinguished scientific men."

He had moreover taken part in really practical work in some Edinburgh workshops, and at the age of seventeen he was constructing small steam-engines and models for illustrative purposes, and two years later he invented a very efficient road steam-engine. The great event in Nasmyth's early life, however, was his engagement in the great engineering works of Henry Maudsley, of London, in 1829. Maudsley was, indeed, so impressed with what he saw of the young Scotchman's intelligence, knowledge, and skill, that he at once took Nasmyth into his confidence as his personal assistant. In London, as in Edinburgh, Mr. Nasmyth made many friends among those whose friendship was best worth having; through Brougham, for instance, he became acquainted with Faraday, whose friendship he retained to the end of the latter's life.

In order that he might be able to live upon his rather scanty wages, Nasmyth invented an ingenious cooking-stove, a sketch of which he gives, and by means of which he was able to cook a "capital dinner" at $4\frac{1}{2}$*d.* Long before this his attention had been given to the contrivance of accurate cutting-tools, and one of the first things he did for Maudsley was to construct a nut-cutting machine. A visit to the north of England, in 1830, one of the objects of which was to see Stephenson's "Rocket," gave him the first idea of settling ultimately in business for himself in the neighbourhood of Manchester. And so indeed he did in 1832, in a very small way, for his means at the time were limited. Business rapidly increased, and he had shortly to remove to new premises at Patricroft, where in 1836 the great Bridgewater Foundry was in complete and efficient action. For twenty years after this Mr. Nasmyth continued at the head of his constantly growing establishment, adding to his inventions, and extending his operations at home and abroad. The result was that at the early age of forty-eight years he felt himself in the happy position to be able to retire entirely from business and devote his life to those scientific and artistic pursuits which had been to him a constant source of pleasure. Indeed it was his full and accurate knowledge of the science of his art, combined with his native insight and common sense, that enabled him to achieve so many mechanical triumphs.

Mr. Nasmyth naturally enters in considerable detail into the history of the steam-hammer, with which his name is so intimately associated. The conception and completion of the invention seems to have been the work of a very brief time. He was incited to it, so early as 1839, by the difficulty which Mr. Humphries, the engineer who had charge of the construction of the *Great Britain* steamship, found in finding forges powerful enough to weld the paddle-shaft of that vessel. Mr. Humphries wrote to Mr. Nasmyth on the subject, and, says the latter:—

"This letter immediately set me a-thinking. How was

it that the existing hammers were incapable of forging a wrought-iron shaft of thirty inches diameter? Simply because of their want of compass, of range and fall, as well as of their want of power of blow. A few moments' rapid thought satisfied me that it was by our rigidly adhering to the old traditional form of a smith's hand hammer—of which the forge and tilt hammer, although driven by water or steam power, were mere enlarged modifications—that the difficulty had arisen; as, whenever the largest forge hammer was tilted up to its full height, its range was so small that when a piece of work of considerable size was placed on the anvil, the hammer became 'gagged'; so that, when the forging required the most powerful blow, it received next to no blow at all, as the clear space for the fall of the hammer was almost entirely occupied by the work on the anvil.

"The obvious remedy was to contrive some method by which a ponderous block of iron should be lifted to a sufficient height above the object on which it was desired to strike a blow, and then to let the block fall down upon the forging, guiding it in its descent by such simple means as should give the required precision in the percussive action of the falling mass. Following up this idea, I got out my 'Scheme Book,' on the pages of which I generally *thought out*, with the aid of pen and pencil, such mechanical adaptations as I had conceived in my mind, and was thereby enabled to render them visible. I then rapidly sketched out my Steam Hammer, having it all clearly before me in my mind's eye. In little more than half an hour after receiving Mr. Humphries' letter narrating its unlooked-for difficulty, I had the whole contrivance, in all its executant details, before me in a page of my Scheme Book, a reduced photographed copy of which I append to this description. The date of this first drawing was the 24th November, 1839."

The paddle-wheel of the *Great Britain* was, however, never forged, as about that time the substitution of the screw for the paddle-wheel as a means of propulsion was attracting much attention. Indeed, Mr. Nasmyth could get no English firm to take up his invention, and was naturally surprised to find, on a visit he made to France in 1842, that his steam-hammer was in full operation at Creuzot, M. Schneider having copied the design from Mr. Nasmyth's drawing when on a visit to Patricroft. Very naturally Mr. Nasmyth on his return to England lost no time in protecting his invention by patent; its career since is well known.

As we said, Mr. Nasmyth retired from business in 1856, twenty-eight years ago, bought a "Cottage" in Kent, a picturesque place near Penshurst, to which he gave the characteristic name of Hammerfield. Long before this he had learned to take an interest in science, especially in geology and astronomy. His investigations into the structure of the moon are well known, and these, as well as his examinations of the sun's surface, have been conducted with telescopes of his own construction. His elaborate work on the moon, with its magnificent series of views of its surface, has long been classical, and his contributions to the subject of the sun's heat are well known. His imagination, when not engaged in devising mechanical contrivances and contributing to scientific theory, has often blossomed into fancy which has found expression in exquisite pictures of fairy-land and other regions of the unseen. Altogether Mr. Nasmyth's long life has been one of almost unchequered success; from the first he has clearly seen what he wished to accomplish, and with scientific precision has devised the most effective means of realising his aims. Not the least delightful and instructive of his many works is the one before us, which we commend to the study of all young engineers, as well as to all who wish to read the story of a successful life simply and pleasantly told.

John Duncan's career, as told by Mr. Jolly, is a complete contrast to that of Mr. Nasmyth. He never rose above the humble station in which he was born, nor apparently ever wished to do so. He had all along to struggle for a bare living, and was essentially unpractical. What little education he had was self-acquired, and it was never much so far as book-learning goes. His love of flowers was a passion. He amid many discouragements managed to acquire a mastery of systematic botany, and his collection of Scottish plants, now in the possession of Aberdeen University, is of real value. Every moment he could spare was devoted to adding to his collection, and partly as weaver and partly as harvester he traversed most of his native land. In other respects he was a man of superior mind, though in no sense a genius, and by no means to be compared with Robert Dick or even Thomas Edward. Mr. Jolly has narrated in our own columns the main facts of Duncan's career. Had he been more happily situated he would certainly have done real service to science. It is some consolation to think that his merits were recognised before he died, and that his last days were surrounded with comforts and attentions to which throughout his previous life he had been a stranger. As we have said, Mr. Jolly has made too big a book of the materials he has collected, and although it abounds in interest, it would have been more creditable to his literary skill had he taken the trouble to rid it of redundancies.

THE HEAVENLY BODIES

The Heavenly Bodies; their Nature and Habitability. By W. Miller, S.S.C. Edinburgh, Author of "Wintering in the Riviera." Pp. 347. (London: Hodder and Stoughton, 1883.)

FEW subjects could be mentioned more remote from the common interests and pursuits of life than what has been usually called the "plurality of worlds," an expression now so long restricted to one well-ascertained meaning as to have lost any ambiguity that might have been charged upon it. The question is one of mere curiosity, and leads to no direct result; but it has always carried with it an attraction irrespective of its unpractical nature, and has exercised the ingenuity of so many minds that its literature is of no inconsiderable extent. To this the book now in our hands is the most recent contribution. It is not the work of an astronomer, as the author himself has informed us; but as his profession leads him to the examination of evidence this need not be considered a material disadvantage. His position, however, in this respect would have been improved by a little more care in the collection of his data, which in some instances, such as Mädler's "central sun," the satellites of Uranus and Neptune, the polar flattening of Mars, and the observations of Schiaparelli, are somewhat in arrear; and it may be the case that those more intimately conversant with the subject would estimate the

comparative value of the evidence somewhat differently. He has taken a very commendable degree of pains in collecting the opinions of former writers; though we have met with no notice of worthy old Derham, or the quick-eyed but fanciful Gruithuisen; but the natural result is the revival of a good deal of antiquated matter that can hardly claim a hearing before a modern tribunal; such as the assumptions of the Cosmotheoros (which by the way he invariably cites as "Cosmothereos") or the affected *niaiseries* of Fontenelle. In fact, excepting for those who would find interest or amusement in specimens of almost all that has been said upon the subject, however absurdly nonsensical, or needlessly pugnacious, the book would gain by a process of winnowing and compression and "weighting," if we may be permitted to use a technical expression. And there can be no question as to the advantage of a more careful revision of the press.

As regards the author's own share, there is much deserving of attention. He writes in an excellent spirit; in espousing the negative side of the question, there is no unfairness towards his opponents; and though some of his arguments carry little weight—for instance that drawn from what seems to him the "dismal," "horrible," "terrifying" aspect of the moon—others are well considered and expressed; and some collateral questions are handled in a way which demands attention, and will well repay it. With regard to the point in hand, if the present volume may not be thought to have done much to decide the controversy, it may be doubtful whether any future successor may do much more. The matter is in reality out of reach. The data are insufficient; and we venture to doubt whether any future generation may be able to attain more satisfactory ones. Long-continued and patient investigation may be fairly expected to throw some light upon the supposed final quiescence of the lunar surface; and possibly on the existence, under certain circumstances, of slight obscurations which might indicate the existence of a very attenuated atmospheric envelope; but this would still leave us at an immense and hopeless distance from any certain proof of habitation. As to the other heavenly bodies our position is worse still. The observations of Schiaparelli, supported to some extent by those of others, and at any rate deserving of respectful attention, tend to divest Mars of some of his supposed similarity to our own globe; and the conclusions hitherto attempted to be drawn as to the condition of the other planetary surfaces are, we venture to think, still less satisfactory. Opinion at present can be little better than conjecture; and it is uncertain at the best whether it will ever be permitted to us to make a further advance. The most ingenious analogical reasoning is not demonstration, and the decision of the finest telescopes would be invoked in vain. An interesting inquiry might be entered upon as to the prospects of opticians and observers; the conclusion possibly might be that their future is somewhat cloudy and obscure. At least we might venture to predict, from past experience, that the accomplished solution of any one of the mysteries which now confront us would only prove a prelude to problems still more insoluble, and proof still more convincing of the comparatively bounded character of all human knowledge.

OUR BOOK SHELF

United States Commission of Fish and Fisheries. Part vii. Report of the Commissioner for 1879.

THE contents of the present volume, embracing details of the work done by the United States Fishery Commissioner for the year 1879, are quite as varied and even of greater interest, if that be possible, than the preceding reports. The specific objects of the methodical inquiry which has now been going on for over twelve years, has for its object to report progress in regard to the propagation of food-fishes in the waters of the United States, as also to afford information as to the decrease in the stock of food-fishes. As has been already stated in the columns of NATURE, in which previous reports have been reviewed, the inquiry which has been so long in progress is being conducted in a thorough and searching way; it embraces the consideration of every topic calculated to throw light on the economy of the American fisheries. Nothing that can be deemed illustrative is neglected—the literature devoted to the natural history of food-fishes, or to descriptions of the fisheries of other countries, especially those of Europe, has been largely utilised in preparing the reports, with the result of making the volumes which have been issued a perfect encyclopædia of fishery information. The contents of the present report embrace a full account of the work overtaken in 1879 and the early part of 1880. The fishes which have been more particularly dealt with in the period noted are the Californian Salmon (*Salmo Quinnat*), the Atlantic Salmon (*S. Salar*), the Mountain or "Rainbow" Trout of California (*S. Irideus*), as also the Schoodic Salmon (*S. Salar*, var. *Sebago*). Various details are also given of what has been done in carp culture, as also of experiments made with the Striped Bass (*Roccus lineatus*), and the Shad (*Alosa sapidissima*). This fish is dealt with quite in wholesale fashion, the figures quoted being really marvellous, as many as 16,062,000 of young shad being distributed, a complete record being kept of the places to which they were forwarded; in the previous year the distribution of this fish reached the figure of fifteen and a half millions. Among the distinctive articles contributed to the present volume are some of rare importance; we may refer to that by Prof. Barlow on "The Marine Algæ of New England," which is both interesting and exhaustive; it extends to 210 pages of the volume now before us, and is illustrated by a series of well-executed drawings. Another paper of importance, full of curious information, is that of Mr. A. E. Verrill, "On the Cephalopods of the North-east Coast of America"; it is also profusely illustrated with fine drawings. "The Propagation of the Eel" is a contribution which is sure to attract attention; the article is by Dr. Otto Hermes, and was read before the German Fishery Association; although brief it contains many features of interest in connection with the natural history of the curious animal of which it treats, and describes most distinctly the differences of the two sexes. The author of this paper announces that the old eels, both males and females, die soon after the spawning season; "the extraordinarily rapid development of their organs of generation exhausts them to such a degree that they die soon after having spawned." This is the reason why they are never seen to return to the rivers. Among the miscellaneous contents of the present report will be found instructive essays on the food of marine animals, by Prof. E. Möbius. In the appendix will be found a very readable account of the herring fisheries of Iceland, as also a short treatise on the fisheries of the west coast of South America. One of the most scientific papers which is given is one containing a reprint of a series of extracts from the investigations of the Commission for the Scientific Examination of the German Seas—it contains much that will prove of interest both to naturalists and economists. It may be safely said alike of the

present and the preceding reports, that they contain a mass of information on fish and fisheries of a kind which has never been before brought to a focus, and in issuing such a guide to all interested, the United States Government has set us an example which we ought at once to follow. The volume is published at Washington, and is printed at the Government Printing Office.

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications.*

[*The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to insure the appearance even of communications containing interesting and novel facts.*]

Cyanogen in Small Induction Sparks in Free Air

AMONG the "Notes" in NATURE for July 19 (p. 281), where the products of combustion are given for various illuminants in common or uncommon use, and where coal-gas, oils, and candles have a fearful amount of both water-vapour and carbonic acid charged against them, the return for electric lights both in the arc and incandescent shapes is given as 0·0 for each; a return which is there considered to show "the great supremacy of electric lighting over all the other methods of illumination when considered as a matter of health."

Now this I believe is most happily true of the incandescent electric lights hermetically sealed in their vacuous glass globes; but who, on second thoughts, would presume to say that it is so with the arc lights, consuming their carbons visibly in the open air? The solid carbon gradually disappears from view, every one allows, and if it has not combined in gaseous condition with the oxygen of the atmosphere, like that of wax candles, it must have mainly combined with the nitrogen, and formed the far more deleterious compound gas, cyanogen, the basis of prussic acid: and that such gas or hydrocyanic acid is produced in the electric arc was set forth by Prof. James Dewar in the *Royal Society Proceedings* for June 19, 1879.

Leaving the great arc lights, therefore, to such a master of the subject, chemical, physical, and electrical, as the Jacksonian Professor in the University of Cambridge, I would request to be allowed to mention here a spectroscopic proof, which I have not seen mentioned before, that cyanogen is also formed in every induction electric spark worked under atmospheric pressure.

In plate 1 of M. Lecoq de Boisbaudran's admirable "Spectres Lumineux" he gives beautifully engraved views of the spectrum of the induction electric spark first at the positive pole, then at the negative pole with a "mean length" of spark, which was in his case probably about one inch; its extreme length with his induction coil and bichromate battery, in its best condition, being two inches.

Now the spectrum he gives for the positive pole is neither more nor less than the low temperature spectrum of nitrogen; that is as we see nitrogen in a gas-vacuum tube, with all its numerous and delicately shaded bands as such, though it is bioxide of nitrogen according to M. Thalén.

But the spectrum which M. Lecoq de Boisbaudran gives for the negative pole has in addition to the above, and besides the red hydrogen line, a number of other most distinct lines and bands, including one line in the violet, which he dignifies with the letter α, and which is certainly the grandest thing in the whole spectrum.

In his printed pages I do not find that the celebrated French spectroscopist gives any explanation of the origin of either that line or the other supernumeraries, the hydrogen line excepted. But on turning to my own paper on "Gaseous Spectra" printed in vol. xxx. of the *Transactions of the Royal Society, Edinburgh*, in 1881, I find on pp. 119 and 122, last column, that almost every one of the lines and bands which I had separated there from the impurities or dissociated elements of the tube's contents and had put down as due to the compound gas "cyanogen" is coincident in place and character with some one or other supernumerary in M. Lecoq de Boisbaudran's spectrum of the negative pole. My spectrum places are indeed very rough, owing to the small amount of dispersion then employed, viz. one simple prism of white flint with a refracting angle of 52°; but the testimony of the whole is cumulative, and, considering

Spark at the Negative Pole in the Open Air by M. Lecoq de Boisbaudran, with a rather Wide Slit

Colour Region.	W. N. Place approx. in Brit. inch.	Intensity approx.	Appearance approx.	Description.
Orange	41,300	2		Narrow band.
Citron	44,850	3		Stronger band with hazy line.
Green	48,600 49,300	4		Group of bands and hazy lines.
Green	50,100 50,800	3		Broad band with stronger edges.
Glaucous	53,800 54,700	4		Larger and stronger than the preceding.
Blue	55,200	2		Very thin line.
Violet	**59,400**	**8**		**Most powerful line, the α of the spectrum.**
Violet	59,500	5		A darkening of the nitrogen band.
Violet	59,900 60,400	5 5		Broad band, with strong terminal bars.

Cyanogen's Concluded Spectral Lines by C. Piazzi Smyth, with a rather Narrow Slit

Colour Region.	W. N. Place approx. in Brit. inch.	Intensity.	Appearance.	Description.	Reference page.
Orange	41,146 41,552	2 2		Cyanogen? Cyanogen	121
Citron	44,878	2		True cyan. group	120 & 121
Green	48,582	4		Sharp line begins a band of lines.	120
	49,350	3		Isolated line.	120 & 122
Green	49,996 50,728	2 3		Cyanogen. Do.	120 and 122
Glaucous.	53,963 54,570	3 2		Not nitrogen nor carbon. Cyanogen?	122
Blue	55,271	2		Cyanogen?	120
Violet	59,405	5		Grand line, followed by a band, characteristic of cyanogen.	120 & 122
Violet	59,985 60,356	2·0 0·2		Cyanogen. Cyanogen?	120 & 122
Violet	60,541	1·0		Nitrogen?	

the totally independent manner in which my results were arrived at, and the certainty with which they were stated on their own merits, perfectly overwhelming.

Thus—of the line which I now identify with that one which is

facile princeps in M. Lecoq de Boisbaudran's spectrum of the negative pole, and was therefore termed *a* by him, though to the confounding of his series of Greek letters in the positive pole's spectrum—I wrote of it in 1880 as "grand line peculiar to cyanogen," "the powerful violet line (viz. the above) at 59,405 W.N.B. inch, may become useful as a reference for place to many observers," and "grandly strong violet line, followed by a band; specially characteristic of cyanogen."

But a better view of the testimony of the whole case will be found in the above pair of tables, in the first of which I have collected, in a rude way of my own, all the lines and bands which are supernumerary in M. Lecoq de Boisbaudran's negative, as compared with his positive, pole; and in the second I have entered my former conclusions from gas-vacuum tube observations of what spectral lines and bands are peculiar to the compound gas cyanogen. C. PIAZZI SMYTH

15, Royal Terrace, Edinburgh, July 25

The Earliest Known Plotting Scale

THE Babylonian statues recently acquired for the Louvre by the mission of M. de Sarzec are of great interest in the history of measurement. The earliest datable measuring rods hitherto known are two Egyptian masons' cubits of wood, of the reign of Hor-em-heb in the fifteenth century B.C.; but on these statues we find represented not merely a mason's rod, but a finely-divided plotting scale, and the date of these figures is placed before the fifteenth century B.C. Of course the accurate lengths of cubits can easily be recovered from the dimensions of buildings of the earliest periods; but no measures, or accurate representations of such, are preserved to us from the primitive times.

There are several of these diorite statues of King Goudea in the Louvre, some rather less and some rather more than life size; all finely executed in a style superior to anything of the later times from Mesopotamia, with which we were already familiar. They are wrought by means of tubular drills and graving tools, by which lengthy and delicate inscriptions are cut all over the surfaces; the tools employed seem to have been very similar to those used by the early Egyptians for their statuary in diorite, which I recently described at the Anthropological Institute.

The statues which now concern us are two seated figures of an architect (or perhaps the king, as founder); these each bear on the knees a drawing board, 6·3 × 11·3 and 7·4 × 12·7 inches respectively. One board is plain, the other has an elaborate outline of a fortified town, showing all the buttresses and turns of the wall. By the right hand of each figure lies a drawing stylus, and along the front of each board a plotting scale, subdivided along both outer and inner face.

These scales have a sloping face along each side, like modern scales, but meeting in a ridge at the top, like French plotting scales, without a level space. The breadth is ·90, and height ·33 inch, sloping therefore about 36°; the length is just over 10½ inches, or half a cubit, the terminals being lines, with a small surplus beyond them.

The subdivisions vary on the different sides; but the general arrangement is a uniform series of spaces, which we will call digits; these are each $\frac{1}{16}$ of the half cubit, or ·653 inch. Then along one side of each rod the *alternate* digits are subdivided; thus there can be no confusion between digit lines and subdivisions. The dividing lines run the whole width of the face; they are about $\frac{1}{60}$ inch wide, and scored out nearly as deep into the diorite. The subdivisions are of halves, thirds, fourths, fifths, and sixths of a digit; and two sixths are carried over to the other side of the scale, and there further divided into twelfths and eighteenths of a digit; this last fraction being only $\frac{1}{28}$ of an inch.

By calculating a normal scale from the various digit lines (as described in "Inductive Metrology," p. 31) the average error of division may then be computed. It is about the same for the digits and also the subdivisions, varying on different sides from ·009 to ·013 inch; the mean error of all the digit marks is ·011 inch, or about half the breadth of a cut. But it is not to be expected that mere decorative representations like these would be divided with the same care as actual working scales. The mean value of the cubit deduced from these scales is 20·89 ± ·07 inch, which is apparently a long variant of the old 20·63 cubit, and not the later Assyrian cubit of 21·4 or 21·6.

The actual values of the divisions of the two sides of each scale are as follows, stating the amounts as differences from the normal scale in thousandths of an inch, which enables the variations to be most plainly seen. The points measured were about one-third from the bottom edge toward the top ridge.

Normal scale.		Without the plan.				With the plan.			
Digits.	Subdivisions	Outer.		Inner.		Outer.		Inner.	
		+	−	+	−	+	−	+	−
0		(. . .)		12		(. . .)		(. . .)	
·653			15		2	(. . .)		(. . .)	
1·306		(. . .)			20	(. . .)		(. . .)	
1·959		(. . .)			8	(. . .)		(. . .)	
2·612		(. . .)			26	(. . .)		(. . .)	
	$\frac{1}{2}$ 2·938	28		. . .		(. . .)		(. . .)	
3·265			9	2		12		11	
3·917			3	2		. . .		25	
	$\frac{1}{2}$ 4·244	. . .			9	. . .		18	
4·570		(. . .)		. . .		. . .		7	
5·223		(. . .)			8	. . .			11?
	$\frac{1}{3}$ 5·441	. . .		2		. . .		(. . .)	
	$\frac{1}{3}$ 5·658	. . .		4		. . .			1
5·876		(. . .)		4		. . .			1
6·529		(. . .)			5	. . .		8	
	$\frac{1}{4}$ 6·692	. . .		21		. . .		6	
	$\frac{1}{4}$ 6·855	. . .		25		. . .		3	
	$\frac{1}{4}$ 7·018	. . .		18		. . .			1
7·182		7		58			8		10
7·835		1		4		. . .		(. . .)	
	$\frac{1}{6}$ 7·966	. . .		3		. . .		8?	
	$\frac{1}{6}$ 8·096	. . .		2		. . .			7
	$\frac{1}{6}$ 8·227	. . .			3	. . .			13
	$\frac{1}{6}$ 8·357	. . .			5	. . .			22
8·488		11		0		. . .			7
	$\frac{1}{2}$ 8·814	. . .		. . .			25	. . .	
9·141			7		12		6		2
	$\frac{1}{6}$ 9·249	. . .			7	. . .			4
	$\frac{1}{6}$ 9·358	. . .			27	. . .			4
	$\frac{1}{6}$ 9·467		14		11	. . .			8
	$\frac{1}{18}$ 9·503		13	. . .		. . .		. . .	
	$\frac{1}{18}$ 9·539		11	. . .		. . .		. . .	
	$\frac{1}{18}$ 9·576		6	0		. . .			19
	$\frac{1}{6}$ 9·685	4		20		. . .			12
	$\frac{1}{12}$ 9·739	14		. . .		. . .		. . .	
9·793		19		20		(. . .)			21
10·446			5		24	(. . .)		(. . .)	

The plain dots show that there was no mark; the dots in brackets where a mark is defaced, or the whole surface destroyed. The great error of ·058 inch is due to a cut run askew, the line being as accurate as the others on the outer face of the rod.

I am indebted to M. Ledrain for kindly granting me permission to take the measurements from these statues.

Bromley, Kent W. M. FLINDERS PETRIE

A Result of our Testimonial System

A LITTLE incident has come under my notice of such a character that I think it ought to be made known to the readers of NATURE.

A candidate, whom I will call Mr. A. B., for a vacant scientific chair in this country writes to an eminent German professor for a "testimonial," and in his letter there occurs the following remarkable sentence:—

". . . 17 *July*, '83

"DEAR SIR,—I intend applying for the vacant chair of . . . at . . ., and would feel grateful if you could send me a testimonial saying a few favourable things of my contributions to the science of . . .

". . . I hope that you will not think me too bold in asking this request, and as I know your time is too valuable to be trespassed on by a stranger, *I beg that you will accept the inclosed.*"

The German professor, whom I will call Prof. C., thereupon writes to a distinguished English professor, who is a personal friend of his, the following letter, which has been placed in my hands with the request that I will add a few comments. The letter, which I give in its original language in order that none of its force may be lost, runs as follows:—

". . . 23 *Juli*, 1883

"Verehrter Herr College,—Ihre freundliche Gesinnung gegen mich, ermuthigt mich, Ihnen folgenden Fall vorzutragen, mit der Bitte möglichst viele Ihrer Herrn Collegen und, wenn Sie es für gut halten, auch die Presse davon in Kenntniss zu setzen.

"Ich hatte schon öfter aus England Briefe erhalten von Candidate für irgendwelche . . . Professur mit der Bitte ein Zengniss ueber ihre Leistungen anzustellen. Ich habe, da mir diese Art der Bewerbung, wie sie in England leider gebräuchlich ist, im höchsten Grade zuwider, meist derartige Schreiben gar nicht beantwortet. Neulich erhielt ich nun aber einen Brief aus . . . von einem gewissen . . . der an Schamlosigkeit Alles uebersteigt, zum Mittel der Bestechung greift. Es klingt unglaublich, aber Herr . . . ist so schamlos, mir als Preis für ein Empfehlungschreiben *Geld anzubieten*. Damit Sie sich selbst davon ueberzeugen können, sende ich Ihnen das Original mit der ergebensten Bitte mir dasselbe nach gewonnener Einsicht bezw. Abschrift, wieder zurückzusenden. Eingelegt war eine Anweisung auf 1 guinea! Letztere sende ich heute ohne Brief recommandirt an . . . zurück. Ich habe Beider hier meinen Freunden gezeigt und werde auch vor Zeugen die Rücksendung der Anweisung auf 1 Guinea vornehmen.

"Ich glaube, verehrter Herr College, dieser Fall ist dazu angethan, weiteren Kreisen mitgetheilt zu werden, um zu verhindern dass ein solch erbärmlicher Mensch wie . . . etwas die Stelle in . . . erhalte. Ihnen im voraus für Ihre Mühe dankend mit vorzüglichste Hochachtung.

"Ihr Ergebenster, . . ."

I imagine that all Englishmen on reading the above will, like myself, be filled with shame that any one speaking our tongue should have laid himself open to such a rebuke.

At the same time it seems to me quite possible that Prof. C.'s view of the matter is unduly severe and indeed unjust. I do not know Mr. A. B. personally, and am quite ignorant of what character he bears; but I can conceive that he has fallen into this disgrace through a clumsy attempt to carry out to its logical conclusion our English system of testimonials. He can hardly have thought that so distinguished and successful a man as Prof. C. could be *bribed* to say something handsome by a post-office order for *one guinea*; and he cannot be so ignorant as not to be aware of the just pride which all Germans feel in the integrity and honour of their professoriate; it is quite open for us to suppose that he was really offering Prof. C. *a fee for a professional service*. And really when you come to think of it, this is a point of view for which something may be said. Only last week, in talking to a colleague about testimonials, I asked him how many testimonials he wrote on an average a week. He replied that he thought *not more than a dozen or fifteen*. In fact when a man, especially one who has spent some years in teaching, has acquired a certain reputation in science, the tax upon his time and energy for the skilful composition and writing of appropriate testimonials amounts during his lifetime to a something which, converted at the market value of his powers into pounds, shillings, and pence, would appear no mean sum.

Now—and this is the kernel of the matter—no one would grudge time spent in assisting a deserving man to get into a place for which he was fitted; but our testimonial system has nowadays reached such dimensions that only a few of the testimonials written have this end in view. I am writing freely, because this is a very serious matter, and one which I have much at heart; I therefore do not hesitate to say, what indeed is well known, that great skill has been reached by many in the art both of writing and reading testimonials. Many testimonials are framed after that well-known formula for acknowledging the receipt of pamphlets which runs as follows:—"Dear Sir,—I beg to thank you for the valuable pamphlet which you have so kindly sent me, and which I will lose no time in reading." And I heard the other day a testimonial praised because it showed the electors whom not to elect.

Surely the time has come to consider whether this plague of testimonials (for it is hardly less) cannot in some measure be stayed. At all events, cannot in higher places at least some steps be taken to mend matters? When such a post as a professorship is vacant, it is the duty of the electors to make themselves acquainted with the manner of man wanted and to find him; our present plan lays upon all persons connected with the subject of the chair the burden of trying to enlighten the electors as to the claims of this or that candidate. A passage in Prof. C.'s letter shows how degrading the Germans think our method; and it is not agreeable to Englishmen to read such passages. Yet every one who has had to struggle for a post with testimonials must feel that such criticisms are just, and that the process is one distasteful to a right-minded man. And it is also unnecessary. I, for one, would rejoice to see the German system of a "call" introduced into our professorial elections; but if we cannot obtain this, let us at least do away with testimonials. In the recent elections at the University of Cambridge, the following significant phrase occurred in the announcements of the vacancies: "testimonials, *if any*, to be addressed, &c."; and as a matter of fact, in the cases of the four chairs recently filled up on the new system, the man chosen in each case had sent in no testimonials. Why cannot this be done in all elections to professorial chairs? Where, as may sometimes be the case, the candidates are previously not all thoroughly known, the electors, by reference, formal or otherwise, can easily make themselves acquainted with their relative merits; and indeed, as I just now said, it is their duty to make such inquiries, and not simply to collate, interpret, and form their decisions on the curious documents which we call testimonials.

Hence, though I venture to send this communication to NATURE for the purpose of making an example of Mr. A. B.'s post-office order for one guinea, I cannot help thinking that he, though sinning, is also sinned against, and that our system of testimonials is to be blamed as well as he. M. FOSTER

Birds and Cholera

YOU ask in one of your "Notes" (p. 329), what can be the cause of birds leaving a locality before the approach of cholera? The following anecdote may be of interest, but I of course cannot vouch for its having any real connection with the subject. It must have been in the summer of 1848 that I was invited to meet a party at my uncle's house in the Close at Salisbury, on the occasion of the visit of the Antiquarian Society. On arriving I found the cholera raging, and the party put off. There were in the house only the gardener and his wife, whom, having been previously servants to my father, I had known from my childhood. The gardener told me that, just before the outbreak of the disease, the man whose duty it was to oil the vane upon the spire had made his annual ascent (of 404 feet), and had perceived a foul scent, which, it seems, had not been noticed below. The inhabitants connected this with the appearance of the epidemic shortly afterwards. Birds might no doubt be affected by such a circumstance. O. FISHER

THIS has been remarked before. It is recorded of the great outbreak of cholera at Salisbury in 1849—can any of your correspondents say where?—that an officer recently from India, happening to make the ascent of the Cathedral, exclaimed suddenly, "I smell cholera!" Immediately afterwards the outbreak followed, when it was observed that the birds (swallows are especially in my remembrance) had fled the neighbourhood. If these two incidents are to be trusted, it can scarcely be doubtful that there is a connection between them. HENRY CECIL

Bregner, Bournemouth, August 6

YOU will find a very interesting but rather sceptical paper on the supposed connection of birds leaving towns with invasions of cholera (NATURE, vol. xxviii. p. 329), by Pfarrer Häckel of Windsheim, in the monthly journal, *Der zoologische Garten* (Bavaria), September, 1873 (vol. xiv. p. 328), published by the Zool. Gesellschaft of Frankfort-on-Main. D. WN.

Freiburg, Badenia, August 4

Animal Intelligence

SEVERAL remarkable instances of intelligence in animals have been given in recent numbers of NATURE. Possibly the following instance of reasoning power in an elephant may not be without interest:—Some years ago I was ascending the lower part of the Darjeeling Hill Road, in the Himalaya Mountains, from Terai. At a certain part of the road, where we met a string of bullock carts, the outer few feet was encumbered by a long flat-topped heap of small rounded boulders, piled there to be broken up for road metal; from the outer edge there was a steep, almost precipitous, slope. On the inner side of the road was a small drain, and then a few feet of comparatively level ground between the drain and the slope above. The carts just mentioned were of the usual kind, the body (constructed of bamboo) about

12 feet long and 3½ feet broad, with the wheels near the middle, each cart being drawn by a pair of bullocks. The *mahaut* (driver) of the elephant I was riding having halted the animal close up to the heap of boulders, there was just room left between the elephant and the chain for the carts to pass. These carts were the ordinary vehicles of the country, and under ordinary circumstances an elephant would no more think of "shying" at them than a London dray horse would think of shying at a cab. Yet as the carts went by one by one my elephant became more and more uneasy, and finally, in spite of the efforts of the *mahaut* to restrain her, mounted on the heap of boulders, at the risk (which, considering how cautious elephants are in treading on suspicious ground, I believe she must have seen quite as clearly as the *mahaut* or I) of rolling down the slope below the road, if the rounded boulders shifted and gave way beneath her weight. It was some time before I perceived the cause of her fear. Elephants, even in India, are uncommon, and bullocks, as well as other domestic animals, generally feel considerable dread of them from their unusual appearance as well as their size. The bullocks in question were greatly frightened at having to pass so close to the bulky brute, and several of them in passing tried to get away from her by jumping the drain. It required all the efforts of the drivers to prevent their doing it. The elephant evidently saw that the bullocks were frightened and that they were trying to jump the drain, and she further calculated that if they did so *the long tail of the cart would swing sharply round in the opposite direction* and strike her violently across the fore legs. Of the two risks she preferred that of mounting on the heap of boulders.

F. R. MALLET

Calcutta, July

AS NATURE frequently contains notices of intelligence in animals, I have ventured to send you the inclosed note from the Reading local paper, as containing a remarkable fact regarding intelligence in a blind horse. The writer, Mr. Gostage, is quite trustworthy, and I have taken pains to verify the truth of his statements.

JOSEPH STEVENS

128, Oxford Road, Reading, August 6

NOTE PUBLISHED IN THE *Reading Observer* OF AUGUST 4, 1883

Sagacity of the Horse

SIR,—A circumstance so fully illustrative of the sagacity of the horse was witnessed in the neighbourhood of Mortimer last Saturday, and reported to me through the owner, that I think it worth publicity. I can vouch for its truthfulness, and if any doubt arises I can introduce such doubter to the owner. The hor e under notice, an old blind one, belonging to a small tradesman and farmer, was turned out to graze on the common near the owner's house. For some cause it wound its way through lanes to the blacksmith's, where he had often been before. The entrance to the forge is difficult of access on account of the ditches on either side, but the animal reached it safely, took its stand by the forge, and then neighed. The blacksmith, being at work in his garden, and hearing a horse neigh, looked for it, and not seeing it, returned to his gardening operations. In a short time he heard it again, but could not see a horse anywhere, until he went into his shop, when he found it standing very quietly by the forge as if waiting to be shod. Thinking some one must have brought it there, the blacksmith looked at its feet, and found one with the shoe pressing into the frog, causing great pain. He then put on another shoe, and sent the horse back to its owner.

This instance of sagacity is so clear and telling that I thought it desirable to ask you, Mr. Editor, to publish it.

Yours truly,
S. GOSTAGE

King's Street, Reading, August, 1883

ACCOUNTS are not rare of female cats having adopted the young of other creatures when deprived of their own, or while nursing their own young, but I have never met with a case like the following:—

My tom cat, Smut, whose eighteenth birthday was lately celebrated, has always been kind to kittens; and a long friendship with a tame rabbit was only terminated by the death of the rabbit in consequence of eating too much plum pudding one Christmas. But his benevolence to feathered creatures was first shown in 1881, when, having a solitary chick hatched out of a clutch, I bethought me of making him useful as nurse, and with some fear put the chick into his basket. The experiment answered admirably, except that Smut sometimes licked the feathers the wrong way; and when about a fortnight afterwards the chicken was accidentally killed, it was curious to see its foster-father's search for it during the following three or four days.

Since then Smut has taken charge of as many as fifteen young chickens at a time, but he has never evinced the same affection for them as for his first feathered foster-child.

J. DE B. F. P.

The Orphange, Wandsworth Road, August 7

Different Sources of Illumination

IN your issue of July 19 you give in the "Notes" (p. 281) some interesting data as to the products of combustion and heat produced by different sources of illumination, each being of 100 candle-power and giving off this light for one hour. This is valuable information, and I am sure that others besides myself would be glad if you could give a reference to the authority. I would also suggest that it would be interesting to have a comparative authoritative statement as to the carbonic acid and heat produced in the same time by an average human being. I was told the other day by a mining engineer that he finds that one oil-lamp contaminates the air to the same extent as one miner when at work. It is often stated that one gas-burner in a theatre is as deleterious as six members of the audience. If the true state of the case were published in your columns, it would be interesting to many.

GEORGE FORBES

34, Great George Street, Westminster, July 20

[The information is based on an article in *La Lumière Électrique* for June 16.—ED.]

A Remarkable Form of Cloud

AN account, which will I believe be found satisfactory, of the formation of the type of cloud described in NATURE (vol. xxviii. pp. 299, 320), will be found in a paper read by me before the Meteorological Society on June 20 last, and which will be published in the next *Quarterly Journal* of the Society. The paper is on "The Structure of Cirro-filum, or Ice-cloud disposed in Threads." A very valuable contribution to our knowledge on this subject will also be found in an article by Dr. Linn ("Ueber die Entstehung der Wolkenstreifen," *Zeitschrift für Meteorologie*, xviii. 52), to which I would refer those of your readers who are interested in the topic.

The cloud is very common, and regular reports of the direction both of movement and of "filature," elements of very considerable value in the prognosis of weather, have been, for some years past, sent to the Meteorological Office by a limited number of observers.

W. CLEMENT LEY

Disease of Potatoes

WHEN I read the note from *Naturen* in NATURE, vol. xxviii. p. 281, it appeared to me that Herr Anda was describing the same effects in the potato stalk as had been described by Berkeley in 1846. In his description of the usual potato disease Berkeley says:—"The stem now rapidly putrefies, the cuticle and its subjacent tissue become pulpy, and separate when touched from the woody parts beneath. The whole soon dries up, and in many instances exhibits in the centre the black, irregular fungoid masses which are known under the name of *Sclerotium varium*, and which are believed to be the mycelium of certain moulds in a high state of condensation."

Now the *Sclerotium varium* grows exactly as described by Herr Anda; but so far as it has appeared here, it does not seem to be truly parasitical, but only begins to be developed on the potato stalks when they are dying down of the common disease. Whether this *Sclerotium* is the same as that referred to by Mr. W. G. Smith (NATURE, vol. xxviii. p. 299) I do not know, but probably it is. He says he did not get his to germinate; while Herr Anda describes the fruit of the *Sclerotia* found at Stavanger.

From "pink eye" potato stalks of last year I threshed out a quarter of a pound of *Sclerotium varium*, and at the present time I have hundreds of specimens germinating in the way Herr Anda describes; one stalk only has yet come to what I regard as the perfect fructification, having developed at the apex a beautiful little cup; but about a score of others of those first

laid on wet cloth are beginning to give distinct evidence of the production of cups. The probability at present is that *S. varium* is the Sclerotium of a Peziza, nearly allied to *Peziza tuberosa*.

A. STEPHEN WILSON

North Kinmundy, Aberdeen, July 30

P.S.—Since the above was written I have discovered amongst growing potatoes great numbers of *S. varium* with the completed fungus attached to them. It is a yellowish-brown Peziza of various diameters up to half an inch. I send you a box of specimens.—A. S. W.

"Zoology at the Fisheries Exhibition"

IN NATURE, vol. xxviii. p. 289, is an article upon the zoology of the Fisheries Exhibition, in which the writer states that some of the corals exhibited by Lady Brassey belong to me and are not that lady's property. Will you permit me to emphatically assert that not a single coral in the case belongs or ever did belong to myself, and that every specimen was procured by Lady Brassey during her voyages in the *Sunbeam*.

What is meant by the words "gratuitous inventions" I cannot understand ; the new species were carefully compared with those in the British Museum, also with those obtained during the *Challenger* expedition, and with the works of Lamarck, Dana, Milne-Edwards, Moseley, and others.

It is possible that the commissionaire in charge may have, in dusting the collection, shifted some of the labels, but the fact remains that Lady Brassey's collection of corals is the only one in the Exhibition which gives any information either upon the nomenclature or habitat of the specimens exhibited.

204, Regent Street, W., August 4 BRYCE-WRIGHT

"The Student's Mechanics"

I HAVE no wish to quarrel with the review you have printed of my book, "The Student's Mechanics ;" and I have to thank the reviewer for drawing attention to one omission, namely, the failure to explain fully the second law of motion, as related to the two methods of measuring force. But I should be glad to be allowed a few words to explain my treatment of Accelerating and Moving force. One of my objects was to clear away, by full explanation, the confusion which no doubt sometimes exists as to those terms ; and this I could not have done if I had omitted them altogether. It will be long before a reader of works on mechanics can safely remain ignorant of their meaning ; and indeed the discussions of force as causing change of velocity simply (as in kinematics), and as causing change of momentum, are still kept so much apart that terms to indicate the distinction do not seem out of place. Nor do I see any confusion likely to arise between "acceleration" and "accelerating force" : the one is the actual change of velocity in a given time, the other is the force which causes that change. The latter is measured by the former, but it is not the same thing. In Art. 422 the word "accelerating" is simply used in opposition to "retarding," in the sense of that which increases velocity instead of diminishing it : I know no other word in use for the same purpose. Lastly, the proof in Art. 359 was given precisely to supply the omission to which your reviewer calls attention, and which does exist in the ordinary proofs that no velocity is lost in passing round a smooth curve. I there show that the sum of such losses, in a given time, is indefinitely small compared with the sum of another set of quantities, which sum is itself finite ; hence the first sum may properly be neglected. WALTER R. BROWNE

Sand

As explained in my note on p. 245, I had not the advantage of perusing Mr. Waller's paper on "Sand." Mr. Gardner, in his notice of it gave the first place to "distinguishing with certainty by the aid of the microscope sand that has been worn by the action of wind from sand that has been for long exposed to surf, and this again from sand brought down from torrents." I assumed this was its primary object. In this I am in error. Mr. Waller says his "paper was to show that chalk flints had scarcely any place in the formation of sand." Had I known this was the purpose of his writing I would not have troubled you with any remarks, as I entirely agree with Mr. Gardner when he says, as in p. 225 : "The coast-line occupied by flint shingle is almost limited to portions of Western Europe, and is relatively insignificant."

I am glad to learn that Mr. Waller has a more comprehensive object in view, and that a large series of sands from modern and ancient formations are being examined microscopically, and shall be glad to supply portions of specimens of the soils and subsoils of Australia and New Zealand which contain sand, and were examined under the microscope ten years ago, to compare their form and appearance with similarly situated soils from Europe. JAMES MELVIN

Treble Primary Rainbow

ON Sunday, July 15, as a heavy thunderstorm was passing away from over this place, a brilliant rainbow appeared a little to the south of east about 5.45 p.m. There was a complete primary arch and a nearly perfect secondary one, and on being led to examine the former in consequence of its appearing unusually broad, it appeared to be made up of three bows, one directly below the other. The red of the spectrum being repeated three times was what drew my attention to this point. The two lower bows appeared smaller than the top primary arch. Thinking I must be suffering from some optical illusion, I got my wife, brother, and my little girl of nine, all to look carefully at the rainbow, and found that they all saw three distinct bows in the primary arch, in addition to the secondary arch. Is not this an unusual occurrence ? R.

Bexley, Kent, July 21

[This is merely the well-known phenomenon called *spurious bows*, which has not yet found its way into the "popular" class of text-books, though the principles of its explanation were long ago pointed out by Young. The full theory was given by Airy, and found to coincide with the very exact measurements of Hallowes Miller. When the raindrops are all of the same size, each wave-length in the rainbow has one principal maximum with an infinite number of subsidiary maxima of rapidly-decreasing brightness. These lie *inside* the chief maximum in the *primary* rainbow, and outside it in the secondary.—ED.]

FUEGIAN ETHNOLOGY

IN Guido Cora's *Cosmos* for May, 1883, Lieut. Bove, of the Italian Antarctic Expedition, supplies some interesting details on the little known inhabitants of Tierra del Fuego, amongst whom he spent some time in the spring of the present year. He speaks highly of the English missionaries stationed at Ushiwaya, in Beagle Channel, who have succeeded in introducing a few rudimentary notions of human culture amongst several tribes hitherto supposed to be quite irreclaimable. As had long been suspected, the archipelago is found to be occupied not by one but by three distinct races, the Alacalufs in the west, the Onas in the east, and the Yagans in the south. Of these the Yagans, who stretch from the north side of Beagle Channel southwards to Cape Horn, appear to be the true aborigines. They have been driven to the southernmost and most inhospitable islands by the Onas and Alacalufs, both intruding from the mainland. The Onas, who are clearly of Tehuelche origin, penetrated from Patagonia across the eastern arm of Magellan Strait, into the large island of King Charles South Land (Eastern Tierra del Fuego), which they now hold almost exclusively. In the same way the Alacalufs, of Araucanian stock, made their way from the Chilian Andes, across the western arm of Magellan Strait, into the western islands, which they now occupy from Cape Pillar to Stewart Island, at the Pacific entrance of Beagle Channel. They number scarcely more than 2000 altogether, while the Yagans and Alacalufs are estimated by the English missionaries at about 3000 each, giving 8000 for the whole archipelago.

Although now representing the most aboriginal element, the Yagans themselves would appear to belong originally to the same Chilian family as the Alacalufs, the points of difference being easily explained by their longer isolation from the parent stock and by the more unfavourable climatic conditions of their present homes. From numerous measurements taken by Bove, they seem to be much below the middle height, although still nearly as tall as the Araucanians of the mainland. Of these the

average stature, according to D'Orbigny, is 5 feet 3 inches, while the Yagans range from 4 feet 10 inches to 5 feet 4 inches, and the women from 4 feet 9 inches to 5 feet. But in other respects they present a more debased appearance than their continental congeners, being distinguished by low brows, prominent zygomatic arches, large pendent lips, flat nose, round face, loose, wrinkly skin (' pelle grinzosa e cadente"), thin extremities, the legs curved outwards. The black hair is of the usual American texture, coarse, lank, and long, but in one district chestnut and wavy, due, no doubt, to mixture with white blood.[1]

They neither tattoo nor paint the body, which is exposed almost naked to the inclemency of an excessively rigorous and stormy climate. In this respect the Fuegians present a striking contrast to the Eskimo at the opposite extremity of the continent, the general cut of whose warm and comfortable attire may, according to Mr. E. B. Tylor, be due to the influence of the old Norse settlements in Greenland. Although Bove gives us two distinct terms, *accar* and *tuma-chi* for *house* and *hut* respectively, the dwellings themselves are all alike described as wretched hovels, made of branches stuck in the ground and loosely bound together in the Botocudo fashion. More skill and care is displayed in the construction of their beechwood canoes, which are generally from fifteen to twenty feet long and about two feet wide. In these frail craft they navigate the intricate channels of their storm-swept waters, and boldly pursue the whale and dolphin often far out on the high seas beyond sight of land ("spesso fuori dalla vista d'ogni terra"). Here, however, it may be well to remember that similar statements were constantly made of the Andaman islanders until Mr. Mann recently showed that in their light outriggers they never venture far from the shore.

Like the Araucanians the Yagans are polygamists, and, like the followers of the Prophet, they have generally four wives. But, while the Araucanians purchase their mates,[2] the Fuegian bride is provided with a dowry consisting usually of a canoe and a few harpoons. Nevertheless all the hard work, such as fishing, hutbuilding, the kindling and preservation of fire, falls to the share of the women, who in return meet with nothing but the most brutal treatment from their helpmates. "How often," writes Bove, "have I seen men seated cosily round a good fire, while the wretched women remained exposed to the snow, wind, water, fishing for their idle and unmannerly husbands!" Notwithstanding their hard lot the women are exceptionally fruitful; but, on the other hand, a small percentage only of the children resist the severity of the climate. They leave the paternal roof at a very early age, and begin to shift for themselves *before* reaching their teens. In fact family ties can scarcely be said to exist, and the only affection of which the Fuegian seems capable is "self love." "How often," again remarks the Italian explorer, "have I seen the father devouring a hunch of meat or bread, while round him stood wives and children, their eyes riveted on the food, with features distorted by hunger, rendered all the more painful at sight of others being sated, timidly gathering the scraps dropping from his lips, and falling rabidly on the remnants of the feast contemptuously thrown to them by the ferocious head of the household!"

Each family circle lives apart in absolute independence, combining only in small tribal groups for the purpose of mutual defence against some common enemy. Thus it is that the first germs of the community are sown by the necessity of self-preservation, just as the fully organised society is still kept together by the same overruling principle. But in the Fuegian community the idea of headship has not yet been evolved. No one claims the right to assume the chieftaincy, or to meddle in the concerns of his neighbour. Hunting or warlike excursions are arranged by common consent, and the spoils of war or the chase are equally distributed amongst the members of the expedition. Certainly the Fuegian social system seems to favour the views of those, rather, who hold that everywhere the commonwealth preceded oligarchy and the monarchy. As the monotheistic conception was arrived at through pantheism and polytheism, so in the social order the autocrat appears as the final outcome of a rude communism and πολυκοιρανία.

The Yagans, however, seem to have scarcely reached the pantheistic, or perhaps it would be more correct to say the pananthropomorphic, state. Religious notions, in the strict sense, cannot be said to exist where no clear distinction has yet been drawn between the natural and supernatural. Even with superstitious ideas they are but little troubled ("sono pochissimo superstiziosi"), while their indifference to the remains of the dead would seem to imply that they have no anticipations of an after life. To the naturalists of the Italian expedition they freely parted with the crania of fathers, friends, and relations, without the least outward symptoms of regret. In one instance, however, a good deal of sentiment was expressed by a young Yagan, who thus somewhat poetically addressed the skull of his father: "Farewell, dear father. You, who when alive never saw aught but our snows and our storms, are now going dead far far away!" This is the language of one, in whom at least dim visions of another existence seem to be dawning.

Considering the extremely low state of their culture, it requires a considerable degree of credulity to accept the statement that their agglutinative language possesses some 30,000 words, besides highly complex and elevated grammatical forms ("ha circa 30,000 vocaboli, e forme grammaticali molto complesse e elevate"). This is naturally regarded as a sure proof that the Yagans have had a much higher origin than might appear from their present debased condition. But it will be safer to await further proof before accepting the statement at all. Reserve is the more needed that we are told somewhat mysteriously that this linguistic phenomenon *was very little studied* by the explorers ("femoneno notato, quantunque pochissimo studiato, dei nostri esploratori.") It is also curious that, with such a copious vocabulary, of which a few specimens are given, the same word *yash* should have to do duty both for *hand* and *finger*, as well as for *head*, this last, however, doubtless as a homophone, or else through one of those mistakes which cannot always be avoided even by *careful* students of barbarous languages. The numerals do not seem to get beyond *five* (cu-pash-pa, an obvious compound), which is again somewhat inconsistent with a vocabulary of 30,000 words! But we may soon expect further light to be thrown upon this point by the English missionaries, who are doing such excellent work among the Yagans of Beagle Channel, and whose labours will doubtless soon be extended to the whole of the Fuegian Archipelago. A. H. KEANE

[1] With this description may be compared that of the fourteen Araucanians now encamped in the Jardin des Plantes, Paris, and figured in the *Illustration* of July 28, 1883. The low brow, high cheek-bone, flat nose, lank hair, and general flat features give to both races a common Mongoloid expression, such as is distinctly seen in the Guarani, Tupi, Botocudos, and so many other South American peoples. This expression seems in fact almost more pronounced in the southern than in the northern races of the New World, and it is certainly remarkable that the physical appearance of the Araucanians and Fuegians should be even more suggestive of an Asiatic origin than is that of the Eskimo and Athabascan groups.

[2] "L'Araucanien peut prendre autant de femmes qu'il en peut nourrir et payer aux parents, car les femmes s'achètent."—*L'Illustration*, July 28, 1883.

THE ISCHIAN EARTHQUAKE

THE report from the Central Observatory, by Prof. de Rossi of Rome, shows that signs of the coming catastrophe were not wanting at the different meteorological stations. What follows is, according to the *Daily News* correspondent, the most interesting part of Prof. de Rossi's report.

"Several days before the 25th and 28th July the micro-

cosmical instruments at Rocca di Papa and connected microphones in Rome showed a great increase in subterranean activity. The earthquake which took place at Cosenza and Catanzaro on July 25 seemed to be the one predicted by those movements; but their continuance and increasing force showed clearly the approach of a new dynamic effort. Science, however, cannot yet determine the topographical point menaced by such effort, because we have not a sufficient number of observatories, and they are especially wanting in the places where the manifestations of the subterranean forces are most to be feared. Thus we could only suspect the direction of the movement, and gather from the daily observations made here and there in Italy that the seismic activity has concentrated in the southern part of the peninsula. The earthquake of Saturday, July 28, was registered by the seismographs in Rome, Velletri, and Ceccano at 9.30 p.m., with slow waves from north to south and east to west. The other instruments, which register quick and abrupt movements of the ground, remained quiet. As far as can be gathered from the observations till now made, this earthquake was an exact but more extensive repetition of that of March 4, 1881, and those just preceding, confirming the previsions and data collected at that time. It is deplorable that my advice respecting the institution of regular observations in those parts was not followed, as such observations would certainly have given warning of the imminent catastrophe. I gave that advice not only immediately after the catastrophe of March 4, but also on my visit to Naples at the Meteorological Congress. In consequence of that visit I wrote in the name of the Observatory to the director of one of the chief baths in Ischia, begging him at least to undertake daily note of the temperature of the thermal waters and the state of the fumarole (natural apertures from which issue smoke and steam). Alteration in the temperature of thermal-mineral springs, when that alteration exceeds certain limits, is one of the surest signs of a subterranean storm, and such alteration has always been noticed at Casamicciola, even without regular scientific observations. This time, as often before, the drying up of wells, subterranean thunder, and slight oscillations of the earth, have preceded the catastrophe, which shows what valuable indications might have been afforded by the delicate seismic instruments, the microphones, and telephones now at our command. The reluctance shown to follow my advice arose purely from a selfish fear lest the establishment of a meteorological observatory at Casamicciola should give an appearance of danger, and frighten visitors away. This false idea is so prevalent in the minds of even educated persons in the place that notices of the occurrence of small phenomena during the last few years have often reached me with great reserve, and very late. Let us hope that such a prejudice will not long continue to the damage of science."

The actual moment of the explosion has, according to the correspondent of the *Standard*, been variously stated. The clock in the Sala Belliazzi stopped at twenty-two minutes past nine, but it is generally agreed that the real time was fifteen or twenty minutes later—a singular detail, which has not been generally noticed.

Shocks of earthquake are reported to have occurred daily in Ischia since the 28th ult.

Prof. Palmieri states that all the later shocks felt at Ischia have been registered instantaneously by the seismographic instruments at the Observatory on Mount Vesuvius. On Friday morning, according to the Naples correspondent of the *Standard*, the instruments showed signs of considerable subterranean disturbance. Vesuvius was rather active, but the fear that a fresh crater was about to open immediately above Torre del Greco appears to be unfounded. Neapolitan passengers returning from Ischia appeared delighted to see Vesuvius blazing away in the distance. "Oh," said they, "so much the better; that may, perhaps, be a safety valve." Rossi and other observers, who differ from Palmieri on this point, predict nothing less than the reopening of the old crater of Monte Epomeo or the opening of a new one.

In making the tour of the hospitals on Wednesday, in order to collect the narratives of the wounded, the *Standard* correspondent found strong confirmation of the fact that there were signs of danger two or three days beforehand, which cannot have escaped the observation of the inhabitants of the island; but they were, unfortunately, he states, suppressed, in order to avoid giving alarm to the visitors, and so spoiling an unusually prosperous season. The Advocate Jeremiah Tonti, of Antria, Bari, who lies badly hurt in the Church of the Pellegrini, adjoining the great hospital, related to the correspondent that he had gone there with his wife to take the baths for rheumatism. The spring used comes forth from the ground so hot that it is necessary to temper it for the bath with one-fourth of cold water, but two days before the disaster the temperature of the spring rose so suddenly that it was found impossible to enter the bath until the supply of cold water had been largely increased. Dr. Dominico Bucco, who lies at the Hospital of the Pellegrini, says that the shocks at Forio and Lacco Ameno were vertical as well as undulatory, so that the floors of the houses fell in one upon another from garret to cellar, sometimes still leaving the outer walls standing. He was also conscious of a momentary whirling motion, as if being drawn into the vortex of a whirlpool.

A telegram from Athens states that a strong shock was felt at Piræus on Saturday last.

WE have received the following communication from Dr. Johnston-Lavis, of Naples:—

The island of Ischia is but too well known from the earliest historic times for the prevalence of earthquakes and even volcanic eruptions. In 1827 a shock destroyed the greater part of Casamicciola, some portion of Lacco Ameno, and injured Fontana Serrara, besides shaking severely Barano and Forio. On March 4, 1881, a quite similar shock to the former brought down a large number of houses and severely injured the rest. The present one occurred at about 9.30 on Saturday, July 28, and resulted in the absolute and total destruction of the whole town, most of Lacco Ameno, and a large part of Forio, Fontana, and Barano.

One remarkable fact is that the exact detailed area has been similarly affected in each case, so that the description of the earthquake of 1827 by Covelli, that of 1881 by myself, and the present would be much the same; the only difference being in intensity. The earliest killed under 50 people, the second 127, and the present will carry the number near a thousand.[1] The large increase of the deaths of the present one is due to its occurrence at the culminant point of the bathing season, so that the hotels were crowded with visitors; and the hour also found many of the peasantry going to bed.

The earthquake of two years since only ruined the worse built houses, and fissured the better ones, which were replastered and patched, so that the present shock has reduced every one to a heap of stones and mortar.

The shock was estimated to have lasted fifteen seconds, but a number of inquiries I have made as to what different persons had done to escape, and how the time was occupied between the first and last sensation, and the distance traversed during the movement, makes me believe thirty seconds nearer the point; for instance, one man awoke, jumped out of bed, stumbled over some furniture, opened the door, descended a flight of twenty steps, and when in the courtyard below still felt the movement.

The sound is said to have resembled a report followed

[1] Later information makes it near 5000.—ED.

by boob, boob -, boob —, boob ——, boob ———, boob ————, and so on.

In the short notice of the earthquake of March 4, 1881, I pointed out that the centre of the mesoseismal area or seismic vertical was at Casamenella, which occupies the same relative position to Epomeo as do Montagnone, Mount Rotaro, Cremate, and many other lateral cones. Also that this earthquake belongs to that subterranean class of movements that precede the bursting forth of an eruption such as the Vesuvian shock of A.D. 63, and the series that gave warning for some years before of the appearance of Monte Nuovo.

Prof. Samuel Haughton and myself are still engaged on a memoir of the last earthquake, and so far as we have gone we have found the following interesting facts:—

1. That the area of injury is very small.
2. That the angle of emergence rapidly diminishes as we recede from the seismic vertical.
3. That the focus is therefore very near the surface.
4. That there is another seismic vertical at Fontana. This is probably explained by conduction along a column of trachyte which occupies the old vent of Epomeo, as Fontana lies in the very centre of the old crater.

Mallet pointed out that shallow foci must produce violent effects in limited areas, that transmission to a distance of the earth-wave diminishes rapidly—conditions we find well illustrated in the present case. This led Prof. Palmieri to believe that the shock of 1881 was the result of the tumbling in of the clay caves near Casamicciola, and he again proposes a similar explanation, as for this one he had not noticed any movement of the seismographs of Naples of Vesuvius. On the former occasion I pointed out in my letter at the time that such could not be the case, not even as the result of the imagined spaces excavated by the dissolving action of the mineral waters. The real truth seems to be the inelastic nature of the tufas, which vary much in density and dip in every imaginable direction, so that the earth-wave has two powerful retarding agencies at work—the absorption by an inelastic medium, and continual reflection and refraction from its irregular structure. If really a falling-in had occurred as the cause of the earthquake, we should expect some signs of it, but such is not the case; there is not a true fissure in the locality, and no apparent changes of level. Nor can we conceive that the houses of Forio, four miles distant, would be shaken about the ears of their inhabitants; besides in this one, unlike the preceding shock, Naples felt the movement quite distinctly.

In addition to the destruction of the houses by the shock, fires have burst forth amongst the ruins, and two large landslips have swept down from the flanks of Epomeo, and converted gardens and vineyards into utter ruin.

In conclusion I would remark, as was done on a former occasion, that we must expect other shocks more violent in character, and that, as one follows the other, the interval of tranquillity will be less, until the final eruption bursts forth. What time such an occurrence may be expected it is only possible to judge by the force, character, and frequency of future events. Only last week I advised Dr. Dohrn of the peril of living at Casamicciola; and another friend, who would not heed my warning as to the event and the dangerous position of certain rooms in his house with regard to the seismic vertical, has lost his son in the part of the building indicated.

Would this not be a remarkably favourable occasion to carry out a thorough investigation of the whole of the phenomena accompanying this type of earthquake? If, for instance, some scientific society would choose a committee, provide a number of suitable seismographs to be placed in different parts of the island, and any other means that might be proposed, so as to study the progress of the focus towards the surface, if such is really the case, the form of the focal cavity, and many other points of interest, it might be the means of preventing further catastrophes by showing the nearing approach of volcanic matter to the surface.

The horrors of the occasion I will not touch upon, as it is the province of other newspapers, not to speak of the hurry in which I send off this rough memorandum of my visit to the island. H. J. JOHNSTON-LAVIS

Naples, July 30

P.S.—Since writing the above, notices from Isernia in the Apennines announce a severe earthquake in that locality, besides others at Sorrento and in other parts of Italy. The three Ischian shocks were each accompanied by a period of seismic activity in Italy and other parts of Europe. Vesuvius is slightly more active.

August 1.—Another slight shock occurred at Casamicciola about 4 p.m., and another a little before 12 p.m.

August 2.—At 12.30 another shock took place.

THERE has not yet been time to collect data which may throw light on the origin of the terrible catastrophe that visited Ischia on the 28th of last month. As in the case of the previous earthquake on the island, one of the most striking features of this last calamity is its extremely local character. There does not appear to have been any simultaneous perceptible tremor at Naples, and Professor Palmieri's delicate seismometers on Vesuvius registered no sympathetic movement on that mountain. That the source of the shock at Ischia must have lain comparatively near the surface may be confidently inferred. Had it been more than a few hundred feet deep, the waves of such a shock would assuredly have been propagated to a considerable distance all round.

Various possible causes of earthquakes have been assigned, each of which may at different times and places be effective in the production of the phenomena. The sudden snap of large masses of rock under great strain may be the origin of the frequent earthquakes of mountain chains, such as those so constantly experienced along the line of the Alps. On a smaller scale similar results may arise on a line of dislocation, as is probably the case at Comrie in Scotland. In volcanic regions the earthquakes that usually precede and accompany volcanic eruptions have been plausibly attributed to the explosions of elastic vapours, and particularly of steam. Ischia lies in a volcanic district, and is itself of volcanic origin. But its earthquakes do not seem to be part of the active volcanic phenomena of the district. So far as information is yet available regarding the recent catastrophe, there appear to have been no concomitant volcanic manifestations, though there were active vents where they might certainly have been expected to show themselves. The only facts yet known that might indicate a connection between the Ischian earthquake and the vulcanicity of the Neapolitan district are the reported outflow of lava from Vesuvius on the 31st, and the alleged increase in volume and temperature of the Ischian hot springs. As regards the descent of lava towards Torre del Greco on Tuesday of last week, it did not take place until three days after the calamity of the 28th ult., and may have been entirely independent of it. Disturbance of the thermal springs of the locality could hardly fail to accompany so severe a shaking of the ground, from whatever source the concussion might arise.

So far as materials exist for forming a judgment on the subject, the recent earthquake at Ischia appears to have been caused by the sudden collapse of some subterranean cavern, situated not far below the surface in the Casamicciola district. Such caverns no doubt frequently exist underneath volcanic vents from which large masses of material have been emitted. It is well known to geologists that one of the final phases in the history of a volcano is the subsidence of the cone. This downward

movement probably continues during a long period of time. It may be on the whole gradual and imperceptible; but if, from time to time, the roofs of the huge vesicles, whence lava and steam have escaped, should give way, though there may be no perceptible change of level at the surface, such shocks will be generated as to convulse the area with earthquakes. We may infer that the Ischian earthquakes, though not directly connected with the present active volcanic phenomena of the district, are the result of the former extravasation of volcanic materials, and the consequent vesicular condition of the earth's crust at the locality. But we must await the careful collection of evidence before any positive conclusion on the subject can be embraced.

THE NORWEGIAN NORTH-SEA EXPEDITION

WITH the general work of the expedition sent out by the Norwegian Government in 1876-8 for the investigation of the physical and biological conditions of the North Atlantic, our readers have already been made familiar by communications from Dr. Mohn during the progress of the expedition. We have, moreover, already noticed one or two of the five volumes containing some of the results of the expedition. When the series of publications connected with the expedition is complete, it will form one of the most important contributions to a knowledge of the deep sea hitherto published. The present article is concerned with vols. iv. and v. of the series, containing a historical account of the expedition, a description of the apparatus used, the astronomical, magnetic, geographical, and natural history observations.

The historical account by Capt. Wille, who was in command of the vessel, the *Voringen*, tells us that so long ago as 1874 Professors Mohn and G. O. Sars memorialised the Norwegian Government on the importance of a thorough investigation of the North Atlantic. In the memorial we find an excellent summary of what had already been done by previous expeditions, and what might be accomplished by a new one. The Norwegian Government entered heartily into the proposal for an expedition, and after taking competent advice in the matter, resolved to agree to the prayer of the memorial, and appointed Capt. Wille to make the necessary preparations. Capt. Wille at once proceeded to England to confer with Sir George Nares, and to purchase apparatus. A suitable vessel, the *Voringen*, was purchased, and specially fitted and equipped for the work of the expedition; very brief and elastic instructions were issued for the general conduct of the expedition, while each member of the comprehensive scientific staff was furnished with special instructions for guidance in his work. The liberal scale on which the expedition was organised has guided the Norwegian Government in the publication of the results. These are contained in a series of large quarto volumes, beautifully printed (in Norwegian and English), and abounding with maps, coloured illustrations, and engravings. These volumes are liberally distributed among institutions and individuals in all countries, wherever indeed they are likely to be of service to science. Such liberality in a comparatively poor Government like that of Norway is in marked contrast to the conduct of the Government of the wealthiest country in the world in respect of the *Challenger* publications.

The general scope of the expedition was (1) to determine by soundings the contour of the sea-bed; (2) the rate and direction of currents; (3) the surface-temperature of the sea; (4) to investigate the physical conditions and chemical constituents of the sea-water; (5) zoological work; (6) botanical work; (7) meteorological observations; (8) magnetical observations; (9) whatever other observations time and place might render practical. Thus it will be seen the programme was comprehensive enough; and as the voluminous reports show, much valuable work was done in each department. Among the scientific staff on board were Prof. Mohn and G. O. Sars.

The *Voringen* prosecuted her work for about three months in the summers of the years 1876-7-8. During

Fig. 1.

that time she made numerous sections over the region lying between the west coast of Norway and a line extending from Iceland to Spitzbergen on the one side, and between Faeroe and the north of Spitzbergen on the other; in 1878 moreover she made a circuit east and

north from Vardoe to Bear Island. On every section stations were established for observations at very frequent intervals; off the coast of Norway these

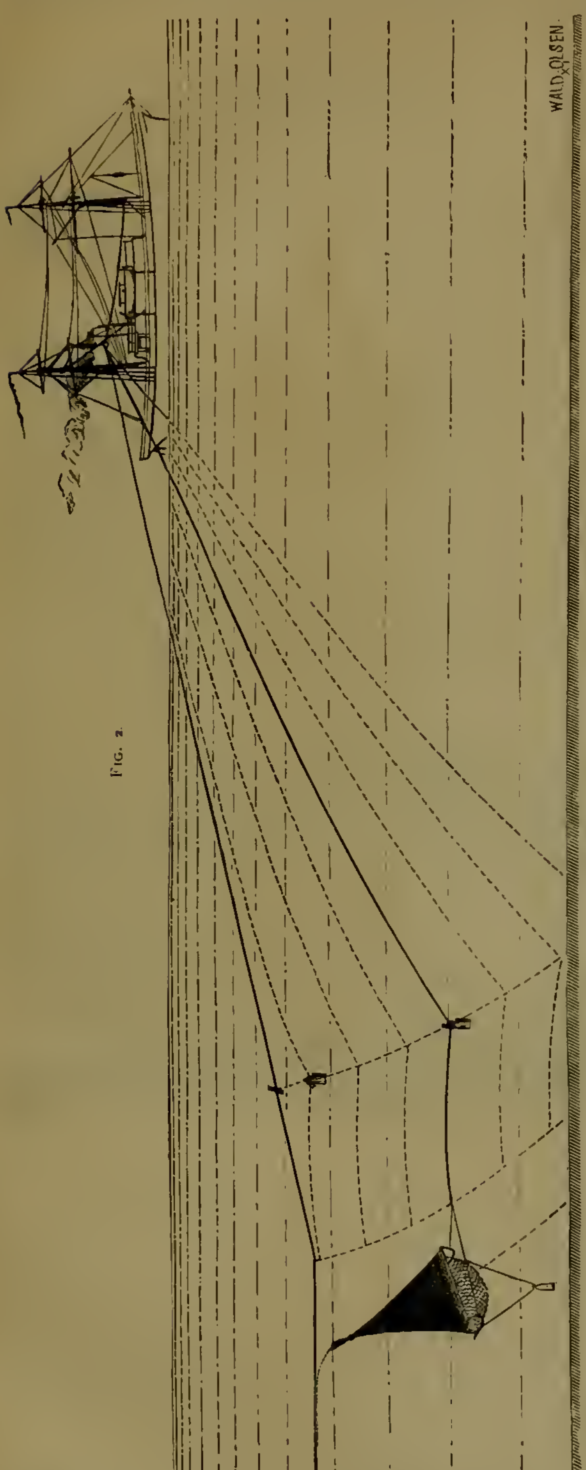


Fig. 2

stations were as close as they could well be. The vessel itself, of 344 tons, was admirably arranged for the work, the best possible use having been made of the not too large space for disposal. The apparatus was abundant, and in its construction the experiences of previous expeditions were fully taken advantage of. We are all so familiar with the apparatus used in deep-sea work that it is unnecessary to describe it in detail. The experience of the first year led to some improvements in the arrangements of the work-room, which occupied the whole breadth of the ship; and the light and ventilation were much improved. As a specimen of the apparatus, we reproduce the illustration of the sounding accumulator (Fig. 1), composed of fifteen straps. To the lower thimble is hung the cast-iron sounding-block, provided with a swivel on artificial rollers, and two hinged arms to act as fair leaders for the line. When in use, the apparatus hangs suspended from the port mainyard-arm. Its most important function is to take off the suddenness of the strain on the line when the vessel is rolling or pitching. For collecting water both from the bottom and intermediate depths, Capt. Wille devised a very ingenious water-bottle, which could hold five litres. The sounding-line, 3000 fathoms, was wound on the port side of the after-deck on a large, strong reel, secured by screws to the deck. For dredging, especially, very careful preparations were made, and a variety of apparatus taken on board. Capt. Wille gives the following interesting description of their method of dredging :—

"We steamed full speed ahead, with the wind a little on our starboard bow. So soon as the vessel had got sufficient headway, the engine was stopped, the dredge lifted by hand over the railing, and dropped into the sea. At the foremast, a man with thick leather gloves stood ready to pay out the dredge-rope, which another kept clear with a handspike as it ran out from the coil in the locker. On the dredge entering the water, the word was immediately given to veer, when the paying out commenced, slowly, however, to make sure that all was right. So soon as the dredge was clear of the propeller, the vessel again went ahead, steaming at a uniform rate of 4 knots, which the engineer was enabled to keep up by frequent reference to the water-log (see below). Meanwhile, we kept steadily veering, while taking care, by frequent holding on to the rope, that the length run out should be properly taut, and steering the course given to the ship when the dredge was put over. After paying out, according to depth, 200, 300, or 400 fathoms, we again stopped, hauled in the rope to the taffrail by means of the lizard and thimble, and fastened, below the latter, with spun-yarn, a wooden toggle to the rope. Starting again (same course and speed), we next ran out the whole length of rope deemed necessary for the operation—not less than double the depth, nay for smaller depths even more.

"The engine was now stopped, after which we hauled in the dredge-rope, as before, to the taffrail, and kept it up in a bight of rope's end. With the lizard was then made fast to the wooden thimble a weight proportioned to the depth, consisting of 3 or 4, and for the deepest dredgings of as many as 6, of the sinkers of the Baillie sounding-machine, weighing each 55 pounds. We now, after letting go the rope, tilted the weight overboard, which spun down along it till stopped by the wooden toggle. The shock of its arrest was distinctly perceptible to a person who had his hand on the rope.

"The vessel was now kept stationary, while the weight and the dredge were sinking. After some experience, we calculated the time required for the dredge to sink 100 fathoms to be about 12 minutes. Fig. 2 will give an idea of the descent of the dredge, or rather of the trawl. The supposed depth in the diagram being 1300 fathoms, the vessel and the trawl are of course on a much larger scale. The dotted lines represent the lines of descent of the weight, the shackle, the dredge-rope, and the beam of the trawl—assuming the trawl to sink more slowly than the weight. When the weight strikes the bottom, the trawl has still some distance to travel, and the last part of its

line of descent will be well-nigh perpendicular. We found that, when worked in the manner described above, both trawl and dredge could as a rule without difficulty be made to reach the bottom in the right position. If the dredge or trawl descend much more slowly than the weight, it will fall vertically, with the heavy end foremost. If, on the other hand, its rate of descent be equal to or exceed that of the weights, it will, on reaching the bottom, have a horizontal component in its motion—which is pretty sure to keep it from clogging during the ensuing operation."

In vol. v. Dr. Mohn renders account of the astronomical observations, as well as the geography and natural history of the expedition, while Capt. Wille describes the magnetic observations. The vessel was well supplied with suitable instruments for determining latitude and longitude, and use was made of them whenever favourable opportunities presented themselves. Observations were thus made at nine important points. The various magnetic elements were determined at eight stations on land and seven at sea.

The most interesting geographical results of the expedition were in connection with Jan Mayen, to which Dr. Mohn devotes considerable space and many fine illustrations, several of them coloured. While the expedition was in progress, Dr. Mohn sent us some account of his observations in this island, with illustrations (vol. xviii. p. 222), but of course the subject is more fully treated in the volume before us. Evidences of volcanic action were observed everywhere, and the forms of some of the old craters are very beautiful. As the main object of the expedition was deep-sea research, Dr. Mohn could not

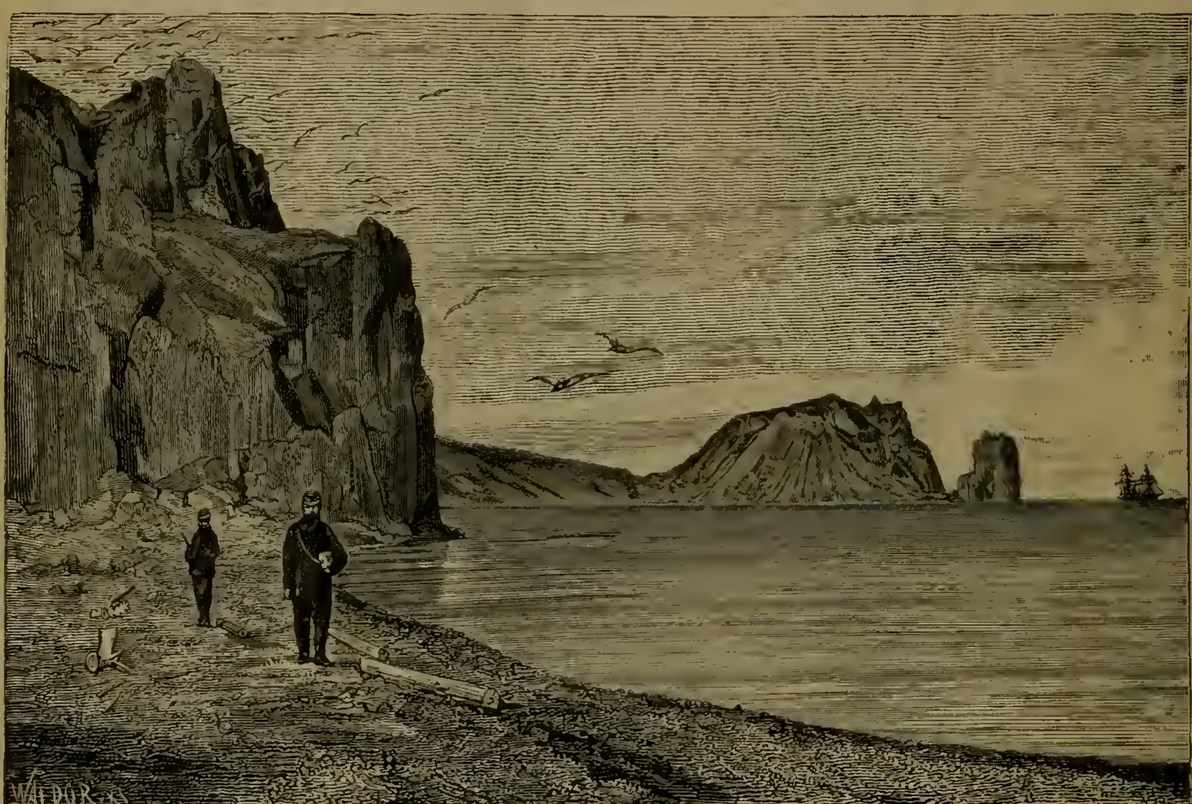


Fig. 3.

give so much time to the observations of the island as he could have wished; nevertheless, by bringing together the observations of the various members of the expedition, and comparing them with the results obtained by Scoresby and other previous observers, Dr. Mohn is able to give a very full and interesting account of this curious island, which we here quote:—

"Cut off on all sides by extensive ocean tracts from the nearest land, the Island of Jan Mayen occupies an isolated position in the Greenland Sea. Between Norway and Jan Mayen the depth reaches 1760 fathoms, towards Spitzbergen upwards of 2000 fathoms, towards Greenland upwards of 1300 fathoms, and towards Iceland upwards of 1000 fathoms. The direction of the island is from N.E. by E. to S.W. by W.; it points towards Denmark Strait, and lies parallel to the volcanic line of Mount Hecla. As previously stated, Jan Mayen is built up of volcanic rocks, all of which would appear to belong to the modern group. Hence the island is probably a later formation than are the Faeroes and Iceland, where the old volcanic rocks prevail either exclusively or in greater part. Its length slightly exceeds 7½ geographical miles. It consists of two large parts or divisions, a northern and a southern, connected together by a lower and narrower tract. The greatest breadth of the northern part is a little more than 2 geographical miles, that of the southern 1½ geographical miles, and the connecting tract (including the lagoon) measures at the narrowest point 1½ English miles across (0·4 geographical mile). The area of the island is 7·32 geographical square miles."

Fig. 3 gives a view of the headland forming the north-east extremity of North or English Bay, the isolated rock in the distance being the Brielle Tower of the Dutch navigators.

(*To be continued.*)

THE SHOOTING STARS OF THE JULY METEORIC EPOCH

QUETELET pointed out, many years ago, the period July 26—30 as a meteoric epoch of considerable intensity, and recent observations have fully confirmed his opinion. There are two special showers contributing to this result, namely, the Aquariads and Perseids. The latter merely represents the oncoming of the great August display which culminates on the 10th and then rapidly dies out.

This year, on July 28th, the sky was very clear from clouds (though a little haze prevailed), and a fairly good opportunity was offered for witnessing these July meteors. It was important that this should be done, because the previous observations were not in satisfactory accordance. Professor Herschel in 1865, July 28th, had observed the chief radiant near the bright star *Fomalhaut*, and in 1881, July 25—30, M. Cruls of the Imperial Observatory at Rio Janeiro, found that the radiant point of more than 90 per cent. of the meteors observed during that period was situated five degrees north of *Fomalhaut*. In 1880, July 28—30, Mr. E. F. Sawyer, of Cambridgeport, Mass., found the major radiant to lie at α 330°, $\delta - 6°$, with minor showers at 328° – 15°, and 341° – 10°. Colonel Tupman, who watched these meteors with considerable success and accuracy during the last few nights of July, 1870, determined the focus of divergence as at 340° – 14°, and the writer from observations at Bristol in 1878 and 1879, corroborated this position, and found that in addition to these Aquariads, there was a very rich contemporary shower, directed from a point near the star cluster χ Persei.

Comparing the various observations to which we have just briefly referred, it will be seen that considerable doubt exists as to the exact centre of radiation of these July meteors. Obviously the point is either in Aquarius or further south in *Piscis Australis*, and near the conspicuous star *Fomalhaut* of that constellation. The observations also suggest that there may be several streams in marked activity at this epoch, and it was with the object of obtaining further evidence towards the settlement of this question, that I reobserved these meteors on the night of July 28 last.

I began watching the eastern heavens at 10.30, and at 10.36 a very fine meteor, as brilliant as Jupiter, appeared near γ Andromedæ. It had a short path of only four degrees, and left a vivid streak. The meteor was evidently much foreshortened and close to its radiant point slightly west of χ Persei, so that it was an early forerunner of the Perseids. At 11.4 another fine meteor, of exactly similar type, was seen falling between α and β Andromedæ, and at 11.10 a third, considerably brighter and quite equal to Venus, traversed a path of nearly thirty degrees between Pegasus and Aquila, where it left a bright streak of some twenty degrees for a few seconds. Several other Perseids were observed later on, and the radiant point was found to lie at 27° + 55°, which conforms fairly well with the position I found for the same display in 1878 at 32° + 53° (63 meteors). As to the expected shower of Aquariads I was not disappointed, though during the earlier part of the night only small ones were seen, and I could not get the position of the radiant with the necessary exactness. Between 13h. and 14h. however, I saw eight Aquariads, and three of these were brilliant. At 13h. 13m. one appeared just below β Andromedæ. It was brighter than a first magnitude star. At 13h. 37m. a fine Aquariad, rivalling Jupiter, was seen in the west region of Pisces, and at 13h. 54m. another of the first magnitude appeared in nearly the same place. They moved slowly and left trains of sparks.

During the 3½ hours (10½h. to 14h.) that I continued to watch I saw eighteen of these Aquariads, and by the intersection of the paths, found the radiant very sharply defined at 337° – 11°, and close to the point I had determined in 1878 and 1879. This shower was far superior to the Perseids in the morning hours, and fully asserted its claim to be considered as the special display of the epoch. The meteors generally have long paths, as the radiant point is not far above the horizon. In all I saw 48 meteors during the night, and of these no less than 28 belonged either to the Perseids or Aquariads.

There can be no doubt that these July Perseids are identical with the celebrated shower of August 10, though the radiant point is some 8° west in July. I have watched these Perseids very carefully from July 25 up to August 16 in several years, and traced the gradual shifting of the radiant point. From my observations during the last week of July, 1878, I had supposed these July Perseids to form a distinct shower to the Perseids of August 10, but from observations obtained on intermediate dates, *i.e.* on August 3, 4, and 5, the connection of the two showers is most certainly established, and the displacement of the radiant point on each successive night can be clearly distinguished by those who will mark the tracks of such meteors as appear near this radiant from say July 25 to August 15.

As to the Aquariads, I believe the maximum takes place on July 27-28, when they are undoubtedly more numerous than the early Perseids. I feel certain that the radiant point is near δ Aquarii or at 339° – 13°. There is another shower near *Fomalhaut*, which appears to have developed remarkable energy in 1881 from M. Cruls' observations, and there are also other showers in Aquarius at this special period, which have led to the difficulty in determining the position of the major radiant. There is certainly a very fine shower of meteors at the end of July from a point a few degrees S.E. of β Aquarii, which has been observed as follows :—

	July 25-31	324° – 6°	Schmidt.
1870	July 28	326 – 13	Tupman.
1880	July 28-30	328 – 15	Sawyer.
	July 25-31	324 – 9	Denning.

I gave some details of this particular stream, which, it may be added, is one remarkable for its large meteors, in the *Monthly Notices of the Royal Astronomical Society* for November, 1881, p. 38.

It now becomes important to watch for the annual returns of these meteors of the July epoch at observatories in the southern hemisphere, where they may be more favourably observed than in high northern latitudes. Obviously, a shower near *Fomalhaut* will be in a great measure marred by the extremely low altitude of the radiant, as that star never attains an altitude even of 10° in this country. At stations further south, the shower of Aquariads appears to be one of great strength and to form a display of first-class importance. Observations made in 1879 show a wide disparity in the number of these meteors visible in different latitudes. Mr. D. W. Barker, during a voyage from London to Melbourne (*Monthly Notices*, Vol. XL., p. 364), in that year observed meteors falling at the rate of 180 per hour on July 28 and 120 per hour on July 29, between 0h. and 4h. a.m. on the dates referred to. Yet, at Bristol on July 28 of the same year, the hourly number was only 23, and on July 29, 11.

The further investigation of the July meteoric epoch offers an attractive field to observers. Apart from the rich shower of Aquariads there are the Perseids, equally interesting from the fact that these early members of the great shower prove it to be one of long duration, and to have a radiant point which shifts its position amongst the stars from night to night. These interesting details will no doubt come under frequent observation in future years.

W. F. DENNING

NOTES

The meeting of the French Association for the Advancement of Science will take place this year at Rouen on the 16th inst.; extensive preparations are being made for the reception of the members. The electric light is to be a prominent feature of the meeting, owing to the project entertained by the municipality of lighting part of the city by the motive power of the Seine at Pont de l'Arche, as we mentioned in a recent note.

The Lords of the Committee of Council on Education have sanctioned the addition of Hygiene to the list of sciences towards instruction in which aid is afforded by the Science and Art Department. The following is the syllabus of the subject:—Elementary stage: (1) food, diet, and cooking; (2) water and beverages; (3) air; (4) removal of waste and impurities; (5) shelter and warming; (6) local conditions; (7) personal hygiene; (8) treatment of slight wounds and accidents. Advanced stage: (1) food and adulterations; (2) water and beverages; (3) examination of air—chemical and microscopical; (4) removal of waste and impurities; (5) shelter and warming; (6) local conditions; (7) personal hygiene; (8) prevention of disease. Honours:—In addition to the topics enumerated under the elementary and advanced stages, questions will be set in the following subjects: trades nuisances, vital statistics, sanitary law.

The new portion of the University of Indiana, at Bloomington, in that State, was set on fire by lightning during a thunderstorm on the night of July 12, the electricity travelling along a telephone wire which served the institution. The laboratory, museum, and library were completely destroyed. The museum contained a collection of fishes, made by Dr. Jordan, which was thought to be the largest and most valuable in the United States. There were 15,000 volumes in the library, besides the so-called Owen collection, the loss of which is believed to be irreparable. The general loss is estimated at $200,000, of which only $30,000 is covered by insurance.

The U.S. bureau of education has, we learn from *Science*, just published a circular of information, containing the results of an inquiry into the effects of co-educating the sexes in 340 cities and large towns of the Union. Of these, 321 practise co-education throughout the public-school course, 17 co-educate for part of the course, and 2 separate the sexes entirely. A careful analysis of the reasons adduced for co-education enables the editor to formulate them as follows: co-education of the sexes is preferred where practised, because it is (1) *natural*, following the usual structure of the family and of society; (2) *customary*, or in harmony with the habits and sentiments of every-day life and law; (3) *impartial*, affording to both sexes equal opportunities for culture; (4) *economical*, using school funds to the best advantage; (5) *convenient* both to superintendent and teachers in assigning, grading, instruction, and discipline; and (6) *beneficial* to the minds, morals, habits, and development of the pupils. The pamphlet concludes by observing that "both the general instruction of girls, and the common employment of women as public-school teachers depend, to a very great degree, on the prevalence of co-education, and that a general discontinuance of it would entail either much increased expense for additional buildings and teachers, or a withdrawal of educational privileges from the future women and mothers of the nation."

In an article entitled "Cholera and Our Water-Supply," in the current number of the *Nineteenth Century*, Dr. Percy F. Frankland draws attention to the vital connection between water-supply and the diffusion of zymotic disease. He points out how, in consequence of the terrible epidemics of Asiatic cholera to which the metropolis has been subjected in the past, the companies supplying London with water from the Thames have been obliged to remove their intakes to a distance which shall insure the freedom of their supply from contamination with the London sewage, and thus at any rate to put an end to their former practice of "rapidly restoring to the inhabitants of London the drainage matters which the sewers had discharged." But although the Thames at Hampton is free from this source of pollution, yet it is similarly fouled, although in a less degree, with the sewage of a population estimated at upwards of half a million which enters the river above the intakes of the water companies. In extenuation of this obviously revolting state of things, many theories have been started: of these the most popular and fallacious is that which, under the title of "the self-purification of river-water," announces that noxious organic matters present in river-water are rapidly destroyed in the course of a few miles' flow. This doctrine, unsupported as it is by any facts or accurate observations, is wholly dogmatic and in complete opposition to all previous knowledge concerning the properties of organic substances in general. The late Rivers Pollution Commissioners, moreover, conclusively proved that water once polluted with sewage is only very slowly purified, and more recent research shows the great tenacity of life possessed by the lower organisms which are believed to be allied to those capable of communicating zymotic disease. Chemical analysis further proves that the Thames water reaches the intakes of the London Water Companies with a but slightly diminished proportion of organic matters. In the face of the now well known fact that London possesses within easy reach water of the purest quality and abundant in quantity, it is inexcusable that such manifestly impure sources should still be resorted to. Hitherto only one of the eight metropolitan water companies has entirely abandoned the polluted rivers and substituted them by the pure water obtained from deep wells sunk into the chalk. London should follow the example of other large towns in Great Britain; thus Glasgow now drinks the waters of Loch Katrine, Manchester is bringing a supply from Cumberland, whilst London, with water of the best quality much nearer at hand, is still compelled to drink the waters of the Thames and Lea.

Three addresses will be delivered at Annonay by members of the Academy of Sciences on the occasion of the forthcoming inauguration of the Montgolfier statue. M. Dupuis de Lome will speak on the general history of ballooning; M. Tisserand, in the name of the Paris Observatory, on the scientific prospects of ballooning; and Col. Perrier, the representative of the President of the Republic, on the results of ballooning in warfare. M. Laussedat, the director of the Conservatoire des Arts, who was the first director of Meudon Châlet Aëronautical Establishment, will speak on the career of the brothers Montgolfier. The aëronautical ascents will be made with a Montgolfier by Eugène Godard, and with a gas balloon by M. Brissonet, *fils*, of Paris. We believe that M. Tisserand will recommend the use of balloons for certain astronomical observations.

The Trustees of the Australian Museum (Sydney) have issued their twenty-ninth Annual Report for 1882. The increasing importance of the Australian Museum, and the growing interest of the public in it, are shown by the remarkable increase of 18,446 visitors during the past year; the attendance being 81,596 on weekdays as against 73,995 in 1881, and 52,505 on Sundays as against 41,660 in 1881, the increase on weekdays being upwards of 14 per cent., and on Sundays upwards of 26 per cent. Application has been made to the Government to consider the necessity of enlarging the Museum buildings. More room is urgently required, not only for purposes of exhibition, but for the office staff and workmen. A catalogue of Australian stalk- and sessile-eyed Crustacea, prepared by Mr. Wm. A. Haswell, M.A., B.Sc., has been printed and distributed extensively among various museums and natural history societies; and the work of

cataloguing the whole of the Museum collections is being pushed forward as rapidly as possible. The most serious loss ever sustained by the Museum has occurred through the recent destruction of the Garden Palace—the large and varied collection of technological and ethnological specimens sent there for exhibition having been totally destroyed by the fire which consumed the building. The Technological Committee lost no time in commencing a new collection; and, having already obtained many ethnological specimens of great interest, they are taking steps to secure as many others as possible. This is a work which admits of no delay, as genuine ethnological examples from the islands are becoming scarcer every day, in consequence of the general spread of trade and civilisation through the whole of Polynesia. Suitable accommodation for the display of the technological and ethnological specimens already in hand should at once, if possible, be provided. The most important work carried on by the Trustees during the year has been the exploration of the caves and rivers of Australia. It was continued until the close of December at the Wellington Caves, where the bones of an immense Echidna and of a large Struthian bird allied to the Emu, as well as some smaller animals of less note, hitherto unknown to science, have been discovered and added to the Museum. Numerous other fossil bones valuable for exchanges with foreign museums have been obtained. The exploration of rivers was conducted by the assistant taxidermist in Queensland, where strong hopes of discovering some new Ganoid fishes were entertained. A special report of this work, with a list of the specimens procured, is given in appendices.

THE Clothworkers' Company have agreed to give a donation of 10,000*l*. for the enlargement of the Textile and Industrial Department of the Yorkshire College at Leeds. Altogether the Clothworkers' Company have given upwards of 25,000*l*. towards this institution.

THE Ornithological Society of Vienna wishes to call the attention of English ornithologists to the International Congress of Ornithologists which will be held next spring at Vienna in connection with the annual exhibition of the Ornithological Society of Vienna, under the protection of H.I.H. the Crown Prince Archduke Rudolf of Austria. The chief business of the Congress will be to pass preliminary resolutions for international legislation regarding the protection of birds. The Austrian Government will send out invitations to the different foreign Governments, and will grant a free passage to Vienna to the representative of each foreign Government. All those interested in the above subject should apply for further information to Dr. Gustavus von Hayek, Hon. First Secretary of the Ornithological Society of Vienna, 3, Marokkaner Gasse, Vienna.

THE following list of candidates successful in the competition for the Whitworth Scholarships, 1883, has been issued by the Science and Art Department:—James Hamilton, Engineer, 200*l*.; William E. Dalby, Engineer Apprentice, 150*l*.; John L. Barnes, Engineer Apprentice, 150*l*.; Thomas K. Mackenzie, Student, formerly Mechanical Engineer, 150*l*; William Sumner, Fitter, 150*l*.; Frank W. Dodd, Engineer Apprentice, 150*l*; Charles N. Pickworth, Mechanical Engineer, 150*l*.; Henry E. Kitton, Mechanical Engineer, 150*l*; James Layzell, Engineer Apprentice, 150*l*.; Horace W. Meteyard, Engineer, 100*l*.; Alfred S. Ormsby, Mechanic, 100*l*.; William P. Abell, Mechanical Engineer, 100*l*.; Alfred W. Bevis, Tutor, formerly Engineer Apprentice, 100*l*.; John W. Aston, Engineer Apprentice, 100*l*.; Alfred E. Mackett, Marine Engine Fitter, 100*l*.; Victor F. Whitehead, Engineer, 100*l*.; Charles Lang, Pattern Maker, 100*l*.; James Bradshaw, Mechanical Engineer, 100*l*.; Alfred J. Joshua, Fitter, 100*l*.; William A. Rogerson, Fitter, 100*l*.; William E. Donohue, Draughtsman (Marine), 100*l*.; Albert H. Case, Engineer, 100*l*.; Alexander Shannon, Engineer, 100*l*.; Mark R. Bullimore, Fitter, 100*l*.; John S. Bean, Engineering Draughtsman, 100*l*.

THE biennial marine excursion of the Birmingham Natural History and Microscopical Society, which took place at Oban in July last, and lasted for ten days, was on the whole most successful. It was attended by twenty-three members of the Society. A superior screw steam yacht, the *Aerolite*, was chartered for the occasion, and the weather being very fine, dredging was carried on daily at various stations which were all recorded on a chart at depths which varied from fifteen to one hundred fathoms. The principal object of this excursion was to secure further specimens of the *Pennatulida*, a few only of which were taken in the dredgings at the same place during 1881. These formed the subject of a special report made to the Society last year by Prof. Marshall, D.Sc., and Mr. W. P. Marshall, M.I.C.E., and for which the Darwin Gold Medal, given by the Midland Union of Natural History Societies was awarded at the Tamworth meeting held in June last. Some special instruments made of galvanised iron and armed with hooks were devised by Mr. W. P. Marshall for the occasion, called the "plough" and the "harrow." These, together with the dredges and trawl, were for the first time on these excursions worked by means of steam gear. A small dredge measuring a few inches was used by hand for testing the nature of the bottom of the sea, and all these various appliances worked admirably. A large number of specimens of *Funiculina quadrangularis* and *Pennatula phosphorea* in various stages of growth were secured in fine condition and unbroken. A number of specimens of Sponges, Zoophytes (including a rare free form of *Zoanthus conchii*, var. *liber*, Gosse), Echinoderms, Crustaceans, Annelids, Tunicates, Mollusca, &c., were also secured. These were exhibited and described to the members during the days and in the evenings by Mr. W. R. Hughes, F.L.S., chairman of the excursion, Mr. W. P. Marshall and Mr. J. F. Goode, Hon. Sec. of the Biological Section, who have also made a preliminary report thereon to a recent meeting of the Society. During the excursion phosphorescence was for the first time observed in *Funiculina*, the characteristic pale blue light coruscating over the whole series of polypes, the length of the specimen being between three and four feet, and presenting a very beautiful effect when viewed in the dark. In addition to the dredging, some attention was paid to the botany and geology of the district by several of the members. During a walk on July 1 fifty species of plants were gathered in flower. A collection of specimens of the rocks of Oban and the vicinity, including Staffa, Iona, Mull, Glencoe, Easdale, &c., was also made for future examination.

THE city of Geneva intends to utilise the current of the Rhone for lighting the whole of the city. A report on the question is being drawn up, which will be submitted to the Council of State.

A PRELIMINARY meeting of the members of the future Société des Électriciens took place at the Ministry of Posts and Telegraphs. M. Cochéry was present, but he declined to preside over the proceedings, and the honour was bestowed upon M. Berger.

AN electrical omnibus was recently tried on the Cour de Carrousel, Paris, before M. Cochéry to prove the facility with which this sort of carriage is handled in spite of its immense weight. The trial, which took place in the busiest hours of the day, attracted much notice from the passers-by, and was generally deemed satisfactory.

THE *Italia del Popolo*, in one of its latest numbers, gives the names of a number of localities from which birds and insects have disappeared just before invasions of cholera.

THE death is announced, at the age of 83, of Linant Pasha (Linant de Bellefinds), one of the leading personages connected

with the existing Suez Canal. Under Said Pasha he was appointed head of the Ponts et Chausées department, and chief engineer of the Suez Canal project. In early life he travelled much in Abyssinia, Kordofan, and Darfur.

SIR CLAUDE DE CRESPIGNY, in company with Mr. Simmons, made a successful balloon voyage from Maldon in Essex across the North Sea to Flushing on Wednesday last week. The start was made at 11 a.m., and Flushing was reached about 8 p.m. The highest altitude reached was 17,000 feet.

WAUSCHAFF of Berlin has lately made a piece of apparatus for registering earth currents. It consists of a very delicate galvanometer inclosed in a case with a clockwork arrangement for moving a photographic plate steadily downwards. A fine ray of light is reflected on to the galvanometer mirror by a total reflection prism and is focused on the photographic plate. The speed of the movement of the plate is 80 mm. per hour, thus allowing variations from minute to minute to be observed.

MM. LELANDE AND CHAPERON have brought out a new battery of very remarkable properties. The battery is a single liquid cell and has a depolarising electrode of oxide of copper, the liquid used is caustic potash, and the other pole is zinc. The battery is made in various forms, its E.M.F. is nearly 1 volt, whilst it is said to give a steady current through even a low resistance for many hours. Finally it is claimed for this battery that when exhausted it can be restored by driving a current from an accumulator through it.

A NEW edition (the fifth) is announced of the "Dictionnaire des Arts et Manufactures et de l'Agriculture," edited by M. Ch. Laboulaye.

MR. BROWNE asks us to say that in his recent article on Glacier Motion, p. 235, by a slip of the pen he stated that the sides of a glacier move faster than the middle, whereas, as every one knows, the reverse is the case.

THE additions to the Zoological Society's Gardens during the past week include a Grivet Monkey (*Cercopithecus griseo-viridis* ♂) from West Africa, presented by Lord Hastings; two Black-backed Jackals (*Canis mesomelas*), two Triangular Pigeons (*Columba guinea*) from South Africa, presented by Mr. R. Southey; two Indian Brush-tailed Porcupines (*Atherura fasciculata*) from Ceylon, presented by Mr. A. Dent; three Puffins (*Fratercula arctica*), British, presented by Mr. H. Becher; a Common Cormorant (*Phalacrocorax carbo*), British, presented by Mr. W. R. Temple; a Common Barn Owl (*Strix flammea*), British, presented by Mr. H. Hanaeur; a Common Wombat (*Phascolomys wombat* ♂) from Tasmania, a Common Cormorant (*Phalacrocorax carbo*), British, a Common Boa (*Boa constrictor*) from West Indies, deposited; a White Stork (*Ciconia alba*), two Common Spoonbills (*Platalea leucorodia*), two Purple Herons (*Ardea purpurea*), European, purchased; a Musk Deer (*Moschus moschiferus* ♂) from Central Asia, received on approval; a Collared Fruit Bat (*Cynonycteris collaris*), two Amherst's Pheasants (*Thaumalea amherstiæ*), two Summer Ducks (*Aix sponsa*), bred in the Gardens.

A CONTRIBUTION TO THE STUDY OF THE TRANSMISSION EASTWARDS ROUND THE GLOBE OF BAROMETRIC ABNORMAL MOVEMENTS

IN his paper on "Abnormal Variations of Barometric Pressure in the Tropics, and their Relation to Sun-spots, Rainfall, and Famines," published in NATURE (vol. xxiii. pp. 88 and 107), Mr. Fred. Chambers pointed out, when treating of the barometric records of the stations, St. Helena, Mauritius, Bombay, Madras, Calcutta, Batavia, and Zi-ka-wei, that abnormal movements which had occurred at a westward station—*e.g.* Mauritius—reappeared at an eastern station—*e.g.* Bombay—some time later, and then again at a further eastern station, Madras, still later, and so on, until they finally reached the most distant station eastwards. It appeared therefore that there were abnormal movements of the atmospheric pressure which travelled from west to east; the rate of travel seemed to vary at different times; and Mr. Chambers summed up his results in the following words:—"It appears then that these atmospheric waves (if such they may be called) travel at a very slow and variable rate round the earth from west to east like the cyclones of extra-tropical latitudes."

In his "Brief Sketch of the Meteorology of the Bombay Presidency in 1880," Mr. Chambers proceeded to test the validity of his conclusions by applying them to an examination of the barometric records of Zanzibar for that year and a portion of the next as compared with the records of Belgaum for the same period; and he again noticed that "there was much similarity in the abnormal movements of barometric pressure at Zanzibar and Belgaum, although these stations are about 2500 miles apart, but that the Belgaum curve lagged decidedly from two to three months behind the Zanzibar curve."

This discovery, if substantiated, would obviously prove of great practical value, inasmuch as it would make it possible to obtain a forecast of the barometric movements about to occur at any particular station by watching the movements already taking place at a point westward of that station. And as definite variations in the atmospheric pressure may be, and in some cases are known to be, accompanied by definite variations in the other meteorological elements, a method of weather prediction would thus be furnished.

It has fallen to my lot to receive and discuss the Zanzibar observations succeeding those last discussed by Mr. Chambers; and the results obtained by my examination of them seem to involve matters of some practical and theoretical interest.

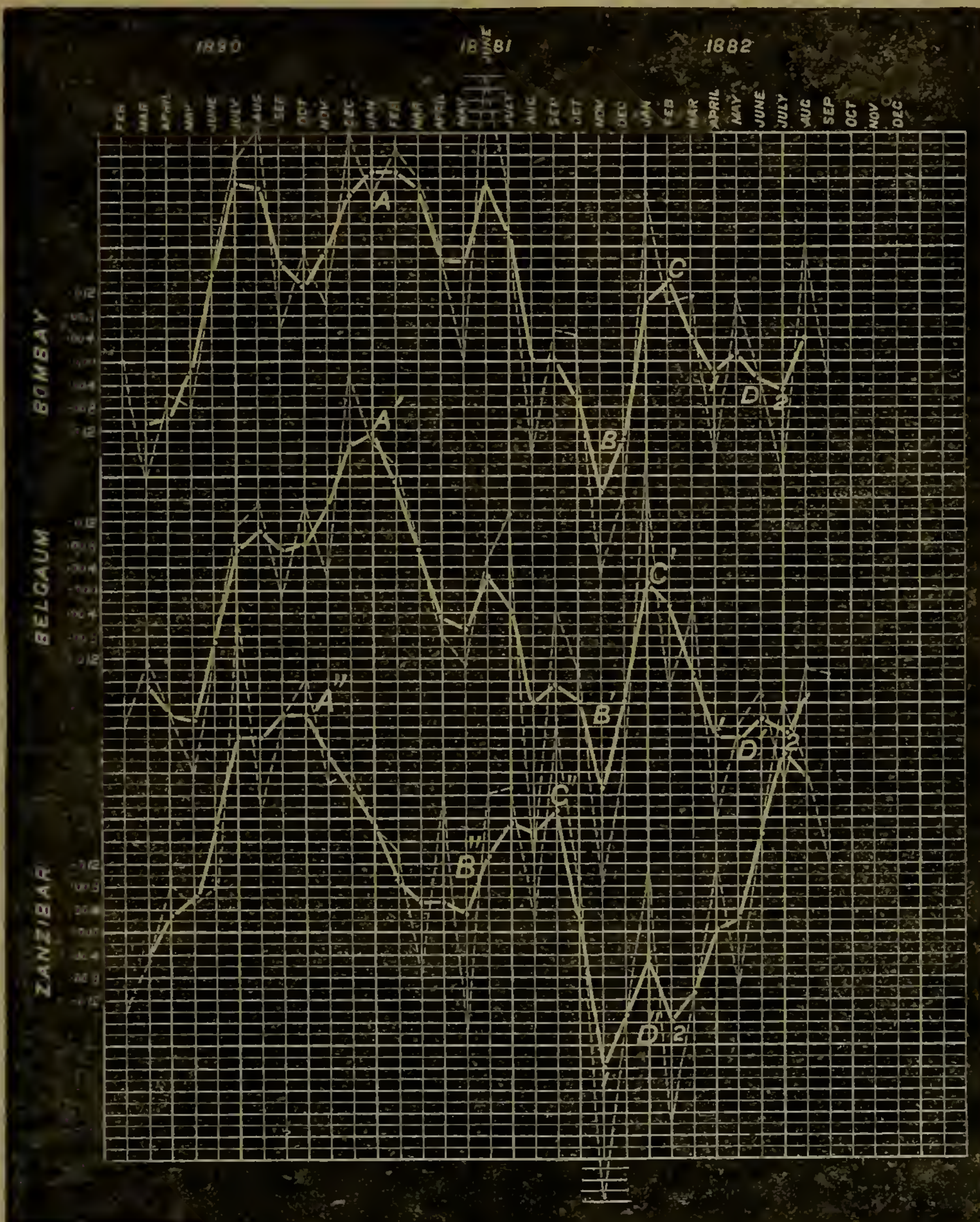
TABLE I.—*Monthly Abnormal Barometric Pressure at Zanzibar, Belgaum, and Bombay*

Months.	Monthly Abnormals (unsmoothed).			Monthly Abnormals (smoothed).		
	Zanzibar	Belgaum	Bombay	Zanzibar	Belgaum	Bombay
February 1880	– ·014	– ·024	·000	—	—	—
March ,,	– ·005	– ·013	– ·020	– ·004	– ·017	– ·011
April ,,	+ ·008	– ·022	– ·006	+ ·003	– ·022	– ·010
May ,,	+ ·005	– ·032	– ·008	+ ·006	– ·023	– ·002
June ,,	+ ·008	– ·008	+ ·015	+ ·018	– ·009	+ ·014
July ,,	+ ·054	+ ·011	+ ·035	+ ·034	+ ·007	+ ·031
August ,,	+ ·022	+ ·015	+ ·041	+ ·034	+ ·010	+ ·030
September ,,	+ ·038	– ·001	+ ·006	+ ·035	+ ·007	+ ·017
October ,,	+ ·044	+ ·015	+ ·019	+ ·038	+ ·008	+ ·013
November ,,	+ ·026	+ ·003	+ ·010	+ ·031	+ ·014	+ ·019
December ,,	+ ·028	+ ·037	+ ·040	+ ·025	+ ·015	+ ·029
January 1881	+ ·018	+ ·026	+ ·029	+ ·019	+ ·027	+ ·033
February ,,	+ ·014	+ ·022	+ ·037	+ ·009	+ ·019	+ ·033
March ,,	– ·007	+ ·008	+ ·032	+ ·005	+ ·007	+ ·030
April ,,	+ ·023	– ·008	+ ·020	+ ·005	– ·005	+ ·018
May ,,	– ·017	– ·013	+ ·001	+ ·003	– ·007	+ ·017
June ,,	+ ·024	+ ·005	+ ·049	+ ·013	+ ·002	+ ·031
July ,,	+ ·025	+ ·013	+ ·028	+ ·019	– ·003	+ ·022
August ,,	+ ·003	– ·046	– ·016	+ ·017	– ·020	·000
September ,,	+ ·037	– ·004	+ ·005	+ ·021	– ·017	·000
October ,,	+ ·009	– ·014	+ ·004	+ ·002	– ·020	– ·006
November ,,	– ·047	– ·051	– ·037	– ·024	– ·035	– ·023
December ,,	– ·014	– ·028	– ·024	– ·015	– ·020	– ·010
January 1882	+ ·012	+ ·026	+ ·029	– ·005	+ ·001	+ ·014
February ,,	– ·033	– ·017	+ ·010	– ·016	– ·003	+ ·014
March ,,	– ·012	– ·003	+ ·011	– ·011	– ·015	+ ·004
April ,,	+ ·012	– ·040	– ·016	·000	– ·026	– ·002
May ,,	– ·009	– ·024	+ ·011	+ ·002	– ·026	+ ·001
June ,,	+ ·017	– ·018	– ·002	+ ·016	– ·023	– ·003
July ,,	+ ·040	– ·034	– ·019	+ ·031	– ·025	– ·005
August ,,	+ ·029	– ·014	+ ·020	+ ·027	– ·019	+ ·004
September ,,	+ ·012	– ·015	– ·002	—	—	—

From these observations the variations from the normal monthly barometric movements have been obtained. They are tabulated in Table I., and are represented graphically by the

thin dotted line of the lowest curve in the accompanying plate. Alongside of them are arranged, both in the table and the plate, the barometric abnormal movement of the meteorological stations, Belgaum and Bombay. In order to facilitate comparisons between the curves of these three stations the actual normal movements, which are represented in the plate by thin dotted lines, have been put through a double process of smoothing; the results are tabulated under the heading of "Monthly Abnormals (smoothed)," and are represented on the plate by the thick continuous lines. The observations not previously discussed are those taken from June, 1881, to September, 1882, of all the stations, together with the Bombay record from February, 1880, to June, 1881.

The addition of this series to the Zanzibar record seems to confirm the result of previous observations, viz. that there are abnormal movements of the atmospheric pressure which affect



a very wide area, and which are not simultaneous in all parts of that area, but travel from west to east. An inspection of the smoothed curve will show what amount of truth there is in this statement. Thus the Zanzibar curve shows an upward bend at A″ and a downward bend at B″, a second upward bend at C″, and a second downward bend at D″. The Bombay and Belgaum curves both show a similar general form, having upward bends at A and A′ corresponding with the upward bend A″ of the Zanzibar curve, downward bends B and B′ corresponding with the downward bend B″ of the Zanzibar curve, and similarly C, C′ and D, D′ corresponding with C″ and D″ of Zanzibar. And it is at once seen that the movements at Zanzibar are in advance of those of Bombay and Belgaum. Thus the Zanzibar maximum A″ took place in October, 1880, whilst the corresponding movements did not appear at Belgaum until the month of January, 1881, and at Bombay until between January and February,

1881; that is to say, at periods of three and three and a half months later. Then again the minimum movement B, B′, and B″ which occurred at Zanzibar in the month of May, 1881, did not appear at Bombay and Belgaum until the month of November of the same year; that is to say, after an interval of six months. Again, the maximum movement C, C′, and C″ occurred at Zanzibar in the month of September, 1881, but not at Belgaum until January, 1882, and at Bombay until February, 1882; that is to say, until after intervals of four and five months respectively. Again, on examining the minimum D, D′, and D″, which is divisible into two minor minima, the first of these minor movements appears at Zanzibar in the month of November, 1881, but at Belgaum between the month of April and May, and at Bombay in the month of April, 1882; that is to say, after intervals of five and a half and five months respectively. Lastly, the second minor movement of the minimum D, D′, and D″ occurred at Zanzibar in the month of February, 1882, and at Belgaum and Bombay in July of the same year; that is, after an interval of five months.

These facts may be presented briefly and concisely thus:—

TABLE II.

	An interval of		An interval of
From A″ to A′ ...	3 months	From A″ to A ...	3·5 months
,, B″ to B′ ...	6 ,,	,, B″ to B ...	6 ,,
,, C″ to C′ ...	4 ,,	,, C″ to C ...	5 ,,
,, D''_1 to D'_1 ...	5·5 ,,	,, D''_1 to D_1 ...	5 ,,
,, D''_2 to D'_2 ...	5 ,,	,, D''_2 to D_2 ...	5 ,,
Average from Zanzibar to Belgaum	4·7 ,,	From Zanzibar to Bombay	4·9 ,,

In the case before us, then, it does appear to be matter of fact that there are movements taking place at the two stations, Belgaum and Bombay, similar in character to movements which have taken place at Zanzibar on an average about five months previously. And assuming that the same course of events will occur in the future, it may be expected that from the month of August to the month of December, 1882, the abnormal variations of the barometer at Bombay and Belgaum will in a general way follow the same course as was taken by the variations at Zanzibar during the months of April, May, June, and July; that is to say, an upward movement.

This prediction might be considered fairly reliable to within about a month one way or the other, were there no modifying conditions. But the curves are seen at a glance to present most decided departures from absolute parallelism; there are movements at Zanzibar which do not reappear at the eastern stations, whilst the eastern stations experience movements which do not appear to have been previously experienced at Zanzibar. Moreover, the rate of transmission of movements from Zanzibar to the west of India has been shown to vary from three to six months. And further, the movements at the eastern stations are sometimes much less or much greater than those which took place at the western station. Evidently, then, there is some influence which tends to produce irregularities in the eastward transmission of the abnormal movements; and this influence must be discovered and its occurrence foreseen and allowed for before the Zanzibar curve could be used for the purpose of predicting the nature of the movements at Belgaum and Bombay, and, as a consequence, the nature of the seasons in Western India.

A second inspection of the curves seems to indicate that not only are there abnormal movements which travel from the western station to the eastern ones, but there are also variations which are felt at all the three stations simultaneously. Thus in the months of July, 1880, June, 1881, and January, 1882, there are simultaneous upward bends of the curves at all the three stations. And again in the months of May, August, and November, 1881, there are simultaneous downward movements at all the three stations. These simultaneous movements are especially observable if the unsmoothed monthly abnormals (the thin dotted lines) be referred to instead of the smoothed curve (the thick continuous line). They are then seen to be exceedingly numerous—so numerous, indeed, that they may well be supposed to frequently mask the non-simultaneous or travelling movements, and cause those movements apparently to present many irregularities. The following table shows concisely the times when upward and downward movements have taken place at all the three stations simultaneously:—

TABLE III.—*At Zanzibar, Belgaum, and Bombay simultaneous Barometric Abnormal Movements*

Occurred in an upward direction in	Occurred in a downward direction in	Cannot be easily traced in
June 1880	May 1880	February 1880
[1] July ,,	November ,,	March ,,
October ,,	January 1881	April ,,
December ,,	March ,,	August ,,
February 1881	[1] May ,,	September ,,
[1] June ,,	[1] August ,,	April 1881
[1] September ,,	October ,,	July ,,
December ,,	[1] November ,,	March 1882
[1] January 1882	[1] February 1882	April ,,
	September ,,	May ,,
		June ,,
		July ,,
		August ,,

Thus out of thirty-two months there were nineteen in which it can be seen that simultaneous movements occurred at the three stations; and out of these nineteen months there were eight in which the movements were very distinct. In the remaining eleven months out of the nineteen the movements were not so prominent or well marked, but were always distinct enough to be readily recognised, and it does not seem unreasonable to suppose that the influence of such movements may have been felt in some if not all of those months in which they cannot be easily traced; that in fact the simultaneous movements may have been so small as to show themselves only in an excessive or deficient movement, upward or downward as the case may have been, of the curve which represents the non-simultaneous or travelling movements. In any case eight of these movements appear to be sufficiently distinct to disallow of doubt; and considering that Zanzibar is about 2500 miles distant from Belgaum, the fact seems to be interesting.

A. N. PEARSON,
Acg. Meteorological Reporter for Western India

Bombay, January 10

(*To be continued.*)

[1] In these months the movements are very distinct.

THE INSTITUTION OF MECHANICAL ENGINEERS IN BELGIUM

THE Institution of Mechanical Engineers has this year held its summer meeting in Belgium—the first time that it has crossed the Channel, except on the two occasions of the exhibitions in Paris. The reception was organised by the Association of Engineers from Liége University (Honorary Secretary, M. Édouard de Laveleye), and was of the most cordial character. The great works of Belgium were thrown open without reserve, and numerous excursions were organised to visit them. Amongst those specially to be noticed are the colossal establishment of the Cockerill Society at Seraing, the great iron and steel works at Ougrée and Sclessin, the vast zinc works of the Vieille Montagne Company, the cloth factories at Verviers, and the splendid collieries of Mariemont, probably the finest examples of colliery plant in the world. Space forbids our entering into a description of these works, and we shall confine ourselves to the papers read, so far as these possess more than a technical interest.

The proceedings opened on Monday evening, July 23, with a reception by the Mayor of Liége, after which the president, Mr. Percy Westmacott, delivered an interesting and suggestive address. After speaking of the great modern extension of Belgian industries, and of the debt which the world owes to the inventive skill of the engineer for providing those processes on which all trades are dependent for cheap and rapid production, he went on to develop his special theme, namely, the advantage of High Speed and its connection with high workmanship. The following extracts are well worth quoting:—

"The keen and continual attention bestowed upon the work to be done, and the means of doing it, has led engineers in general to regard speed of production as one of the first elements of success. There is indeed a proverb, 'more haste, less speed;' but this, though true of human labour, which ceases to

be accurate when forced beyond a certain rate, does not hold good of mechanical processes. Generally it may be said that rapidity of working not only reduces cost but improves the result, and also confers great benefits from the way in which it brings out and perfects the highest qualities of the engineer. To be able to do a thing leisurely and quietly simply requires the rudest materials and the rudest workmanship; but if work is to be done quickly, or the appliances made to move quickly, the case alters. Mechanical energy increases as the square of the speed; and so it may be said that the mental energy and skill required to carry on work increase also at something like the square of the speed with which that work is performed. The materials used must be far stronger and far finer; everything must be well proportioned and balanced; there must be the most perfect arrangement in each structure and in every part of a structure, and the most perfect workmanship in the fitting of those parts together; and thus we may almost reverse the proverb, and say of mechanical processes, 'The higher the speed, the better the work.'

"The torpedo boat is an excellent example of the advance towards high speeds, and shows what can be accomplished by studying lightness and strength in combination. In running at 22½ knots an hour, an engine with cylinders of 16-inch stroke will make 480 revolutions per minute, which gives 1280 feet per minute for piston speed; and it is remarked that engines running at that high rate work much more smoothly than at slower speeds, and that the difficulty of lubrication diminishes as the speed increases: doubtless the experiments on friction which are now being conducted by this Institution will throw light upon this subject.

"An important experiment on high speed in light vessels, which will doubtless be watched with much interest, is now being carried out. Mr. Loftus Perkins is building a steel vessel with a screw at each end: she is 150 feet long; her boiler pressure will be about 800 lbs. per square inch, and she has a four-cylinder compound condensing engine of 800 h.p. working on to a single crank, and making from 400 to 500 revolutions per minute. When this vessel is laden with 300 passengers, her total weight will not much exceed 150 tons. Should this experiment be successful, it will materially advance the solution of the problem, how to put the largest possible amount of propelling power into a vessel, and so to drive her at the highest possible speed.

"Again, in touching upon speeds, the mind naturally reverts to railway travelling. Here, however, it would seem as if for the present we had reached a maximum. It is surprising how soon the speed of the locomotive was brought up to something approaching its present limit. George Stephenson was laughed at in 1825 for maintaining that trains would be drawn by a locomotive at twelve miles an hour, but the 'Rocket' herself attained a speed of twenty-nine miles an hour at the Rainhill competition in 1829, and long afterwards ran four miles in four and a half minutes. In 1834 the average speed of trains on the Liverpool and Manchester Railway was twenty miles an hour; in 1838 it was twenty-five miles an hour. But by 1840 there were engines on the Great Western Railway capable of running fifty miles an hour with a train and eighty miles an hour without. In 1841 we find Stephenson himself ranged on the side of caution, and suggesting that forty miles an hour should be the highest regular speed for trains. In 1851 Mr. Crampton, who had already in 1849 inaugurated the express service of the Continent on the Northern Railway of France, conveyed a train twenty miles in nineteen minutes, four miles in the journey being at the rate of seventy-five miles an hour. Thus, it is a remarkable fact that the highest speed at which locomotives run in ordinary practice scarcely seems to have been raised during the last thirty years; on the other hand, the weight of the trains has been perhaps doubled.

"What are the causes which have tended to prevent any improvement in this particular? In the first place it may be said that the permanent way would suffer seriously by further increase in speed; but this could surely be overcome in time by improving the permanent way itself, which also remains very much in the same condition and of the same construction as it was twenty-five years ago. Again, it may be said that the running at a higher speed would require more powerful engines, and hence that trains now worked by a single engine would require two, or would have to be split up into two trains at a great increase in running expenses. This, however, assumes that it is not possible so to improve the engine that it shall be able to exert a considerably higher power without an inadmissible increase in weight. By utilising a larger part of the total weight of the engine as adhesion weight it would be easy to obtain the amount of adhesion required for the increased tractive force; and for this purpose Mr. Webb's compound locomotive (to be described by the author in a paper he has prepared for this meeting) which enables the number of driving wheels to be increased without the use of coupling-rods, appears to merit particular attention.

"Another point in which improvement may possibly arise in the future should be noticed. On the Russian railways, where both coal and wood are dear, the burning of petroleum has now taken a practical form. Our member, Mr. Thomas Urquhart, has been very successful in this direction, and is now running locomotives regularly which use only petroleum refuse, and which show a marked economy over coal or wood. To test the point he prepared three locomotives of exactly the same type, and started them on successive days under exactly similar conditions of weather, train, and section of road. The trips were made both ways, and the results per verst, including fuel required in lighting up, were as follows:—

	Copecks.
Anthracite, 52·9 Russian lbs., cost	26·35
Wood, 0·0107 cubic sashin, cost	23·54
Petroleum refuse, 27·36 Russian lbs., cost ...	11·64

"There is thus in this instance an economy of at least 50 per cent. on the side of petroleum, the boiler pressure being from 120 lbs. to 130 lbs. and the gross load over 400 tons. At the same time the weight of fuel used, as against coal, is diminished by about 50 per cent., which is a most important item.

"Although petroleum is scarcely a product of Western Europe, we have to notice on the other hand the progress which has lately been made in the extraction of oil as a waste product from coal, &c. Mr. Jameson has extracted as much as nine gallons per ton from mere shale. It is suggested that markets for such oil will be difficult to find; but it seems allowable to hazard the idea that we may hereafter see our locomotives, even in England, running with oil fuel, which would be at once much lighter and much more easily renewed than the coal which is used at present, and get rid of the intolerable nuisance of smoke and dirt. There might in fact be an oil tank and a water tank side by side at every stopping station, and the engine would replenish her store of fuel at the same time as her store of water.

"Another point in which speed and perfection of workmanship have gone hand in hand is the important industry connected with textile fabrics. When Arkwright first brought his inventive mind and mechanical skill to bear upon this subject, the tools he had to work with were rude compared with the tools of the present day, and could not produce the accurate work now attainable; and therefore the speed at which he was able to drive his spindles was not remarkable. But our member, Mr. John Dodd, of Messrs. Platt Bros., informs me that the average speed of mule spindles at Oldham, in new mills with new machinery, and spinning No. 32 yarn from American cotton, is about 8500 revolutions per minute; whilst speeds as high as 9500 or even 10,000 revolutions have been attained. When we consider the delicate nature of the material under treatment, the disastrous result of the slightest hitch or unevenness in working, and the perfection of mechanism required to bring up a multitude of spindles to such a speed from that of the comparatively slow main shaft of the mill, we may give every credit to the constructive skill which has achieved such a result. In woollen mills (of which we hope to see some excellent examples at Verviers on Thursday next) the speed is 4000 revolutions per minute. The progress made here has not been so great, mainly, in Mr. Dodd's opinion, from wood being still adhered to as the material for the bobbins. Here therefore is a case where improved material may yet produce improved speeds; but with cotton Mr. Dodd considers that the extreme possibilities as to speed have been very nearly attained. The limit however is imposed by the feebleness of the material, not by any lack of skill or enterprise on the part of the engineer. 'If higher speeds were required,' says Mr. Dodd, and I fully believe him, 'we could make spindles which would be equal to the demand.'

"The construction of modern artillery, and with still greater justice the methods of employing it, may properly be brought under the scope of this address. I doubt whether of late years any mechanical appliances or arrangements have given greater impetus to skilful work and to the improvement of materials, especially of steel. Twenty-five years ago the largest piece of ordnance in use was a gun weighing 4¾ tons, firing with a maximum charge

of about $15\frac{1}{2}$ lbs. of powder a ball of 66 lbs., and made of cast-iron, a treacherous material for such purposes. We have now guns built up on well understood mechanical principles, of the most trustworthy and suitable material known, weighing 100 tons and firing with charges of 772 lbs. of powder shells of 2000 lbs. Already considerable experience has been obtained with guns of this weight. No fewer than fourteen have been issued from the Elswick Works, and several more are in the course of construction.

"Perhaps the most interesting feature in these formidable pieces of ordnance is the ease, rapidity, and noiselessness with which they are worked. It is of course impossible that such ponderous pieces could be brought into practical use without the aid of some mechanical appliances; but it is scarcely an exaggeration to say that nothing can work with greater precision and ease and be better under control than the hydraulic machinery employed for opening and closing the breech of the gun, ramming home the charge, elevating or depressing, running in or out, and training with accuracy on a given object. Two men working levers perform all these operations, and they, together with the machinery, are under complete protection from an enemy's fire.

"The projectile when fired has an energy imparted to it equal to nearly 48,450 foot tons, yet the gun is under such entire control that its recoil, due to this enormous force, is completely absorbed in a distance little exceeding three feet, without undue strain to any part of the mechanism. When it is remembered that the internal dimensions of the costly turrets in which guns of this size are ordinarily mounted depend mainly upon the space allowed for recoil, it is clear that it is of very great importance to reduce this space to a minimum.

"The fact which lies at the basis of these results is of course this, that the attainment of a high speed requires a more perfect machine, and with a more perfect machine more perfect work is turned out.

"In conclusion, it should be remembered that high speed, especially the speed of rotation, is almost necessary to give perfect accuracy and steadiness to motion, as in the case of an ordinary spinning top, of a gyroscope, and again of the ingenious centrifugal machines now in use for separating cream, &c. The speeds which we find in Nature are beyond all conception high, and her operations under those speeds are absolutely true and perfect. We cannot hope to vie with Nature even to an infinitesimal fraction of her powers of speed and accuracy; but in this, as in many other great lessons taught by her, we see the direction in which we must travel in our efforts towards the perfection of work.

"Finally, it is unfortunately a necessity that nations should still provide themselves with materials for war; and engineers have to devote their minds to the perfecting of such materials. It does not seem impossible that projectiles may be gradually developed, of such precision and devastating power as to make the existence of life within a certain range well nigh impossible. Were this accomplished, it is clear that nations would hesitate more and more before rushing into a war so destructive; and even if they did so, its rapid termination would unquestionably go far to diminish the various miseries which war always brings in its train. Hence it may not unfairly be said that the attention and skill given to the arts of war is really our best warrant for the continuance of peace."

On the next morning the papers read were on the "History of the Coal and Iron Industries in the Liége District," by M. Edouard de Laveleye, and on the "Manufacture of Zinc in Belgium," by M. St. Paul de Sinçay. The first of these was generally of an historical character, giving many interesting details as to the development of collieries and ironworks in Belgium. A claim was put in on behalf of Belgium for two most important discoveries in the metallurgy of iron, namely, the blast furnace and the cementation process. With regard to the present position of coal-working in this district, it was observed that all the difficulties which generally beset the mining of coal have to be encountered in their severest form. The chief of them—fire-damp—is nowhere so destructive, though its effects have been to a great extent obviated by the introduction of the Davy lamp and afterwards the improved safety-lamp of Mueseler. This lamp will resist a current of air of 15 feet per second, and has also the great property of self-extinguishment. In the recent disaster at L'Agrappe, which cost the lives of more than 100 miners, a sudden escape of gas issued from the shaft and burnt for several hours like an enormous gas-burner; but there was no explosion inside the mine, the 220 Mueseler lamps which were underground at the time having all been extinguished. Similar escapes of gas have taken place on other occasions and in enormous volume, without having previously given any indication of their appearance. Science appears to be powerless to prevent these disasters.

The second paper gave a sketch of the manufacture of zinc, which is a special trade in Belgium. Little was said as to the details of metallurgy, but it appears that the Belgian process, invented by Dony, of Liége, in 1810, is superseding all others, even in England. The difficulty and loss in reduction are, however, very great, and the labour is described as harder even than that of the puddler.

The third paper, by M. Mélin, was on "The Manufacture of Sugar from Beetroot," and formed a complete and exhaustive monograph on a manufacture of which but little is known in England. We regret that we can only give the briefest possible sketch of the processes. The beetroot, of which the cultivation was fully described, contains about 95 per cent. of juice in weight, and 5 per cent. of cellulose. These 95 parts of juice contain 10 parts of sugar, 2 of solid matter, and 83 of water. In manufacturing, the special point to be considered is the percentage of sugar, together with the purity of the juice. The manufacture is carried on in the winter only, and the beetroots are piled in silos until they are required for use. They are then washed, and are now ready for the extraction of the juice. For this purpose two systems are employed. On the first or hydraulic system, the roots are immersed in powerful rasping machines, and so reduced to pulp. This pulp is collected in sacks, which are piled up one upon the other between the table and the pressure head of a hydraulic press. The press is started, and acts with a pressure of about 450 lbs. per square inch on the pile of sacks, squeezing the juice through them. The dry pulp is used for feeding cattle, and is of considerable value. On the second or diffusion system the beetroots are cut up by a cutting machine into small slices called *cossettes*. These are placed in cylindrical vessels with an opening at the top for charging, and another at the bottom for emptying. These vessels are filled with water, and the result is that a current of endosmosis takes place from the water towards the juice in the cells, and a current of exosmosis from the juice towards the water. These currents go on until equilibrium is produced; and if fresh water is substituted they begin afresh. In this way the whole of the sugar contained in the cells is gradually drawn out. On the other hand, the water passes from the more exhausted to the less exhausted cells, and thus gradually increases in richness. A number of such vessels are used, forming what is called a diffusion battery; but in each of them the process going on is the enriching of the juice on the one hand and the impoverishing of the slices on the other. The slices are finally pressed in order to remove the residue of juice, but this is never effected so completely as by the hydraulic method.

The next process is that of defecation, which consists in adding milk of lime to the juice, in order to neutralise the organic acids which are precipitated, and also to decompose the salts of potassium, sodium, and ammonia. The result is that the dark brown juice becomes perfectly clear and of an amber colour. The scum which floats on the top, and which contains much juice, is passed through filtering presses, and the dried cake is sold as manure. After defecation the juice is filtered, twice at least, through animal charcoal under a sufficient pressure. It is then evaporated and transformed into syrup in a series of three vertical vessels, of which the first communicates with the second, the second with the third, and the third with a condenser. Steam is admitted to the first, and passes through to the last; and, owing to the partial vacuum produced by means of the condenser, causes an evaporation of the juice in all three. The next process is that of boiling this group, so as to allow the sugar to crystallise. This goes on within cast-iron vessels under a high vacuum, and heated by steam at high pressure circulating through worms. After a certain amount of evaporation, crystallisation begins in the form of an immense number of small grains of sugar. To develop these grains syrup is pumped in at regular intervals and with great care, so that the crystals may grow steadily and may be large, regular, and hard. Finally the crystals are dried by ceasing to supply syrup and introducing a current of steam. After eight to ten hours the sugar is removed from the boilers, and placed in vertical turbines running at 1000 revolutions per minute. Under the action of centrifugal force the boiled mass is spread upon the outsides of

these turbines, which are perforated, and the syrup passes through the holes, while the sugar remains behind. This sugar is cooled, and is called sugar No. 1. The syrup is boiled over again so as to obtain a second sugar called No. 2, and by a similar process a sugar No. 3 is obtained. The time of crystallisation, however, increases greatly, and for syrup No. 3 it is as much as six months. The final residue is molasses, which contains a large proportion of sugar that cannot be reduced by boiling. It is sold to make alcohol, or subjected to osmosis, by which the salts contained are drawn off and replaced by water; the sugar is then revivified and rendered capable of being crystalised. The paper concluded by giving careful analyses of the juice and of the products in all the stages of manufacture.

The next paper read was "On the Application of Electricity to the Working of Coal-Mines," by Mr. A. C. Bagot. The writer described a system of electric signals replacing the old system of signalling from the bottom to the top of the shaft by a gong worked by means of a wire. Galvanised iron telegraph wire was found to form the best communication, and the most suitable batteries to be 12-cell Leclanché. The system used for signalling in underground haulage planes, which are frequently the scene of accidents, was also described. Electricity had also been applied to signal the indications of an anemometer placed in the return air-way up to the engine-room at the surface. By an arrangement of clockwork and revolving tape, the engineer obtains an automatic and continuous record of the speed of the main air current at any part of the mine. Lastly the telephone might be applied with advantage for hearing the action of the pump-valves in the pumping shaft, without having to send the sinkers down.

Electricity may, however, be brought to bear for other purposes in mines, such as illumination and transmission of power. For lighting the pit bank, powerful arc lamps are found very serviceable, and the ordinary staff of a colliery, after a week's instruction, is capable of maintaining the appliances in operation. Alternating high-tension machines are very unadvisable on account of the likelihood of accident, and the Edison low-tension machine is said to be the best that can be used. At Risca Colliery a cable is taken down the pit from a dynamo at the surface, and is connected with a series of Crompton incandescent lamps at the bottom. These give an excellent light, and greatly facilitate the work of the men about the bottom of the shaft. But Mr. Bagot's opinion is strongly against the use of electric lamps in the working stalls and faces; partly because such lamps do not, like safety-lamps, indicate the approach of gas, partly because the line-wires may easily be broken, and partly because the hewer requires to be constantly shifting his light. With regard to the transmission of power by an electric tramway, as now in use at Zankerode, the writer holds that small locomotives worked by steam or compressed air are at present far more economical; so that the question of electric haulage need not in his opinion be considered at present.

These latter opinions did not pass without challenge. M. Tresca, who was present, pointed out that there was another form of electric transmission, viz. by a fixed cable with a dynamo at each end. Where work had to be done at some special part of a colliery, especially on an emergency, he believed that this would be found a handy and economical system. At the mines of La Perronière power was thus conveyed to a distance of 500 metres, and with a useful effect of about 30 per cent. This, in spite of over-bold statements to the contrary, was about the utmost which at present could be obtained in practice. The various sources of loss in such transmission were enumerated as follows:—First getting up the speed from that of the motor engine to that of the generator; secondly, loss within the generator itself; thirdly, loss in transmission along the cable; fourthly, loss within the receiver; fifthly, loss in slowing down the speed of the receiver to that of the main shaft. These defects were all now fully recognised, and might gradually, he hoped, be overcome. With regard to the amount of power which could thus be transmitted, the well-known experiments of M. Deprez showed 5 to 6 h.p. But within the last week he had succeeded in transmitting 31 h.p. from a Gramme machine to a great distance. The facility of installation was a great advantage in this system of transmission. It was far superior to that by an electric locomotive, as to which at present he had little to say; but on the whole he was more firm than ever in the view that a negative conclusion with regard to the electrical transmission of power was at any rate premature.

The next paper was by Mr. Webb, of Crewe, upon "Compound Locomotive Engines." It described the system devised by him, and now carried out in several engines running upon the London and North-Western Railway.

The last paper read at Liége was on the St. Gothard Railway, by Herr Wendelstein of Lucerne. This paper gave an interesting description of the works of the railway, and of the Brandt hydraulic drill, which was used with great success for one of the spiral tunnels. It then passed on to the question of ventilation, which was very fully gone into. Tables were given of the temperature in the great tunnel during and after construction, together with an account of the observations made on the ventilation both of that tunnel and of the spiral tunnels. The subject is as interesting from a scientific as it is important from a practical point of view, the result being that artificial measures of ventilation, the necessity for which was fully discussed, are found to be wholly needless. We regret that space does not allow us to reproduce this part of the paper.

During a subsequent visit of the Institution to Antwerp, a paper was read by M. Royers, describing the great harbour works which are now being constructed at that city, especially the long quay wall which is being built far out in the river by a special system of floating cofferdam designed by Mr. Hersent. In addition to these papers a large number of notices of the various works to be visited, &c., had been prepared and were distributed at the meeting. We understand that copies of any of these, or of the papers above mentioned, may be obtained on application to the Secretary, 10, Victoria Chambers, Westminster, S.W.

GEOGRAPHICAL NOTES

In the *Transactions of the Berlin Geographical Society* (May-June) is an interesting paper by Herr Arthur Krause on South-eastern Alaska, or that strip of coast stretching from Mount Elias to Fort Simpson, comprehending about 120 miles breadth of continent, and the numerous islands lying alongside of it. Herr A. Krause passed the winter of 1882 with his brother at a factory to the north of the Lynn Canal, making short tours the following spring into the interior, as far as the Yukon district, and Herr Krause's paper is the result of his observations. The lower course of the Yukon River, as far as Fort Yukon, has been traced and astronomically observed by Raymond in his "Reconnaissance of the Yukon River, 1871." Its upper course and sources, on the other hand, have only seldom been visited by people of the Hudson's Bay Company and by gold seekers. The most important head stream is the Polly River, which springs from France's Lake on the west of the Rocky Mountains. From the south the Polly receives a powerful current, figuring in certain maps as the Lewis River. A northern offshoot of the Lynn channel cuts so deeply into the interior that in two short days' marches you can pass thence to the Yukon river. To the north of the Lynn Channel is the varied district of Chileat, forming the watershed between the coast and the Yukon river, and parting two distinct zones of flora and fauna. The Chileat district, like the whole of the west coast, is mountainous, and its peaks condensing the vapour driven by western winds from the warmer region of the sea, the whole western tract is distinguished by its violent falls of rain in summer and snow in winter, as also by its abundance of glaciers. Glacier Bay, to the west of the entrance of the Lynn Channel, is quite filled with ice in consequence of vast glaciers falling into it. All the valleys, too, along the coast abound in glaciers. As soon, however, as the watershed and the slope towards Yukon river are reached, the glaciers disappear. With this change also appears a corresponding change in vegetable and animal forms. The low banks and islands along the coast are covered with poplars, alders, willows, and thickets. Higher up on the slopes you meet a thick belt of pine. A few green trees of diminutive size, birch, maple, and mountain ash, may be observed, but these are mostly interwoven in the enormous thick underwoods of the pine forests. In some lower-lying spots an almost tropical luxuriance of vegetation surprises the traveller. On the inland side of the watershed the whole physiognomy of vegetation is in striking contrast with that on the sea side—is barer, drier, and lighter. Instructive particulars are also given by Herr Krause regarding the fur and fishing trades of this region.

In the *Bulletin of the Italian Geographical Society* for July is a paper giving a historic survey of the Harar district, Somaliland, by the Rev. P. Taurin Cahague. Great interest attaches to this place since Frederick Müller has shown that it forms a distinct ethnologic enclave allied to

the Semitic group of Abyssinia in the midst of the Hamitic populations of Somaliland. The town of Harar itself was never the capital of an independent kingdom, as has been wrongly stated by many writers, but simply a large emporium and station of great importance between the old Abyssinian empire and Massawa on the Gulf of Aden. Some years ago it was attached to the possessions of the Khedive, but on the withdrawal of the Egyptian troops the district was overrun by the fierce Oromo (Galla) people, who exterminated most of the old Amharic (Abyssinian) population.—In the same number is an editorial note, with illustration, on a human foot incised by the Bushmen of South Africa on a stone, which has been presented by Dr. Holub to the Society, and is now deposited in the Royal Prehistoric Museum, Rome.

THE general census of Japan, taken on the first day of the present year, gives the total population of the country at 36,700,110, made up of 18,598,998 males, and 18,101,112 females. The population of the larger towns is given as follows:—Osaka, 1,772,333; Hiogo, 1,418,521; Nagasaki, 1,204,629; Tokio, 987,887; Kioto, 835,215. To avoid erroneous conclusions it may be well to state that the figures here given are not the populations of the towns and cities mentioned, but of the administrative districts, locally known as *fu* or *ken*, bearing these names. In some instances, *e.g.* Hiogo and Nagasaki, these districts are as large as a medium-sized English county, and in all cases they include the towns and villages for several (from ten to thirty) miles around. Thus these statistics can by no means be accepted as data for the respective sizes of the towns. These would run, we believe, as follows: Tokio, Osaka, Kioto, Nagasaki, Hiogo; the two latter being smaller than probably a dozen other Japanese towns which might be mentioned—Nagoya, Sendai, Niigata, Kagoshima, Shimonoseki, &c. Statisticians should therefore receive these figures with the explanation here given.

AMONG the papers in parts 3 and 4 of the *Verhandlungen der Gesellschaft für Erdkunde zu Berlin* for the current year, we find one by Dr. Schwarz on Montenegro, the land and people; another by Dr. Uhle of Dresden on the divinity *Ba'ara Guru* of the Malays; and also some geographical sketches of Portugal by Herr Müller-Beeck.

SOCIETIES AND ACADEMIES

PARIS

Academy of Sciences, July 30.—M. Blanchard, president, n the chair.—Active or dynamic resistance of solids. Graphic representation of the laws of longitudinal thrust applied to one end of a prismatic rod, the other end of which is fixed (continued), by MM. de Saint-Venant and Flamant.—Experiments on the reproduction of albite (white shorl) in an aqueous medium, by MM. C. Freidel and Ed. Sarasin. From a composition of silicate of soda and albite (Na_2O, Al_2O_3, $6SiO_2$) in a temperature ranging from 432° to 517° C., abundant precipitates of albite were obtained in the form of minute particles, which appeared as fine needle-points and short thick crystals with facets distinctly visible under the microscope. Steel and platinum vessels strong enough to resist this high temperature were specially constructed by MM. Golaz, père et fils.—Separation of gallium (continued). Separation from vanadium, by M. Lecoq de Boisbaudran.—Experimental researches on the action of a liquid introduced by a special process into the tissues of the vine for the purpose of destroying phylloxera (continued), by M. P. de Lafitte.—Capacity of various soils for retaining water under conditions suitable for viticulture, by M. P. Pichard. Appended is a comparative table showing the various degrees of resistance offered to the infiltration of water by siliceous, argillaceous, calcareous, and other soils in the south-east of France.—On the integration of a certain class of partial differential equations of the second order with two independent variants, by M. A. Picart.—On the critical temperature and critical pressure of oxygen, by M. S. Wroblewsky. The critical point is approximately determined at −113° C.—A determination of the inward inert resistance of any electric system, independently of the disturbing action of its interior electromotor forces, whose number, seat, and size remain unknown quantities, by M. G. Cabanellas.—On the visibility of the ultra-violet rays, by M. J. L. Soret.—A silicophosphate of crystallised lime obtained by liberating phosphorus in the process of iron-smelting, by MM. Ad. Carnot and Richard.—On the artificial production of rhodonite (silicate of manganese) and tephroite, by M. Alex. Gorgeu. A new and easy method is explained for producing these two natural crystallised silicates of manganese based on the reciprocal action of silicium and the red chloride of manganese in aqueous vapour.—On the "chloride of menthylum" obtained by Oppenheim from menthol by the action of a concentrated solution of chlorhydric acid, by M. G. Arth.—Experiments on poisoning by the oxide of carbon, with a view to ascertain whether this gas passes from the mother to the fœtus, by MM. Gréhant and Quinquaud. The authors, who experimented on bitches, arrived at an opposite conclusion from Andreas Hogyes of Klausenburg, who experimented on rabbits, and who concluded that the fœtus remained unaffected by the poison which was fatal to the mother.—On the open epithelium ("cellule epithéliale fenêtrée") of the closed follicules of the intestine of the rabbit, and its temporary stomata, by M. J. Renaut.—Researches on the structure of the constituent parts of the vent in Cephalopods, by M. P. Girod.—Observations and experiments on the circulation of the sap in plants under the tropics, by M. V. Marcano. From the experiments carried on at Caracas, Venezuela (10° 30′ 50″ N. lat.), the author considers that in intertropical vegetation the cycle of circulation is completed within a period of twenty-four hours, presenting two *maxima* of relative fixity, and that the inner pressure of the sap is inferior to that of the atmosphere during the dry but far greater during the rainy season, a phenomenon attributed mainly to the water directly absorbed by the leaves.—On the differentiation and anatomic variations of the branches of forest and fruit-bearing trees, and some other plants, by M. Laborie.—On the action of silica on the growth of maize, by M. V. Jodin.—On the alterations produced by age on wheat-flour preserved in bins and sacks, by M. Balland.—Experiments on evaporation made at Arles during the years 1876-82, by M. A. Salles. In his remarks on this paper, M. Lalanne dwells on the great importance of the subject in connection with the projected inland sea towards the southern frontier of Tunis.—Observations on Part IV. of M. de Koninck's work on the carboniferous fauna of Belgium, by M. Hébert.

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THURSDAY, AUGUST 16, 1883

RECENT TRAVEL IN EASTERN ASIA

The Golden Chersonese. By Isabella L. Bird. (London: John Murray, 1883.)

Across Chrysê; being the Narrative of a Journey of Exploration through the South China Border Lands from Canton to Mandalay. By Archibald R. Colquhoun. Two Vols. (London: Sampson Low, 1883.)

Among the Mongols. By the Rev. James Gilmour. (London: Religious Tract Society, 1883.)

Eight Years in Japan, 1873-1881. *Work, Travel, and Recreation.* By E. G. Holtham, M.Inst.C.E. (London: Kegan Paul, Trench, and Co., 1883.)

WHEN Miss Bird last took leave of her readers, steaming away from the coasts of Japan, her labours and wanderings were by no means over. In this volume she takes up the thread of her narrative exactly where she dropped it in her last book, and we find ourselves with her again just where we parted before, and pay visits first to Hong Kong and then to Canton before starting for the newer ground. If this volume at all falls short in the interest and, we may say, importance of the last, it is owing alone to the Malayan peninsula falling so far short of Japan in both its interesting ancient and marvellous modern history. It is as fresh ground as Japan: for of the eastern half of the peninsula nothing is known but the coast line. Yet commerce promises to open it up; for the export and import trade of the Straits Settlements amounted together in 1880 to £32,353,000. Ironstone, containing 60 per cent. of metal, is said to be used for macadamising the roads at Singapore; and the vastest tin fields in the world are found in the western Malay States.

Miss Bird sailed from a leaden stormy sky on the Pacific into the sunny harbour of Hong Kong to find the town on fire, an incident giving early employment to her graphic pen. Her remarks that, whenever the rocks are quarried there, fever breaks out, is one on which further observations would be valuable. Miss Bird reports also that the Hong Kong hospital doctors have drugs which throw patients into a profound sleep, during which the most severe operations can be painlessly performed, and from which the patients awake without even a headache.

From Hong Kong she makes an excursion to Canton, where her "admiration and amazement never cease." We must remark that the simple exercise of the faculty of seeing seems to give an unusually intense pleasure to Miss Bird. Further on she describes the rough life she led as "very enchanting," even where she owns that the redundancy of insect and reptile life certainly was oppressive! The river population, though looked down upon by the land-dwellers, seem as usual to have been sharpened and improved by the struggle for existence; at any rate Miss Bird seems to prefer their women. Miss Bird stops in the neighbourhood of a Cochin Chinese village where river boats are more crowded than at Canton. Among other low characteristics of the "hideous" inhabitants she notes a wide separation of the great toe from the rest.

In the seas about Singapore there is "nothing scanty, feeble, or pale," while on land she finds a perpetual struggle between man and the jungle, and a power of vegetation which must be a source of wealth to the former when he has numbers and energy to control it. The average temperature there is 80° F., with no greater range in any part than 24°: moist and uniform. This moisture adds greatly to the oppressiveness of the heat—nowhere else did Miss Bird feel it so overpowering as in a canoe on a river at night—but our traveller is one who can feel mere living to be a luxury with the thermometer at 88°, and her powers of endurance are shown by the early collapse of one of the only two companions she made in any part of her journey, although the daughter of the Resident at Malacca. This town is now out of the line of traffic, and Miss Bird describes in equal wealth of words its monotonous silence and sleepiness, and the impression and fascination it produced upon her. It is only 2° north of the equator, and the journeyings which commence from here are in small territories on the west coast of the Malayan peninsula, only 3° further north.

The jungle there is not an entanglement of profuse and matted scrub but a noble forest of majestic trees, many of them supported at their roots by three buttresses, behind which thirty men could find shelter. On many of the top branches of these other trees have taken root from seeds deposited by birds, and have attained considerable size. Under these giants stand the lesser trees grouped in glorious confusion. A long list of such is given, all of which are bound together by the rattan with its tough strands from 100 to 1200 feet in length. An enthusiastic description of magnificent tropical flowers follows here; but elsewhere she reconciles her description with the different one which Mr. Wallace gives by remarking that "a traveller through a tropical jungle may see very few flowers, and be inclined to disparage it. It is necessary to go on adjacent rising ground and look down where trees and trailers are exhibiting their gorgeousness," "where indeed one has to look for most of the flowers." The silence and colourlessness of the heart of the forest, she tells us, and the colour, light, vivacity, and movement among the tree tops contrast most curiously. Even with the latter our masses of flowers, buttercups and daisies, gorse or heather, are compared favourably among very few things of home which are compared favourably with what she finds abroad.

Of the mangrove she notes that the seeds germinate while still attached to the branch—a long root pierces the covering and grows rapidly downwards from the heavy end of the fruit—which arrangement secures that when the fruit falls off the root shall become at once embedded in the mud; of the cocoanut palm, that in loose sandy soil near the salt water it needs neither manure nor care of any kind, but if planted more than two hundred yards from the sea it requires manure or human habitation, and that its fruit takes fourteen months from the appearance of the blossom till the ripe fruit falls; of the nutmeg that it grows like a nectarine on a tree forty or fifty feet high, with shining foliage. A ripe one open revealed the nutmeg, with its dark brown shell showing through its crimson reticulated envelope of mace, the whole lying in a bed of pure white, a beautiful object.

"The sensitive plant with its tripartite leaves, green

above and brown below, is a fascinating plant, and at first one feels guilty of cruelty if one wounds its sensibilities. Touch any part of a leaf ever so lightly, and as quick as thought it rolls up. Touch the centre leaf of the three ever so lightly, and leaf and stalk fall smitten. Touch a branch and every leaf closes, and every stalk falls as if weighted with lead. Walk over it and you seem to have blasted the earth with a fiery tread, leaving desolation behind. Every trailing plant falls—the leaves closing show only their red-brown backs, and all the beauty has vanished; but the burned and withered-looking earth is as fair as ever the next morning."

It is satisfactory to read that the elephant, so near extermination in Africa through the pursuit of the ivory trade, is still plentiful in these forest-covered interiors, though novelty seemed its only recommendation to Miss Bird as a beast of burden. Half a ton is considered a sufficient load for one if it be of metal, but if more bulky, from four to six hundredweight. In passing through the forest an elephant always puts his foot into the hole that another elephant's foot has made. They have the greatest horror of anything that looks like a fence; and a slight one made of reeds usually keeps them out of padi, cane, and maize plantations. The insect which can draw blood from the wrinkled hide of an elephant is curiously small. The boiled or stewed trunk of the latter, we are told, tastes much like beef.

A most tender account is given of the living and dying of a tame monkey, which Miss Bird believes to be an "agile gibbon—a creature so delicate that it has never yet survived a voyage to England"; and curiously human are the differences in disposition between different species of monkeys which she observes. When tamed by living with Europeans these apes acquire a great aversion to Malays.

Some small bright-eyed lizards which ran about her room went up the walls in search of flies. They dart upon the fly with very great speed, but just as you think they are about to swallow him, they pause for a second or two and then make the spring. "I have never seen a fly escape during this pause, which looks as if the lizard charmed or petrified his victim." The Malays have a proverb based upon this fact: "Even the lizard gives the fly time to pray." One evening Miss Bird found seventeen lizards in her room and two in her slippers!

A snake about 8 feet long has gained its name of a "two-headed snake" because after the proper head is dead the tail will stand up and move forwards.

An interesting account is given of a column of ants, officered by larger ones, making their way to the stump of a tree from which the outer layer of bark had been removed, leaving an under layer apparently permeated with a rich sweet secretion, which a quantity of reddish ants of much larger size and with large mandibles were engaged in stripping off. The large pieces which they dropped were broken up and carried away by the smaller ants round the base. Other proceedings which she describes seemed inscrutable to Miss Bird.

Among the gorgeous butterflies, Miss Bird describes one with the upper part of its wings of jet black velvet, and the lower half of its body and the under side of its wings of peacock-blue velvet, spotted; another of the same "make" but with gold instead of blue; and a third with the upper part of the body and wings white with erise spots. All these measured full five inches across their expanded wings. In one opening of the forest only she counted thirty-seven varieties of these brilliant creatures, not in hundreds, but in thousands, mixed up with the blue and crimson dragon-flies, and iridescent flies all joyous in the sunshine. Many birds rival them in beauty of plumage, though some resemble less brilliant European species.

The Malays are fond of animal pets; their low voices and gentle supple movements never shock the timid sensitiveness of brutes. A bird called a mina articulated so plainly that Miss Bird did not know whether a bird or a Malay spoke. Monkeys gather cocoanuts to order for their masters.

The Malays have an elaborate civilisation, laws, and even a literature of their own. They are a decently clothed, comfortably housed, settled, agricultural people, skilful in some arts, especially the working of gold, and they are rigid monotheists. Their houses show good work in lattice and bamboo, carved doorways, and portières of red silk, pillows and cushions of gold embroidery laid over exquisitely fine matting on the floors. Yet Miss Bird says that with no visible reason they have been dwindling away for several generations, and if they were swept away to-morrow not a trace of them except their metal work would be found. But nothing impresses itself so often or so strongly upon Miss Bird as the energy, enterprise, and large emigration of the Chinese. Most of her remarks about them might be thought to apply to the English; and indeed, so far from wishing to correct such an impression, she asserts that "to say that the Chinese make as good emigrants as the British is barely to give them their due. They have equal stamina and are more industrious and thrifty." Though the old hatred of foreigners in their native country does not pass away from them, and Miss Bird heard them mutter the phrase of "foreign devils" as she passed along the streets of Canton, yet the Chinese who are born in the Straits glory in being British-born subjects, and despise the immigrant Chinese. The principal result of British rule seems likely to be, from Miss Bird's account, that the Chinaman, striving, thriving, and oblivious of everything but his own interests, will soon overspread the whole of the Far East. Singapore is to all appearance a Chinese town, with 86,766 Chinese against 1283 European residents.

We think no one can help enjoying this happy traveller's book; though few would be led to think they would enjoy the same journey as thoroughly as she describes doing. One adjective fairly describes all her descriptions of what she meets with—they are superlative!

Mr. Colquhoun's object in undertaking the journey which he records in these two portly volumes, was to find a trade route from Rangoon through Burmah and the Shan States into South-western China. His attention was attracted to this subject by a previous journey to Zimmé or Kiang-mai on the Me Ping, and he accordingly decided to devote his first leave of absence from his official duties in India to attacking his task from the Chinese side. Briefly, then, he went up the Si Kiang, or Canton River, from its mouth to Pêsê, near the borders of Yunnan, and travelled through the southern districts of this province, passing the great towns Kaihua, Linan, and Puerh to Ssumao, immediately on the border of the independent

Shan States, the real goal of his journey. Here, where the most interesting part of his work was to commence, and when he had overcome many obstacles, he found himself compelled to abandon his plan by the refusal of his interpreter to proceed into these strange regions. It is not difficult to understand the bitter disappointment with which he turned northwards, when only a few weeks' journey from Zimmé, and passing almost across Yunnan to Tali, he took the usual route of Gill, Margary, and others through Manwyne and Bhamo, and thence by the Irrawaddy to Mandalay and British Burmah. It is to be hoped, not less in the interests of geography than of commerce, that Mr. Colquhoun may shortly be able to undertake the journey again, aided by the great commercial bodies of England; for we are bound to say that he exhibited throughout the journey many of the highest and most valuable qualities that a traveller can exhibit among strange peoples—patience in overcoming obstacles, unfailing good temper, tact in dealing with officials and with his own followers; and at the same time energy, industry, and skill in making and recording scientific observations. These volumes appear to have been written from day to day as the journey progressed, and this accounts for much repetition, and for an absence of arrangement which is none the less occasionally irritating. But how are we to account for the presence of illustrations in this important and scientific work of such hackneyed subjects as "Chinese Children," "Modes of Dressing the Hair," "Boats at Futshan," &c., such as may be found in any popular volume published on China during the last fifty years? They swell the size of the book, without in any degree adding to its interest or value. In fact, there was ample room for judicious pruning, and a single moderate-sized volume would have been sufficient to contain a full record of the journey, including the excellent maps, and the amusing sketches of the aboriginal tribes of Southern Yunnan. But we must not look our gift-horse too much in the mouth; and the faults to which we have adverted do not prevent Mr. Colquhoun's journey from being one of the most valuable contributions to our knowledge of the geography of China and its southern border-lands that we have had since Lagrée's adventurous journey up the Meikong and through Yunnan to the Yang-tsze about ten years ago. He appears to have settled the hydrography of many of the numerous rivers that flow from Yunnan through the Indo-Chinese peninsula, and his accounts of the various tribes inhabiting the southern borders of that province add much to ethnological knowledge. One fact, of great importance at the present time, which Mr. Colquhoun places beyond doubt is that the Songkoi River, which flows through Tonkin, and which the French regard as the future trade-route into South-western China, can never be used for that purpose with success. Its highest navigable point is cut off from the province by a range of lofty mountains, and when these are crossed, the district reached is a barren one. The real wealth of these regions appears to lie farther to the westward, about Puerh, Ssumao, and in the Independent Shan States, where the traveller found a busy and thriving trade. In the new journey which Mr. Colquhoun is about to undertake with more funds, and with other advantages which he did not possess last year, we are sure he will meet with the success which unfortunate circumstances then snatched from him at the last moment.

Mr. Gilmour's volume is one of the most charming books about a strange people that we have read for many a day. There is much deficiency in the matter of dates, but we gather that he commenced his missionary labours in Mongolia about 1870, and that he is still connected with the Peking mission. He lived amongst this nomad people as one of themselves. He learned the language in a manner that would have approved itself to the late Prof. Palmer, and then he travelled over the vast tract of country lying between the great wall of China on the south and the Amour on the north, sometimes joining caravans, sometimes alone, now staying in Mongol tents, now pitching his own tent on the confines of an encampment, from which the people came out to visit and hear him, or to get from him foreign medicines, which they expected to work extraordinary cures. In addition Mr. Gilmour has lived in towns such as Kalgan, on the southern frontier of Mongolia, Urga and Kiachta, and appears even to have once gone as far as Irkutsk. As a result he knows the Mongols from the inside; he has penetrated into their superstitions, their religion and habits of life, and he therefore is never compelled to hammer out a little substance to cover a large space. Indeed his wealth of material would in some hands have easily been extended to two portly volumes. Of geographical information there is very little, except an account of a journey across Mongolia from Kalgan to Kiachta, on the Siberian frontier; but the customs, religion, superstitions, &c., of the inhabitants of Mongolia are fully described, and the volume may thus be of much value to the ethnologist and student of comparative culture. It is in addition written in a simple and most amusing way.

The complaint that our books on Japan for the general reader are written by "globe-trotters" and travellers who have spent but a short time in the country is in a fair way of being removed. Mr. Holtham's is the second volume published during the past two years in which a resident on his return home has given the public the benefit of his experiences. Mr. Holtham was employed as an engineer on the Japanese railways. For the first two years survey work took him up country, but when the Japanese Government found they were exceeding their funds in various directions, the projected railways were abandoned for the time being, and Mr. Holtham was called in to administer one or other of the two small railways then in actual running order. One of these he extended slowly till it reached Kioto; the other he succeeded in relaying. The nature of the experiences of an engineer surveying for railways may be guessed with tolerable accuracy, but Mr. Holtham tells his story in a quaint and humorous fashion which, if a little strained now and again, is as a rule very taking. In addition to what may be called the professional section of the volume, there are also records of various journeys in the interior, but none of these are on unbeaten tracks; and interspersed everywhere we find interesting and amusing comments on what was going on under the author's eye in society and politics in Japan. It may be commended especially to readers who desire, from whatever motive, to know the conditions under which the scientific and professional

man works under the Japanese Government. Many of these are exceedingly irritating, among them being the incompetence and presumption of native colleagues, who are fond of proceeding in what Mr. Holtham styles "the rough and ready heaven-born-genius-and-see-it-with-half-an-eye kind of way" in cases where his old-fashioned education led him to seek first carefully for facts. The author passes over unpleasantnesses such as these in a very kindly way, but there can, we believe, be no question that many most important elements of the true scientific spirit are sadly lacking in young Japan. Energy, thirst for knowledge, and ingenuity exist in abundance, but we are not so assured of the patience, and caution in research, and respect for the opinions of older and more experienced heads, which are also necessary. Hence, doubtless, we find so many promising schemes come to nought. It is more satisfactory to find that, in Mr. Holtham's opinion, the students who have been so carefully trained under excellent foreign teachers in the Imperial College of Engineering give great promise of subsequent practical usefulness. The foreign staff of the Japanese Railway Department has now been almost wholly replaced by natives, and it will be very interesting to watch the Japanese walking alone. A few years will show how far they were justified in getting rid of the men to whom they owe their substantial public works. However this may be, we can cordially recommend "Eight Years in Japan" as a very interesting and amusing book.

ELEMENTARY APPLIED MECHANICS

Elementary Applied Mechanics. Part II. By Thos. Alexander, C.E., and Arthur Watson Thomson, C.E., B.Sc. (London: Macmillan and Co., 1883.)

IN this volume the authors have pursued the same course as that followed by Prof. Alexander in the first volume of his "Elementary Applied Mechanics," in giving an abundant commentary, illustrated by a large number of practical examples, of those parts of Rankine's "Applied Mechanics" which deal with transverse stresses and the shearing forces and bending moments on beams and cantilevers.

They have thus supplied a want which has long been felt both by teachers and students of a text-book which should treat applied mechanics in a way similar to that pursued in mathematical works.

The work before us is accurate and clearly written, and the explanations given are so full that it may be easily understood by any one whose mental powers are not so hopelessly deficient that he would be liable to incur responsibility for culpable homicide if he were to undertake to design or construct a bridge, or any sort of structure in which defects might be attended with risk to human life.

Thoroughly penetrated with the scientific spirit of Rankine's work, though happily with a more perfect acquaintance with the limits of average human intelligence, the authors have given at length the proofs of the formulæ belonging to this part of applied mechanics, and they have also examined carefully the various cases which occur owing to the different modes of loading a beam.

The results arrived at and the methods employed, many of which are new, have, in each case, been rendered more easy of apprehension by the addition of a solution of the same question by simple graphical methods, nearly all of which depend, by a proper change in the scale on which vertical ordinates are measured, on the use of an invariable parabolic segment which is to be carefully constructed beforehand in wood or cardboard, and employed throughout.

By this means complicated questions on beams with both a dead and travelling load, can be easily dealt with, and the curves of bending moment and maximum bending moment readily drawn.

The mathematics employed are of the simplest character, not extending, except in one or two instances, beyond elementary algebra, whilst those properties of the parabola which are employed are previously proved in the form of lemmas.

But excellent as is the theoretical exposition of principles in the book, we are disposed to attach even greater importance to the large collection of examples scattered through it, in which the facts and formulæ of the text are applied to well chosen practical examples.

It has been a great misfortune, which all teachers of the subject have deplored, that the writers of books on it have spared themselves the labour of compiling a set of numerical examples, which would enable students to obtain that grasp of it which examples alone can give, and at the same time afford them the assurance that the formulæ they have been studying have some practical significance.

Those which are scattered through this work are judiciously selected, and they are accompanied, when necessary, by hints for their solution. We set a high value on this feature of the book, and we believe that a student, even though otherwise unassisted, who should carefully read it and conscientiously work through the examples, would acquire a knowledge, theoretically sound and practically useful, of this part of applied mechanics which he could not gain with the same labour and in the same time from the study of any other book which has been published on the subject.

J. F. MAIN

OUR BOOK SHELF

Text-Book of Physics. By J. D. Everett, M.A., D.C.L., F.R.S. Illustrated. (Glasgow: Blackie and Son, 1883.)

THIS book of 300 pages well fulfils the author's intention of providing an elementary text-book which may especially serve as an introduction to the well-known work of Deschanel with which his name is associated. It is full of matter, which is presented to the reader in a thoroughly systematised and acceptable condition.

The definitions, we need hardly say, are excellent and well worthy of the reputation of one who has taken a prominent part in scientific definition and terminology. Indeed we have rarely seen the chief points of scientific interest so clearly explained as they are in this volume.

We give the following as a good illustration (p. 119):—

"Fuel is a reservoir of potential energy, inasmuch as its elements are ready, whenever opportunity is given, to unite with the oxygen of the air and develop a large amount of heat. The words 'whenever opportunity is given' require some explanation. . . . If we have a large stone lying near the edge of a precipice 1000 feet deep, the stone will not move over of itself, but is ready to fall when opportunity is given, and a trifling expenditure of work in moving the stone to the edge will enable it to descend to the foot with terrific violence. . . . In the firing of a gun there is a combination of illustrations of

the kind of action we are considering. First, a little work spent in pulling the trigger releases a strong spring in the lock, and brings about a smart blow with the hammer. This blow liberates the explosive energy of the percussion cap, which in its turn fires the powder. Thus we have a series of processes in which the running down of a small quantity of energy gives opportunity for the running down of a larger."

This is only one illustration out of many which might be given. In fine the student of physics will find in this volume an accurate and clearly cut map of the various districts for the more minute details of which he must of course be referred to other guides. B. S.

Formulaire Pratique de L'Électricien. Par E. Hospitalier, Première Année, 1883. (Paris: G. Masson.)

In his "Formulaire Pratique de l'Électricien" M. Hospitalier has supplied us with a work which cannot fail to be of value as a convenient book of reference. It is divided into five parts. In the first are stated as briefly as possible those general principles with which every one who is in any way connected with electrical matters should be familiar. In the second is given the derivation of the electrical and magnetic units, with which are tabulated all the arbitrary units that have been or are at all generally used. In the third chapter almost every instrument and method that may be employed for making any measurement which an electrician is likely to require is mentioned, and when necessary explained by a figure. Though it must have been difficult to decide what to include and what to omit, surely considering the growing importance of "diagrams" so very useful an instrument as Amsler's planimeter might have been mentioned.

The fourth chapter, which in quantity is equal to all the rest of the book put together, contains a large amount of miscellaneous information. After giving the usual mathematical tables and formulæ, and several tables of the physical properties of bodies, the author treats in succession of batteries, accumulators, electro-metallurgy, thermo-electricity, dynamo-machines, and motors, electric lighting, the telegraph, and telephone.

The fifth chapter consists of a few pages, in which the composition of various alloys, cements, and varnishes, and a few manipulative processes are described. This part might and no doubt will be improved. For instance, a troublesome process of amalgamating iron is given, but no mention is made of the well known property that sodium possesses of making mercury wet iron or platinum.

There can be no doubt that both in the laboratory and in the workshop this will be found one of the most handy and complete books of reference existing. C. V. B.

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts, No notice is taken of anonymous communications.*

[*The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to insure the appearance even of communications containing interesting and novel facts.*]

"Elevation and Subsidence"

In an article in last week's Nature on "Elevation and Subsidence" (p. 323), Darwin's theory on the formation of coral islands is contrasted with mine, and it is apparently assumed that my theory is opposed to subsidence in those regions of the ocean where atolls and barrier reefs are situated.

I am not aware I have ever contended for this, but as several writers have lately attributed this opinion to me it may be as well to restate the position. My view is that the characteristic form of barrier reefs and atolls is in no way dependent on subsidence; that subsidence is not the cause of their peculiar features; that these reefs may be met with indifferently in stationary areas, in areas of subsidence, and in areas of elevation; and that elevation and subsidence only modify in a minor way the appearance of these islands.

All naturalists will be willing to acknowledge that Darwin's theory is "simple and admirable," but I do not think it will be generally admitted by those who have carefully examined coral reefs in recent years that it "accounts satisfactorily for all the observed phenomena of coral growth."

According to the explanation given by Darwin, the foundations of the coral reef sink gradually, and the corals, as gradually, build up the reef to the level of the waves. In this way these interesting coral islands are slowly developed.

It appears to me that the chief phenomena of barrier reefs and atolls are more satisfactorily accounted for in another way:—

1. By a physiological fact—the very vigorous growth of the reef-forming species on the outer or seaward face of the reef where there is abundance of food, and the much less vigorous growth and even death of these species on the inner parts of the reefs and in the lagoons, where there is much less food, and where there are other conditions inimical to growth.

2. By a physical and chemical fact—the removal of lime in suspension and in solution from the inner portions of the reefs and from the lagoons, where much dead coral is exposed to the action of sea water containing carbonic acid—the result being the formation, the deepening, and the widening of lagoons and lagoon channels.

My theory is represented as demanding "290 volcanic peaks at the sea level in the Pacific coral area alone."

What I have endeavoured to show is that the sum of all the agencies at work above the sea tends to reduce volcanic cones down to twenty or thirty fathoms beneath the waves, and the sum of all the agencies at work under the sea level tends to build up volcanic cones to within twenty or thirty fathoms of the surface.

In both cases banks are formed on which reef-building corals grow and eventually develop into atolls. The nearer the summit of the cone is to the sea level, whether above or below it, the more rapid is the formation of the bank.

Atolls, as we now see them, should, according to Darwin's theory, be situated on the summits of gigantic pillars of coral, which are probably higher (or deeper) the greater the diameter of the atoll. These pillars should rest on volcanic cones or peaks of continental land; and in the "Pacific coral area alone" should mark the spots where "290 peaks" have subsided. Where are the soundings which corroborate this part of Darwin's theory?

John Murray

Challenger Office, 32, Queen Street, Edinburgh, August 6

In his article on "Elevation and Subsidence" (Nature, vol. xxviii. p. 323) Mr. Starkie Gardner has given some very interesting new illustrations, drawn from observations of his own upon lava-fields in Iceland, where it is his belief that tracts have sunk owing to the mere weight of lava poured over them. But there are several places in his reasoning upon the general question of the condition of the earth's interior where he appears to argue upon the supposition that pressure by itself can be the cause of heat, and consequently of an increase of temperature among deeply buried rocks. This, however, as those who are acquainted with the science of energy will know, is clearly a mistake. It is only where pressure has produced motion, and that motion has been destroyed as visible motion in a mass of matter, and transformed into motion among the molecules of the matter, that the mass can be heated thereby.

Supposing that rocks at a considerable depth, and therefore under great pressure, are hot enough to be melted, it does not follow that the pressure is the cause either of the high temperature or of the fusion. We must look for some other cause for that high temperature and fusion, and this can only be guessed at. But it probably arises from the earth having once been an incandescent body—a little sun—which is now gradually cooling.

I have been led to make these remarks because Mr. Starkie Gardner has referred to a publication of my own with greater approbation than it perhaps deserves; but at the same time he says that the views he has put forward in his article present some important differences from mine. I wish therefore to be allowed to say that what I have now mentioned, and the consequences which he has deduced from it, are the only important points in which I should disagree with him. O. Fisher

Harlton, Cambridge, August 13

THE canal which it is proposed to make, connecting the Mediterranean and Red Sea *viâ* the Dead Sea and Gulf of Akaba, will, if carried out, throw considerable light upon the theory discussed by Mr. J. Starkie Gardner in your issue of August 2 (p. 323). The low-lying area which this scheme would submerge occupies the greater part of the Jordan Valley, and extends some distance to the south of the Red Sea, where the depression is at least 1300 feet. If there is any truth in the theory which ascribes elevation and depression to the denudation of rock from one area and its accumulation upon another, the introduction of such an immense weight of water from the Gulf of Akaba into the Jordan Valley will cause considerable subsidence in its vicinity. To what extent this would be the case it is difficult to say, but even a slight subsidence would much facilitate the cutting of the Mediterranean end of the canal.

Derby, Mill Hill, August 4 R. MOUNTFORD DEELEY

"The Speke and Grant Zebra"

ABOUT four months ago I wrote Mr. Joseph Thomson, the explorer who was selected by the Royal Geographical Society to examine the snow-clad mountains in Eastern Africa, and I requested him to look out for the "Speke and Grant" zebra mentioned in NATURE of April 26 last by Sir Joseph Fayrer, and I have had the following reply from Mr. Thomson, dated Mombassa, June 6, 1883:—

"With regard to your two questions I am happy to say that I can give you satisfactory answers.

"Within the last month I have seen hundreds of zebras, and I have shot three—one female and two males. The ground colour is *white* and the legs are striped to the hoofs. Of these facts I am certain, but to make quite sure I shall take care to note their characteristics in detail on my return. I did not know it was a subject of dispute."

The subject of dispute referred to is that the French zoologist, M. Milne-Edwards named a zebra after the President of the Republic, *E. Grevyi*, which appears to be no other than the animal which we shot twelve of in 1860-63. J. A. GRANT

19, Upper Grosvenor Street, W., August 10

The Fisheries Exhibition

THE allusion that you made to the marine invertebrates in our department led one of your scientific readers immediately to examine them. He was surprised to find them properly arranged, classified, and named, with a few exceptions. All the alcoholic specimens were looking bright and beautiful. The specimens of the marvellous Alcyonarian of British Columbia, *Osteocella*, Gray, or *Verrillia Blakeii*, as it is called by those who have sent it, are in a state of perfect preservation. They are not so well accommodated as I could wish, owing to their great length, 6 or 7 feet; still they are to be seen very distinctly, doubled up in a glass jar, 3 feet 5 inches in height, filled with strong alcohol clear as water. The fine specimen of *Cryptochiton stelleri*, collected and contributed by His Excellency the Marquis of Lorne, was also found by your reader to be properly exhibited in a convenient glass jar, and labelled inside and out. The large and interesting collection of marine invertebrates exhibited by the Government of the Dominion of Canada is formed of collections contributed by the Museum of McGill College, Montreal, Laval University, Quebec, and from the Nova Scotia Provincial Museum. The collection of Edible Mollusca was made by the late John R. Willis, of Halifax, N.S.

Canadian Department, I.F.E., D. HONEYMAN
August 7 Canadian Commissioner

Birds and Cholera

ALLOW me to relate an anecdote in point. I was with a regiment, to which at the time I belonged, in Mauritius, when that bright and beautiful isle was desolated by Asiatic cholera in the year 1854. It was the subject of common remark that during the prevalence of the epidemic the Indian Minah-bird or starlings—"martins" they used to be called in the island—abandoned, or seemed to abandon, the main barrack square and other open spaces they were wont to frequent in the neighbourhood of Port Louis, and were nowhere to be seen. These birds had been imported from India many years before, and were protected as destroyers of certain insect pests in the sugar-canes. They were correspondingly tame in their habits. Presently they betook themselves to the forest or Grand Bois, remaining in the centre of the little island; they could not have left by sea. They reappeared, or seemed to us to reappear, when the sickness passed away. Mauritius was then one of the stations where meteorological observations were systematically recorded. I rather think that the disappearance of the birds from the haunts of men during the epidemic and their reappearance when it ceased were duly noted by the Colonial Meteorologist, the late Col., then Lieut., A. B. Fyers, Royal Engineers, in his report. At any rate, I distinctly remember his noting another circumstance, viz. that the decline of the cholera mortality in the island, which was sudden and marked, was coincident with a marked change in the electric condition of the atmosphere at Port Louis, as indicated by the pith-ball electroscope.

I venture to suggest that the collection and investigation of trustworthy meteorological data during the prevalence of epidemics and of collateral information bearing thereupon has not yet received as much attention as it deserves from observers outside the medical profession. H. M. C.

August 10

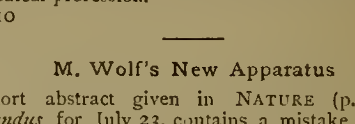
M. Wolf's New Apparatus

THE short abstract given in NATURE (p. 336) of the *Comptes rendus* for July 23, contains a mistake in respect to M. Wolf's paper "Sur un appareil à l'étude des mouvements du sol." It is stated that M. Wolf's apparatus involves the same principle as that by which my brother and I magnified the displacements of the vertical. This is not the case, since he uses an ingenious arrangement of reading by reflection from mercury. In the abstract in NATURE "sol" has been translated "sun" instead of "soil." G. H. DARWIN

Trinity College, Cambridge, August 9

Double Shadows

ONE cloudless evening lately, while walking on a hillside near the southern shore of Loch Etive, Aygyllshire, facing the setting sun, I observed each member of our little company cast a double shadow on the upward slope of the hill; first, the usual complete, well defined shadow cast in clear sunshine; and second, a longer fainter shadow of the upper part of the figure, extending for some distance in the same line beyond the first. The explanation was not far to seek. The loch beneath us was perfectly calm, and reflected the sun's disk with dazzling brilliancy. The second shadow was evidently produced by the reflected rays, thus:—



The phenomenon must be of frequent occurrence, but I do not remember seeing it noticed. I should add it was only observable for a few yards at a particular part of the hillside; a little higher or a little lower it ceased to be visible—doubtless because in the one case the reflected rays fell short, and in the other passed overhead. D. B.

Glasgow, August 2

Regnard's Incandescent Lamp

HERR VON PETERSEN, the engineer of the Zoological Station in this town, recently having occasion to use a powerful light, took advantage of the apparatus described in NATURE (vol. xxvi. p. 108) under the name of Regnard's Incandescent Lamp. He used the apparatus figured and described in NATURE, but neither with air forced through petroleum or benzine, nor even with gas forced through the same liquids, could he raise the platinum wire cage to more than a dull red heat, and the flame was never more brilliant than an ordinary Bunsen burner. The experiments were repeated several times with slight variations, but always with the same result.

I have written this letter at the request of Herr von Petersen, as you do not generally publish communications in a foreign language. ARTHUR E. SHIPLEY

Stazione Zoologica Napoli, July 26

Disease of Potatoes

THE *Sclerotia* referred to by Mr. Worthington G. Smith (in NATURE, vol. xxviii. p. 299) as having destroyed the potatoes in Norway have been sent to me from two different places on our western coast. As I usually travel every summer, I had no opportunity of cultivating them myself; so I sent them to Prof. De Bary of Strasburg, who kindly informs me that he has cultivated them with success. They belong to *Peziza sclerotiorum* (Lib.). The spores of our Norwegian Peziza will produce *Sclerotia*, as he has proved by experiment, also in *Daucus carota*, and very likely in *Phaseolus* and some other plants.

Christiania, August 6 A. BLYTT

Determination of "*H*"

SINCE the publication of a method for the determination of the value of the horizontal component of the earth's magnetism by Mr. A. Gray in NATURE, vol. xxvii. p. 32, I have worked out the value of "*H*" for my laboratory here, and from six sets of experiments carried out during the month of March in a small building constructed free from iron near the laboratory, I find "*H*" to equal 0·18365. The method proposed by Mr. Gray was closely adhered to throughout the experiments.

Taunton, August 8 FREDERIC JOHN SMITH

Fireball

ABOUT 8.25 p.m. on the 11th inst. my attention was suddenly attracted in the direction of the window of my dining-room, which looks south, by a brilliant ball of fire of a deep amethyst colour. It was travelling across the clear blue sky at the rate of about twenty miles an hour in an easterly direction and at an angle of 45°. Before disappearing behind a cloud it seemed to throw a few particles of itself forwards at a greater speed than that at which it was travelling. I trust others saw it under more favourable circumstances, and that they will communicate their experience of its course to you. CHARLES F. CASELLA

The Lawns, Highgate, August 14

Palæolithic Implements at Stratford

ALLOW me to say that a few weeks ago I found an abraded ochreous *oval* Palæolithic implement at *Stratford* (*in situ* two feet from surface). I have shown it to Mr. W. G. Smith, who says it is very interesting, as implements are rare in that locality, and especially oval ones, and he thought it as well for me to communicate with you, as it may interest some of your readers.

49, Beech Street, E.C. G. F. LAWRENCE

EARTH PULSATIONS

FOR many years philosophers have speculated as to whether the surface of the earth is really so stable as it usually appears. With the sudden and violent motions of our soil which we call earthquakes man has been familiar since the earliest times, and the origin of these disturbances has always formed a fruitful source of speculation. With the help of properly constructed instruments, our knowledge of the nature of these movements has during the last few years been greatly extended, and we are brought to the conclusion that these natural vibrations are propagated through the surface of our earth in a manner very different to that which we should have anticipated from our knowledge of elastic solids. Another order of earth movements which, in the hands of Timoteo Bertelli of Florence, M. S. di Rossi of Rome, and other Italian investigators, have recently received considerable attention, are *Earth Tremors*. From observations carried on during the past ten years it would appear that the soil of Italy is practically in a perpetual state of vibration, even in districts far removed from volcanic centres. On account of the smallness in the amplitude of these motions they are only to be observed with the aid of specially constructed instruments. Messrs. George and Horace Darwin, in connection with their experiments on the disturbance of gravity caused by lunar attraction, have shown that these movements are common to the soil of Britain. Like observations have been made in Japan, and it does not seem improbable that after further experiments have been carried out we shall be brought to the conclusion that the surface of the whole globe is affected with similar microseismical disturbances.

In addition to these minute movements, which escape the attention of the ordinary observer on account of the smallness of their amplitude, theoretical investigation has shown that there may be existing in the soil on which we live movements which have escaped our attention on account of the slowness of their period. These motions for want of a better term I call *Earth Pulsations*. Mr. George Darwin in his last report to the British Association has shown that movements of that nature may be produced by barometrical variation. A rise of the barometer over an area is equivalent to loading that area with a weight, in consequence of which it is depressed. When the barometer falls, the load is removed from the area, which in virtue of its elasticity rises to its original position. This fall and rise of the ground completes a single pulsation.

On the assumption that the earth is extremely rigid, Mr. Darwin calculates that if the barometer rises an inch over an area like Australia, the load is sufficient to sink that continent two or three inches.

The tides which twice a day load our shores cause the land to rise and fall in a similar manner. On the shores of the Atlantic, Mr. Darwin has calculated that this rise and fall of the land may be as much as five inches. By these risings and fallings of the land the inclination of the surface is so altered that the stile of a plummet suspended from a rigid support ought not always to hang over the same spot. There would be a deflection of the vertical.

In short, calculation respecting the effects of loads of various descriptions which we know are by natural operations continually being placed upon and removed from the surface of various areas of the earth's surface, indicate that slow pulsatory movements of the earth's surface must be taking place, causing variations in inclination of one portion of the earth's crust relatively to another. That pulsatory motions of this description have repeatedly been observed it may be shown that there is but little doubt. The magnitude of these disturbances however is so great that we can hardly attribute their origin solely to the causes which have just been indicated. Rather than seeking an explanation from agencies exogenous to our earth we might perhaps with advantage appeal to the endogenous phenomena of our planet. When the barometer falls, which we have shown corresponds to an upward motion of the earth's crust, we know from the results of experiment that microseismic motions are particularly noticeable.

As a pictorial illustration of what this really means, we may imagine ourselves to be residing on the loosely fitting lid of a large cauldron, the relief of the external pressure over which increases the activity of its internal ebullition; the jars attendant on this ebullition are gradually propagated from their endogenous source to the exterior of our planet. This travelling outwards would take place much in the same way that the vibrations consequent to the rattle and jar of a large factory slowly spread themselves farther and farther from the point where they were produced.

Admitting an action of this description to take place, it would then follow that this extra liberation of gaseous material beneath the earth's crust would result in an increased upward pressure from within, and a tendency on the part of the earth's crust to elevation. If we accept this as an explanation of the increased activity of a tremor indicator, then such an instrument may be regarded as a barometer, measuring by its motions the variations in the internal pressure of our planet.

The relief of external pressure and the increase of the

internal pressure it will be observed both tend in the same direction, namely, to an elevation of the earth's crust.

This explanation of the increased activity of earth tremors which is I believe due to M. di Rossi, is here only advanced as a speculation—more probable perhaps than many others. We know how a mass of sulphur which has been fused in the presence of water, in a closed boiler, gives up in the form of steam the occluded moisture upon the relief of pressure. In a similar manner we see steam escaping from volcanic vents and cooling streams of lava. We also know how gas escapes from the pores and cavities in a seam of coal on the fall of the barometrical column. We also know that certain wells increase the height of their column under like conditions. The latter of these phenomena may be added to that which we have already mentioned, as a result consequent on diminution of atmospheric pressure, which, by its tendency to render an area of less weight, facilitates its rise.

The next question is as to whether we have any direct evidence of such heavings and sinkings in our earth's crust.

Although some of the proofs which are brought forward to show that slow pulsations like these are phenomena which have been repeatedly observed are unsatisfactory, taking them one with another they indicate that these pulsatory phenomena have a real existence.

Pendulums for instance which have been suspended for the purposes of seismometrical observations, have, both by observers in Italy and Japan, been seen to have moved a short distance out from and then back to their normal position.

This motion has simply taken place on one side of their central position, and is not due to a swing. The character of these records is such that we might imagine the soil on which the support of the pendulum had rested to have been slowly tilted and slowly lowered. They are the most marked on those pendulums provided with an index writing a record of its motions on a smoked glass plate, which index is so arranged that it gives a multiplied representation of the relative motion between it and the earth. As motions of this sort might be possibly due to the action of moisture in the soil tilting the support of the pendulum, and to a variety of other accidental causes, we cannot insist on them as being certain indications that there are slow tips in the soil, but for the present allow them to remain as possible proofs of such phenomena.

Evidences of displacements of the vertical which are more definite than the above are those made by Bertelli, Rossi, Count Malvasi, and other Italian observers, who, whilst recording earth tremors, have spent so much time in watching the vibrations of stiles of delicate pendulums by means of microscopes. As a result of these observations we are told that the point about which the stile of a pendulum oscillates is variable. These displacements take place in various azimuths, and they appear to be connected with changes of the barometer.

From this and from the fact that it is found that a number of different pendulums differently situated on the same area give similar evidence of these movements, it would hardly seem that this phenomena could be attributed to changes in temperature, moisture, and the like. M. S. di Rossi lays stress on this point, especially in connection with his microseismograph, where there are a number of pendulums of unequal length which give indications of a like character. The directions in which these tips of the soil take place, which phenomena are noticeable in seismic as well as microseismic motions, Rossi states are related to the direction of certain lines of faulting.

Bubbles of delicate levels when examined by a microscope change their position with meteorological variations, but Rossi also tells us that they change their position, sometimes not to return for a long time during a microseismic storm. Here again we have another phenomena pointing to the fact that microseismic disturbances are the companions of slow alterations in level.[1]

The more definite kinds of information which we have to bring forward, tending to prove the existence of earth pulsations too slow in period to be felt, are those which appear to be resultant phenomena of great earthquakes.

The phenomena that we are certain of in connection with earth vibrations, whether these vibrations are produced artificially by explosions of dynamite in bore holes, or whether they are produced naturally by earthquakes, are, firstly, that a disturbance as it dies out at a given point often shows in the diagrams obtained by seismographs a decrease in period; and secondly, a similar decrease in the period of the disturbance takes place as the disturbance spreads.

As examples of these actions I will refer to the diagrams which I have given in a paper on the "Systematic Observation of Earthquakes" in vol. iv. of the *Transactions of the Seismological Society of Japan.*

In a diagram of the disturbance of March 1, 1882, it seems that the vibrations at the commencement of the disturbance had a period of about 3 per second, near the middle of the disturbance the period is about 1·1, whilst near the end the period has decreased to ·46. That is to say, the back and forth motion of the ground at the commencement of the earthquake was six times as great as it was near the end, when to make one complete oscillation it took between two and three seconds. Probably the period became still less, but was not recorded owing to the insensibility of the instruments to such slow motions.

We have not yet the means of comparing together diagrams of two or more earthquakes, one having been taken near to the origin and the other at a distance. The only comparisons which I have been enabled to make have been those of diagrams taken of the same earthquake—one in Tokio and the other in Yokohama. As this base is only sixteen miles, and the earthquake may have originated at a distance of several hundreds of miles, comparisons like these can be of but little value.

The best diagrams to illustrate the point I wish to bring forward are those at the end of the paper just referred to. These are the results obtained at three stations in a straight line, but at different distances from the origin, of a disturbance produced by exploding a charge of dynamite in a bore hole. A simple inspection of the diagrams shows that at the near station the disturbance consisted of back and forth motions which, compared with the same disturbance as recorded at a more distant station, were very rapid. Further, by examining the diagram of the motions, say at the near station, it is clearly evident that the period of the back and forth motion rapidly decreased as the motion died out.

Then illustrations are given, as examples out of a large series of other records, all showing like results.

Although we must draw a distinction between earth waves and water waves, we yet see that in these points they present a striking likeness. Let us take, for example, any of the large earthquake waves which have originated off the coast of South America, and then radiated outwards, until they spread across the Pacific, to be recorded in Japan and other countries perhaps twenty-five hours afterwards, at a distance of nearly 9000 miles from their origin. Near this origin they appeared as walls of water, which were seen rapidly advancing towards the coast. These have been from 20 to 200 feet in height, and they succeeded each other at rapid intervals, until finally they died out as gentle waves. By the time these walls of water traversed the Pacific to, let us say, Japan, they

[1] Since my return to Japan in January, 1883, I may mention that I have commenced series of observations on earth tremors and earth pulsations, and on several occasions have observed very marked coincidences between barometrical depressions and these movements. Not only are these atmospheric changes accompanied with microseismic storms, but there are deflections in the stile of a pendulum, and changes in the position of the bulbs of delicate levels, which at such times can be seen with the naked eye to SURGE back and forth through a small range.

broadened out to a swell so flat that it could not be detected on the smoothest water excepting along shore lines, where the water rose and fell like the tide. Instead of a wall of water 60 feet in height we have long flat undulations perhaps 8 feet in height, but with a distance from crest to crest of more than 120 miles.

If we turn to the effects of large earthquakes as exhibited on the land, I think that we shall find records of phenomena which are only to be explained on the assumption of an action having taken place analogous to that which takes place so often in the ocean, or an action similar to that exhibited by small earthquakes and artificially produced disturbances if greatly exaggerated.

As a remarkable instance of such phenomena we may take the great earthquake of Lisbon on November 1, 1755. In Spain, Northern Italy, the South of France and Germany, Northern Africa, Madeira and other Atlantic Islands, the effects of the disturbance which created so much devastation in Portugal were also more or less severely felt as violent movements of the soil.

In other countries further distant, as, for instance, Great Britain, Holland, Norway and Sweden, and North America, although the records are numerous, the only phenomena which were particularly observed were the slow oscillations of the waters in lakes, ponds, canals, &c. In some instances the observers especially remarked that *there was no motion in the soil.*

Pebbly Dam in Derbyshire, which is a large body of water covering some 30 acres, commenced to oscillate as a strong current from the south.

A canal near Godalming flowed 8 feet over the walk on the north side.

Coniston Water in Cumberland, which is about five miles long, oscillated for about five minutes, rising a yard up its shores. Near Durham a pond 40 yards long and 10 yards broad rose and fell about 1 foot for six or seven minutes. There were four or five ebbs and flows per minute.

Loch Lomond rose and fell through about 2½ feet every five minutes, and all other lochs in Scotland seem to have been similarly agitated.

At Shirbrun Castle in Oxfordshire, where the water in some moats and ponds was very carefully observed, it was noticed that the floods began gently, the velocity then increased, till at last with great impetuosity they reached their full height. Here the water remained for a little while, until the ebb commenced, at first gently but finally with great rapidity. At two extremities of a moat about 100 yards long it was found that the sinkings and risings were almost simultaneous. The motions in a pond a short distance from the moat were also observed, and it was found that the risings and sinkings of the two did not agree.

During these motions there were several maxima.

These few examples of the motions of waters without any record of the motions of the ground at the time of the Lisbon earthquake must be taken as examples of a very large number of similar observations of which we have detailed accounts.

Like agitations it must also be remembered were perceived in North America and in Scandinavia, and if the lakes of other distant countries had been provided with sufficiently delicate apparatus, it is not unlikely that like disturbances would have been recorded.

The only explanation for these phenomena appears to be that the short quick vibrations which had ruined so many cities in Portugal had by the time that they had radiated to distant countries gradually become changed into long flat waves having a period of perhaps several minutes, and in countries like England these pulse-like movements were too gentle to be perceived excepting in the effects produced by tipping up the beds of lakes and ponds.

The phenomenon was not unlike that of a swell produced by a distant storm.

At Amsterdam and other towns chandeliers in churches were observed to swing. At Haarlem floods rose over the sides of tubs, and it is expressly mentioned that no motion was perceived in the ground.

At the Hague a tallow chandler was surprised at the clashing noise made by his candles, and this the more so because no motion was felt under foot.

At Toplitz the pulsation of the ground appears to have manifested itself in effects upon the springs. The flow of the principal spring was greatly increased. Before this increase it became turbid, and at one time stopped. Subsequently it became clear, and flowed as usual, but the water was hotter and more strongly mineralised.

At one or two places, as, for instance, in Britain, slight earthquakes were experienced. These, however, were local, and in every probability were secondary disturbances produced by the pulsations causing ground in a critical state to give way.

In this earthquake I think, then, that we have a clear case of the production of pulsations in the soil that were too slow to be felt by ordinary observers.

Motions like these might be called slow earthquakes, and it does not seem unlikely that they are the resultants of all large disturbances. When they accompany a large earthquake like that of Lisbon, their cause is evident. But when we see the waters of lakes and ponds oscillating, the bulbs of levels disturbed, and the plummet line of pendulums displaced, the reason of these phenomena are not so apparent. It would seem possible that in some cases pulsations producing these phenomena might have their origin beneath the oceans, or deep down beneath the earth's crust. Perhaps, instead of commencing with the snap and jar of an earthquake, they may commence as a heaving or sinking of a considerable area, which may be regarded as an uncompleted effort in the establishment of an earthquake or a volcano. The very fact that we know that volcanoes rising from deep oceans have in the first instance forced their way against a pressure of at least three or four tons to the square inch, indicates to us the existence of internal pressures tending to raise the crust of the earth, which pressures are infinitely greater than any of the pressures which we have upon the surface of our earth produced by tides and variations in the barometrical column. If we follow the views of Mr. Mallet in considering that the pressures exerted on the crust of our earth may in volcanic regions be roughly estimated by the height of a column of lava in the volcanoes of such districts, we see that in the neighbourhood of a volcano like Cotopaxi the upward pressures must have been many times greater than the pressures already mentioned—sea level being taken as the line of hydrostatic equilibrium. The chief point, however, is that beneath the crust of our earth enormous pressures exist tending to cause eruption; and farther, that these are variable. Before a volcano bursts forth we should expect that there would be in its vicinity an upward bulging of the crust, and after its formation a fall. Farther, it is not difficult to conjecture other possible means by which such pressures may obtain relief.

Should these pressures then find relief without rupturing the surface, it is not difficult to imagine them as the originators of vast pulsations which may be recorded on the surface of the earth as wave like motions of slow period similar to the motions in the outer area of a tract disturbed by a destructive earthquake.

That slow, undulatory motions or changes in the vertical do occur in the crust of the earth, whatever may be their origin, we have numerous phenomena which certainly admit of explanation on such an assumption.

In Switzerland from time to time we hear of oscillations in the waters of lakes known under the name of *Rhussen* and *Seiches.* These, it may be remarked, are common to the lakes and inland seas of many countries.

Other examples of what may have been a slow oscil-

lating motion of the earth's crust are referred to by Mr. George Darwin in his Report to the British Association in 1882. One of them was made by M. Magnus Nyrén at Pulkova, who, when engaged in levelling the axis of a telescope, observed spontaneous oscillations in the bulb of the level.

This was on May 10 (April 28), 1877. The complete period was about twenty seconds, the amplitude being 1"·5 and 2". One hour and fourteen minutes before this he observes that there had been a severe earthquake at Iquique, the distance to which in a straight line was 10,600 kilometres, and on an arc of a great circle 12,500 kilometres.

On September 20 (8) in 1867 Mr. Wagner had observed at Pulkova oscillations of 3", seven minutes before which there had been an earthquake at Malta.

On April 4 (March 23), 1868, an agitation of the level had been observed by M. Gromadzki, five minutes before which there had been an earthquake in Turkestan.

Similar observations had been made twice before. These, however, had not been connected with any earthquakes, at least—Mr. Darwin remarks—with certainty.

Like phenomena are mentioned by M. S. di Rossi, in his "Meteorologica Endogena."

Thus on March 20, 1881, at 9 p.m., a watchmaker in Buenos Ayres observed that all his clocks oscillating north and south suddenly began to increase their amplitude, until some of them became twice as great as before. Similar observations were made in all the other shops. No motion of the earth was detected. Subsequently it was learnt that this corresponded with an earthquake in Santiago and Mendoza.

Another remarkable example illustrating the like phenomena are the observations which were made on December 21, 1860, by means of a barometer in San Francisco, which oscillated, with periods of rest, for half an hour. No shock was felt, nor is it likely that it was a local accident, as it could not be produced artificially. On the following day, however, a violent earthquake was experienced at Santiago.

This brings me to the end of the few important illustrations of the phenomena of earth pulsations which I have at my disposal. With a little trouble I have no doubt that these might be greatly multiplied. As they stand, however, I think that they are quite sufficient to convince us of the existence of phenomena which hitherto have been almost entirely overlooked. That disturbances of the vertical are from time to time produced by long pulse-like waves can, with these examples before us, hardly be doubted. It must, however, be noted that they are of a different order to those phenomena which were so carefully sought for by the Darwins at Cambridge.

JOHN MILNE

Tokio, Japan

ON THE SUPPOSED HUMAN FOOTPRINTS RECENTLY FOUND IN NEVADA[1]

DURING the past summer various accounts have been published of the discovery of human footprints in sandstone near Carson, Nevada. The locality is in the yard of the State prison, and the tracks were uncovered in quarrying stone for building purposes. Many different kinds of tracks were found, some of which were made by an animal allied to the elephant; some resembled those of the horse and the deer; others were apparently made by a wolf. There were also tracks made by large birds.

The footprints occur in series, and are all nearly in the same horizon. Some of the smaller tracks are sharp and distinct, but most of the impressions are indefinite in outline, owing apparently to the fact that the exact surface on which they were made is not usually exposed.

[1] Abstract of a paper read before the National Academy of Sciences, at New York, November 17, 1882.

The supposed human footprints are in six series, each with alternate right and left tracks. The stride is from two and a half to over three feet in extent. The individual footprints are from eighteen to twenty inches in length, and about eight inches wide. The distance between the line of right-hand and left-hand tracks, or the straddle, is eighteen to nineteen inches.

The form and general appearance of the supposed human tracks is shown in Fig. 2, which is a reduced copy of one of the impressions represented by Dr. W. H. Harkness, in his paper before the California Academy of Sciences, August 7, 1882. The shaded portion was restored by him from other footprints of the series. A

FIG. 1.—Left hind foot of *Mylodon robustus* (after Owen). One-sixth natural size.

copy of this impression was given also by Prof. Joseph Le Conte, in his paper before the same Society, August 27, 1882.

The size of these footprints, and especially the width between the right and left series, are strong evidence that they were not made by men, as has been so generally supposed.

A more probable explanation is that the impressions are the tracks of a large sloth, either *Mylodon* or *Morotherium*, remains of which have been found in essentially the same horizon. In support of this view it may be said that the footprints are almost exactly what these animals would make if the hind feet covered the impressions of

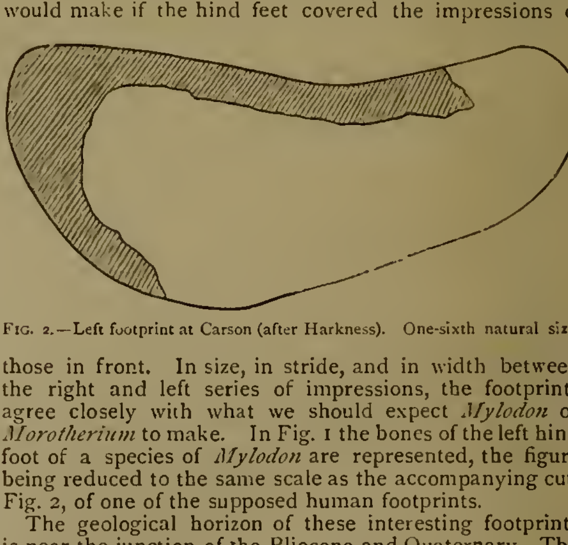


FIG. 2.—Left footprint at Carson (after Harkness). One-sixth natural size.

those in front. In size, in stride, and in width between the right and left series of impressions, the footprints agree closely with what we should expect *Mylodon* or *Morotherium* to make. In Fig. 1 the bones of the left hind foot of a species of *Mylodon* are represented, the figure being reduced to the same scale as the accompanying cut, Fig. 2, of one of the supposed human footprints.

The geological horizon of these interesting footprints is near the junction of the Pliocene and Quaternary. The evidence, at present, appears to point to the Equus beds of the upper Pliocene as the nearest equivalent.

Since the above communication was read, the writer has had an opportunity of examining photographs and casts of the Carson footprints, and is confirmed in his opinion that the supposed human tracks were made by large

Edentates. The important fact has recently been determined that some of these tracks show impressions of the fore feet. The latter are somewhat outside of the large footprints, as would naturally be the case if the animal changed its course.

O. C. MARSH

WINTER LIFE AT FORT RAE

IT was not until the beginning of December that our winter really set in, but when it did so there was no mistake about it, as the 1st of the month began with the thermometer at $-34°$, and except for some mild weather at Christmas, the cold continued through that month. January was colder still, the thermometer once or twice approaching $-50°$, but in the early part of February a violent storm was accompanied by a remarkable rise of temperature (to $+20°$), and followed by some mild weather, since which the thermometer has again fallen, reaching $-39°$ a couple of days ago.

This, however, I am informed by the inhabitants, is the mildest winter that has been known for many years, and I have no doubt that a temperature of $-60°$ is not uncommon in severe winters.

It is strange how much less one feels this extreme cold than might be imagined. For the first day or two it was unpleasant, but after that the system seemed to accommodate itself to it, so that a day when the temperature was anywhere above $-15°$ felt quite warm and pleasant. To-day, for instance, I am writing with my window open, although the thermometer is several degrees below zero, and there is a light breeze. There have been days, it is true, when—with the thermometer near $-30°$, and a strong breeze blowing, filling the air with snowdrift like a dense fog—outdoor exercise was most unpleasant, probably resulting in a frozen face, but such days were not very numerous, a strong wind, even from the cold quarter (the north-west), sending the temperature up in a way that I cannot quite account for.

Now the climate reminds me of Davos Platz, the sun having considerable power; there is, however, more wind. Yesterday the black bulb *in vacuo* read 82°. The only drawback is the intense glare from the snow, which makes coloured spectacles a necessity.

During the first part of the winter we were a little anxious about food, not that we were in any danger of starvation, as the Indians had brought in quantities of dried meat in the autumn, but dried meat is a most unpalatable article of diet, and requires strong teeth and a strong digestion; and then the fishery was not as productive as usual, and the daily produce of the nets (which are set under the ice) was gradually diminishing. At last, however, the deer made their appearance some forty miles from this, and since then our supplies of fresh meat have come in regularly. Rabbits, too, have lately become most numerous. These animals are the great resource of the Indians in times of scarcity, but they are not always plentiful. They are said to attain their maximum once in ten years, when they seem to suffer from a disease which shows itself in lumps on their heads; the following year there is hardly a rabbit to be seen, and then they gradually increase for another ten years.

The winter has passed very uneventfully. On November 17 and two or three following days there were magnetic disturbances of great violence, due, no doubt, to the large sunspot. The displays of aurora at that time, however, were not of any remarkable brilliancy; we have had far brighter ones since, with far less magnetic disturbance. But as a rule the auroras have not been remarkable, though a night seldom or never passes without more or less—the brilliant coloured ones one reads about are conspicuous by their absence. For the most part they are all of the same yellowish colour, showing the single characteristic bright line in the spectroscope, but a bright aurora usually shows more or less prismatic colouring along the lower edge, with a spectrum sometimes of one or two additional bright lines, as a rule towards the violet end of the spectrum, though on one occasion I observed a bright band in the red.

Aurora is very rarely seen until night has quite set in, but on three occasions we have seen it shortly after sunset, and on these occasions it was of a reddish or copper colour, as though partly coloured by the sun's light; it must, I think, have been associated with thin cloud. Its motion and shape showed it to be aurora.

The terrestrial radiation thermometer placed on the snow generally showed a depression of from 10° to 20° on every calm, clear day throughout the winter, even by day when sheltered from the sun. The lowest readings were, as might be expected, with the dry north-west wind. Sometimes the first warning of an impending change of wind to the south-east was given by a rise of this thermometer before the barometer was affected.

A thermometer suspended on the outer wall of the observatory at times read 9° or 10° lower than one in the screen, owing to radiation, and I think that the common practice of exposing unsheltered thermometers may explain some of the low temperatures sometimes reported from this country.

Our daily routine of observations goes on very regularly. Lately wolves have taken to prowling about the neighbourhood, and the observer on duty goes to visit the thermometers armed with a huge club; of course a gun or axe cannot be allowed near the observatory on account of the magnetic instruments.

A remarkable epidemic of influenza made its appearance here in January. We first heard of it among the Indians far to the north-west of this. When it arrived here it attacked every soul in the place—Indians and whites—fortunately in a very mild form, and we hear that Fort Simpson, on the Mackenzie, suffered in the same way. Such an occurrence is most unusual in this country. With this exception we have all enjoyed good health.

We expect the ice to break up about the middle of June, and then will come the reign of the mosquitoes, which make the summer the most disagreeable season of the year in this country. Fortunately they do not last long in this latitude, and by the end of August, when we set out on our homeward journey, they will be over.

Fort Rae, March 25

HENRY P. DAWSON

THE NORWEGIAN NORTH-SEA EXPEDITION[1]

II.

DR. MOHN continues his description of Jan Mayen Island as follows:—

"The northern part of Jan Mayen is larger and more elevated than the southern. From its central tract towers the monarch of the island, Mount Beerenberg, an extinct volcano, rising in regal majesty to the height of 6400 feet. The crater measures 4360 feet in diameter. The upper cone, which shelves at an angle of 42° and attains an altitude of about 2000 feet, would, to judge from the black spots so conspicuous on its western declivity, appear to be composed of ashes. The base supporting the cone slopes out in every direction at an angle of from 8 to 10 degrees, and this incline is retained towards the north and east to a depth of at least 1000 fathoms beneath the sea-level. The edge of the crater has a jagged appearance, and the loftiest peak lies on the west side of the mountain Towards the north the wall of the crater has partially given way down to a height of from 600 to 700 feet. The depression thus formed extends northwards towards the north coast of the island, bounded on either side by diverging mountain ridges, that here and there project ledge-like one above the other. This is Beeren-

[1] Concluded from p. 350.

berg's *val del bove*, which constitutes the snow-field for the largest of its glaciers, that jut out from the north side of the mountain. On the east side, too, are seen prominent ribs, all of which intersect the nevés of the east side; towards the south and west, however, the surface of the outer cone would appear to be remarkably smooth, at the edge of the crater only being furrowed with shallow depressions between the jags. The base of Mount Beerenberg shelves towards the west, south-west, and north-east, with a comparatively gentle incline, either to the water's edge or the low-lying shore; towards the north and east, however, the descent at the coast is very abrupt, exhibiting precipices 1000 feet high. In several places the base of the mountain is intersected by deep ravines, through which the glaciers find a passage to the sea.

"The height of the southern part of the island cannot be compared to that of the northern. The southern land constitutes a wide plateau, which, in a south-easterly and southerly direction, exhibits numerous precipices along the coast, but, towards the north-west, has extending before it a low-lying foreland, less than 300 feet above the sea. The height of the plateau I estimated at 1000 feet. Rising above this tableland are seen several summits; the loftiest, which has apparently a conical form, and may therefore be of eruptive origin, can hardly attain an altitude of 1600 feet above the sea-level.

"The low middle tract of the island, which is built up of compact masses of lava, and bears numerous eruptive craters, has at its lowest point an elevation of only 200 feet, or perhaps even less, whereas the crater summits reach a height of 400 to 600 feet. The altitude of Fugleberg we found by observation to be 490 feet; that of Egg Island was estimated at 400 to 500 feet.

Fig. 4.

"As shown by Carl Vogt, the base of Mount Beerenberg is composed partly of layers of lava, and partly of layers of tuff, that would appear to have flowed or been discharged from the great central crater previous to the formation of the upper cone of ashes. The middle tract of the island exhibits a similar structure, and, to judge from its appearance, also the southern part. Above this stupendous mass of lava rise a number of small parasitic craters, the greater part of which have retained a conical form. Such, for instance, are Sars's crater, the crater east of the southern glacier, the Esk and Vogt craters, Danielssen's and Blytt's craters, and the craters in the vicinity of Guinea Bay. Fugleberg on the west coast, and Egg Island on the east, are no longer conical, the outer edge of the crater having given way and fallen into the sea. Some of the parasitic craters are built up of lava, and would appear to have sent forth considerable currents, as the Vogt and Esk craters; the summit of others consists of loose erupted masses, cinders, and ashes (rapilli), as the craters in the vicinity of Mary Muss Bay and Guinea Bay; others are composed of layers of tuff, tuff-conglomerate, and compact masses of lava, as the Fugleberg, and others again of ashes alone, as Egg Island and the Berna crater.

"The chief volcanic fissure in which Jan Mayen Island is built must obviously extend in the longitudinal direction of the land, parallel to the volcanic line of Mount Hecla. Meanwhile, the grouping of the parasitic craters would seem to intimate the existence of transverse fissures running from W.N.W. to E.S.E.; for in that direction there are, apparently, several rows of parasitic craters, as the Esk, Vogt, Berna, the Fugleberg, and Egg Island, Hoyberg, and the crater in the vicinity of the 'pilot-boat' (?). Must we regard it as mere accident that each of the terminal craters towards the south-east in the two first rows should have discharged ashes alone?

"Jan Mayen has no valleys of considerable extent; the

large ravines in the northern part of the island are filled with glaciers, and the southern land would appear to be but little intersected by vales or ravines. Of brooks or rivulets very few have been observed. A characteristic feature, distinguishing the coast of Jan Mayen, are the fantastic-shaped rocks that in many places rise abruptly from the sea, of which we have mentioned several. They are no doubt in greater part fragments of lava detached from currents that had flowed into the sea.

"The coasts of Jan Mayen are, as previously stated, in many places lofty and precipitous. In some localities, however, there is a low expanse of foreshore consisting of lava, partially covered with sand. This foreshore, which is separately marked on the map, lies so low in places as

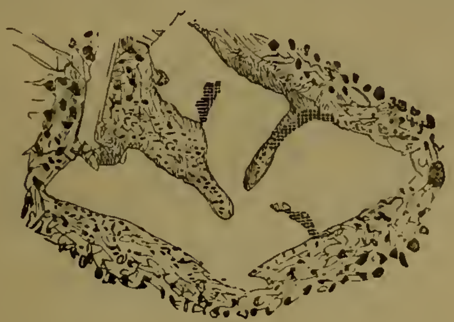


FIG. 5.

to be covered with driftwood. Some localities, too, exhibit a low sandy beach, bestrewn with large quantities of driftwood, the jaws and vertebræ of whales, bits of wreck, and seaweed.

"Nowhere on the shores of Jan Mayen has a harbour been found that could afford a ship or a boat shelter in all kinds of weather.[1] Hence, to land is possible only with the sea comparatively smooth, which it rarely is, save when drift-ice encompasses the island. Specially noteworthy are the two lagoons, cut off from the sea by barriers of black sand, only a few feet high and a couple of hundred paces broad. They both contain fresh water, the surface of which lies but very little above that of the sea. The lagoon on the west side of the island is deep enough to afford a good harbour were the barrier cut

FIG. 6.

through to a sufficient depth. The lagoon on the east side is comparatively shallow.

"Jan Mayen lies wholly within the Greenland Arctic current. At a depth of from 10 to 20 fathoms the temperature of the sea is all the year round below zero. In the winter there is frequently open water off the coasts of Jan Mayen, sealers often passing to the west of the island. The summer is naturally cold, from the presence of ice-cold water so near the surface of the sea. The northern part of Jan Mayen rises, at a height of about 2300 feet, into the region of perpetual frost. The upper cone of Mount Beerenberg is snow-capped, save on the steepest parts of its declivity, where the black mountain-wall is seen protruding. The base of Beerenberg is girt with a belt of snow, from which prodigious glaciers take their origin, nine of the largest reaching down to the water's edge. The southern part of the island would not appear to be glaciated. Large patches of snow are everywhere observed throughout the summer in the vicinity of the sea.

"Jan Mayen has but a meagre flora. Bright herbage, however, is not wanting; the green carpet of moss, in places of considerable extent, forms a striking and pleasant contrast to the black, brown, and red of the surrounding rocks. The plants collected by Dr. Danielssen

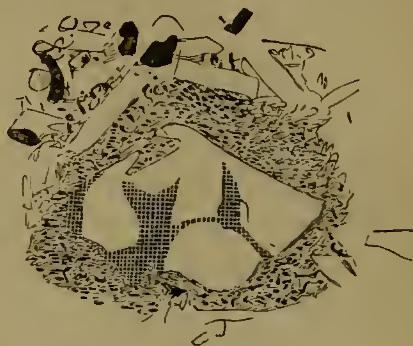


FIG. 7.

on the isthmus south of Mary Muss Bay, are, according to Prof. A. Blytt, as follows:—

"*Saxifraga cæspitosa*, L.
" *nivalis*, L.
" *oppositifolia*, L.
" *rivularis*, L.
Ranunculus glacialis, L.
Halianthus peploides, Fr.
Cerastium alpinum, L.?
Draba corymbosa, R. Br.
Cochlearia officinalis, L.
Oxyria digyna, Campd.
Catabrosa algida, Fr.

FIG. 8.

"Of mammiferous animals, the Polar Fox, *Canis lagopus*, is by no means rare on Jan Mayen. Of birds, Mr. Friele has noted the following species:—

"*Somateria mollissima*, Leach.—Rare.
Larus glaucus, Brün.—Common.
Fulmarus glacialis, Lin.—Exceedingly abundant.
Grylle Mandti, Licht.—Abundant.
Uria aara, Schlegel.—Abundant.
Mergulus alle, Lin.—Abundant.
Tringa maritima?

"If the land fauna of the island is meagre, that of the sea is proportionately rich, a fact which the numerous zoological memoirs published in this General Report will sufficiently attest."

[1] Little Sand Bay would appear, according to the account in the "Zeespiegel," to be a good harbour for boats, protected as it is by an outlying chain of islets.

Fig. 4 again shows the island in its winter garb, and is from a drawing made by Lieut. Ring, R.N., when commanding the sealer *Capella*.

"We have Sars's crater, on the slope shelving towards Cape North-East; we see, too, the great glaciers on the north side, also Cape North-West and Muyen's Cross Cape, in a line with the point of view; and the low tract of the island, with the heights of the southern part, are boldly defined in the picture. The crater of Beerenberg, with its sunken edge on the north side, is also seen, and lower down a huge, cauldron-shaped depression, from which the great northern glaciers take their origin."

Some very interesting mineralogical specimens were brought from Jan Mayen, on which Mr. H. Reusch, of the Norwegian Geological Survey, reports. We reproduce illustrations of four specimens of olivine in basalt.

In Fig. 5 the surrounding rock exhibits a remarkably fine granulation in immediate proximity to the crystals, which it pierces in sac-like ramifications. In Fig. 6 discoloured glass is seen piercing the crystal from the surrounding rock, which has a fine granulation. In Fig. 7 the surrounding base is finely granulated. At the top of the figure is seen basalt of the dominant degree of granulation. Discoloured glass pierces the crystal from the rock surrounding it. In Fig. 8 the iron ore occurring as rod-shaped corpuscles has a definite position towards the crystal of olivine.—Magnified 360 diameters.

Dr. Mohn concludes his instructive account of the geography of this fruitful expedition by some brief observations on Bear Island and Spitzbergen, at various points of which the *Voringen* touched.

SCIENCE AT CAMBRIDGE

WE understand that Dr. M. Foster, who, upon his appointment as Professor of Physiology at Cambridge, ceased to be Prælector at Trinity College, addressed to the Master of Trinity the following letter, which perhaps may interest those of our readers who are not acquainted with the peculiar organisation of our old Universities.

Shelford, Cambridge, July 28, 1883

MY DEAR MASTER,—The University having done me the honour to appoint me to the newly established Chair of Physiology, my connection with the College as Prælector comes to an end, though I rejoice that I am still counted among the Fellows of the Society. I cannot let this opportunity pass without making some attempt to thank you, and through you the College, for all you have done for me during the thirteen years of my Prælectorship. You called me, a comparatively unknown young man, to the College in 1870; you not only at once gave me leave to follow out my own views as to what I ought to do, but from that time onward have constantly supported me, not simply with cordial approbation, but also with most material assistance.

I have reason to believe that many persons not conversant with the organisation and working of the University, are under the impression that the necessary expenses which my work has entailed have been provided out of University funds. But I am sure that the authorities of the University would be the last to wish that anything done by the College should be considered as done by the University. And as a matter of fact, when I say that I was allowed the use for four years of one room, and for ten years of two rooms, in the University buildings, and that during the last three years I have enjoyed the advantages of the admirable laboratory which has been built for me, with use of gas and water, I have mentioned all that I have received from the University, with the exception of grant of microscopes to the late Prof. Balfour and myself in common. Not only my own remuneration has come from the College, but all the really large expenditure involved in my teaching physiology, save what has been met by the fees of the students, has been provided for in one way or another by the College.

At the outset the College gave me a large grant of money for apparatus, and some years afterwards a second smaller grant. During the whole thirteen years I have received from the College an annual sum for the payment of my laboratory servants; and for several years past two demonstrators (one at a comparatively high salary), as well as during the past year three assistant demonstrators, have been paid partly from the tuition fund of the College, partly by funds which, though furnished by private liberality, cannot be wholly dissociated from the College. I think I may fairly say that I have never asked anything of you in vain. I might add that what you have done for me did not prevent you from also assisting our lamented Balfour, working in a closely allied branch of science, or, upon his sad death, from affording material help in carrying on the work which he left behind through aid given to Mr. Adam Sedgwick.

Let me assure you that I fully appreciate all the College has done for me; but perhaps after all I feel still more keenly the sympathy and kindness with which as a stranger I was first received among you, and which have made the thirteen years of my Prælectorship the brightest as well as the best years of my life.

Yours ever truly,
M. FOSTER

THE ISCHIA EARTHQUAKE

A SLIGHT shock of earthquake occurred in Casamicciola at seven o'clock on Sunday morning, at the Gurgitello, where that of July 28 created the most ruin, but it was limited to that spot, and caused no damage. It is reported that a fissure a kilometre in length and thirty kilometres in depth has opened on the south-west flank of Mount Epomeo. The smoke ejected from the *fumaroli* at the summit of the mountain has considerably diminished in quantity. The Naples Academy of Sciences has appointed a Commission to investigate the telluric conditions of Ischia.

The following communication from the *Times* correspondent at Rome is important:—

"From a second report made by Prof. Michele Stefano di Rossi, head of the Central Geodynamic Observatory at Rome, to the Minister of Agriculture, on the phenomena connected with the earthquake in Ischia, it appears that not only were there for some days beforehand very distinct premonitory signs at Casamicciola of the impending catastrophe, but that throughout the peninsula forewarnings, identical in character, were numerous and widespread. On the island of Ischia there was an extraordinary increase in the temperature of the thermal waters and in the violence of the *fumaroli* (*i.e.* the natural smoke funnels) at the spot called Monte Cito. These phenomena were noticed eight days before the catastrophe occurred. On these important points the evidence which Prof. di Rossi obtained is abundant. There is less conclusive testimony concerning the shrinking and consequent scarcity of the drinking water in the wells. But he has absolutely certified that, commencing from a period a fortnight anterior to July 28, many slight shocks of earthquake, of almost daily recurrence, were felt, and subterranean rumblings were heard. Phenomena identical with these preceded the earthquakes in Ischia in 1828, 1851, and 1881; and Prof. di Rossi emphatically states that had an observatory been established in Ischia after the earthquake of 1881, according to the advice he then gave, and the phenomena which manifested themselves at Cassamicciola from July 20 onwards been communicated to him at the Central Observatory in Rome, he would not have hesitated an instant in pointing out the imminent danger of an impending

seismic disturbance. While the above-mentioned phenomena were occurring in Ischia, without their being communicated to Rome, or even, for want of means, properly noted on the spot, the existence of unusual subterranean activity was simultaneously marked by the instruments in all the observatories on the mainland. That activity, though varying in intensity in different places, manifested a general and regularly progressive augmentation. Slight shocks of earthquake were felt at various points. On July 25 the Solfatara of Albano, on the extinct Latin volcanoes on the southern side of the Roman Campagna, sent forth sounds never before remarked. On the same day a widely extended earthquake, reaching from Cosenza to Catanzaro, occurred in Calabria. On Friday, the 27th, the hissing noises from the Solfatara of Albano were so acute that the people did not dare to draw the sulphur water for those who needed it, and simultaneously the seismic instruments at Pesaro registered severe oscillations. At Vesuvius on the evening of Friday, the 27th, shocks were felt, with an augmentation of activity. There were shocks at Latera, upon the Ciminian volcanoes, and shocks at Perugia. On the afternoon of the 28th renewed activity was manifested at Pesaro and at Fermo; and in short the observations during that afternoon of general calm throughout the peninsula gave indications of a vast subterranean disturbance, extending as far as all Umbria, the district of Viterbo, and the Marches.

"The direction of these extended movements was everywhere identical with those at Casamicciola—namely, from north to south, and from east to west. At the same time also, on the morning of the 28th, the flow from the principal source of the sulphur streams, near Tivoli, showed a considerable diminution; while simultaneously an increased quantity of carbonic acid gas was given forth. The regular observations at Bologna, at Pisanello, near Piacenza, and at Rome, showed that there was a distinct lowering in the levels of the wells before July 28, and as marked a rise after that date. These facts confer increased credibility on the imperfect evidence of there having been a deficiency of water in the wells at Casamicciola. Moreover, on the morning of Sunday, the 29th, the usually very cold waters of the Solfatara of Albano were in a boiling state. The intimate connection between these phenomena on the peninsula and the catastrophe in Ischia is more than evident, and their distinct dynamic and volcanic character absolutely excludes the idea of a mere local sinking in the level."

NOTES

TELEGRAMS from Drontheim to Vienna announce that the members of the Austrian expedition to Jan Mayen have arrived there safe and well, after an absence of six months. This was one of the circumpolar observing parties, and during the year's residence on Jan Mayen neither officers nor men suffered from scurvy or other disease. The chief of the expedition telegraphs to the Geographical Society of Vienna that they have made "perfect observations, rich collections, and taken geodetic and photographic views of the island."

DURING the coming year, we learn from *Science*, experiments will be made at the physical laboratory of Johns Hopkins University with a view to aid in establishing an international unit of electrical resistance. The experiments will be carried on under the direction of Prof. Rowland, with an appropriation from the Government of the United States. The results will be communicated to the International Commission of Electricians meeting in Paris.

DR. ROBERT MOFFAT, the famous African missionary, has died at the advanced age of eighty-seven years. He was among the first to show the way to Central South Africa, and added not a little to our knowledge of the Bechuanas and other tribes south of the Zambesi. He was Livingstone's father-in-law, and the special direction of the great missionary-traveller's African work was to a considerable extent due to Moffat's example and advice.

THE Ninth Annual Conference of the Cryptogamic Society of Scotland will be held at Dumfries on September 11, 12, and 13. Fellows who purpose attending the Conference are requested to communicate with the local Secretary, Mr. J. Rutherford, Jardington, Dumfries.

THE statue of the brothers Montgolfier was unveiled at Annonay on Monday, as part of the ceremonies commemorative of the centenary of the inventors of balloons. M. de Fonvielle writes to us from Annonay, August 12: "This celebration has been organised merely by private exertions in continuation of the banquet given by the Académie d'Aérostation of Paris on November, 1882, to commemorate the centenary of the first private experiment tried by Joseph Montgolfier at Avignon in his rooms. A local committee was formed in Annonay under the presidency of M. Séguin, the eldest son of Marc Séguin, Member of the Institute, a nephew of the Montgolfier to whom is attributed the creation in France of tubular boilers and metallic bridges. M. Henry Vidon of Annonay was appointed general secretary. The exertions of the Committee were very successful, and about 4020*l*. were collected, principally in the immediate vicinity of Annonay and at Paris; foreign subscriptions were very few. It was decided to erect on the Place des Cordeliers, where the first experiment took place on June 5, 1783, a statue representing the two brothers inventing the 'Montgolfière.' The plaster cast has been executed, and will be inaugurated tomorrow before a large audience. The ceremony will begin at two o'clock with a speech delivered by M. Séguin, after which a small Montgolfière will be sent up from the exact spot where the first experiment took place. On Saturday an aëronautical ascent was made from the Champs de Mars with a small balloon of 3000 cubic feet, the largest that the gas establishment could fill without inconvenience."

IN the just published parts 4 and 5 of his "Abbildungen von Vogelskelettes," Dr. Meyer, of Dresden, proves that the Notornis from the South Island of New Zealand belongs to a different species from that from the North Island—*Notornis mantelli*—and he names the former *N. hochstetteri*. It is known that Prof. Owen founded on some fragments of the skull and the bones from the North Island in the year 1848 the genus Notornis, and that he called the species, without then knowing a skin, *N. mantelli*, after the discoverer. The two skins, which were figured by John Gould in the years 1850 and 1869, and which now adorn the galleries of the British Museum, came from the South Island, and were identified with the bones from the North Island. The Dresden Museum having acquired the skin and skeleton of a specimen of Notornis hunted in the year 1879 on the South Island—all three specimens were procured from within a range of ninety miles—Dr. Meyer compared his skeleton with Prof. Owen's life-size figures in the *Transactions of the Zoological Society*, and found them to be different, which fact is not to be wondered at, as New Zealand has proved to be very rich in species of flightless birds, and as the Notornis fragments came from another island than the three skins and the skeleton, perhaps Notornis became extinct on the North Island, whereas it still survives in certain parts of the South Island. Dr. Meyer is of opinion that if the bones and the skull had been taken out of the skins preserved in the British Museum, one would have known already in the year 1850, or at least in 1869, that they differed from the *Notornis mantelli* fragments of the North Island. The name of those skins, therefore, must be altered, according to Dr. Meyer to *N. hochstetteri*. Dr. Meyer has figured the skeleton of *N. mantelli* in plates 34–37 of his work.

We are glad to see that the system of appointing men as professors who only teach, and scarcely that, is now being discussed in the United States. It is to be hoped that it will soon be discussed here. The following letter from a correspondent in Germany to the *New York Nation* of July 26 gives the last contribution to the ventilation of the subject:—"Sir,—The controversy carried on in your journal in regard to professorial salaries has not failed to attract considerable attention in Germany, and especially the comparisons instituted between the financial condition of American and that of German professors. As some wrong ideas seem to prevail in America on the subject of the remuneration of university professors in this country, I should like to call attention to the real state of affairs. In the first place, it must be understood that a man who is elected to fill a chair at some German university is not expected to act merely as a teacher. His abilities as an instructor are, as a rule, regarded as a matter of minor importance, if they are at all taken into consideration on his appointment. But he is required to advance science; and, to enable him to fulfil the expectations entertained of him, the Government feels bound to make him financially independent. A grand laboratory or observatory or clinic is placed at his disposal, enormous sums are voted to defray the expenses of the most costly scientific experiments, and, in order to allow him to devote himself exclusively to the advancement of his science, a large salary insures him against the necessity of undertaking extraneous labour. The salary of an 'ordinary professor' amounts to 15,000, often 20,000 or 25,000 marks ($3500 to $5000) per annum. Besides his regular pay he receives the fees paid him by those who attend his lectures. At large universities, like those of Berlin, Breslau, Munich, and Vienna, these fees may reach extraordinary amounts. At Berlin, Reichert, the Professor of Anatomy, is paid 120 marks ($30) by each student for the lecture on anatomy and the concomitant dissecting exercises, during the winter term alone. There were over 400 students, and the sum thus received by one professor amounts to over 48,000 marks. A professor of law or philosophy generally gets 20 marks from each student for a course of lectures extending over one term, and delivered three or four hours a week. As a professor usually delivers more than one course of lectures a term, and as his lectures, especially at a very large university, may be attended by about 150 or 200 students, the emoluments which he enjoys besides his salary may be considered as affording him quite a respectable income. Added to this, the 'ordinary professor' holds his position during good behaviour. Should he choose to resign in his old age, he has claims to a good pension. Socially the professors rank as high as officers, which signifies the respect in which the devotees of science are held in this military country. Prof. Esmarch of Kiel is allied by marriage to the Imperial family of Germany. This care which the German people takes of its savants, in absolving them from the necessity of engaging in the 'madding strife' for existence, is the main secret of the success of the German university system and German scientific triumphs. When the brain-power of the American nation shall be concentrated under such favourable conditions at a few grand seats of learning, and the drudgery of the pedagogue be exchanged for the fruitful labour of the independent scientist, then the youth of America will no longer be compelled to seek opportunities for intellectual development in Europe alone.—L. N."

Estes and Lauriat, Boston (U.S.), announce, the *Nation* informs us, several important new publications: "Ornithology of the World," a popular treatise by Dr. Elliott Coues, fully illustrated; the same author's "Key to North American Birds," revised to date and entirely rewritten, with the incorporation of a practical manual of field ornithology; "The Natural History of Man," a popular work based on Hellwald's "Naturgeschichte des Menschen," which has been translated by Mr. J. S. Kingsley, and edited by him in conjunction with Messrs. W. H. Dall, F. W. Putnam, and Stephen Salisbury, jun.; "Travels in Mexico," by Fred. A. Ober, well equipped with drawings from the author's photographs and with maps.

On July 19 a terrible hailstorm is reported to have passed over the Government of Tomsk in Siberia. The hailstones were as big as eggs. Two women struck on the head were killed on the spot, besides a number of animals and birds. A terrible hailstorm is also reported to have raged in Iowa on the night of August 7. The track of the storm was four miles wide, passing through three counties. All vegetation was destroyed in its course. One woman lost her life, and many persons were injured. Twenty-two cattle were killed. The hail fell in some places to a depth of five feet. On the Rock Island, Chicago, and Milwaukee Railroads the trains were blocked; and at Lonah station nine freight cars were blown from the rails.

A special correspondent of the *Daily Telegraph*, who has been on a visit to Mr. Stanley, and has journeyed a considerable distance up the Congo, contributes the first of a series of letters to Tuesday's issue of that journal, accompanied by a map and a rough sketch.

It appears from recent analyses communicated to the Kieff Society of Naturalists, that the *Sorghum saccharatum*, of Minnesota, U.S., which was recently introduced into the Russian provinces of Poltava and Kieff, yielded as much as 14·2 to 16·7 per cent. of its weight of crystalline sugar, thus exceeding the average percentage of sugar, which commonly is from 9 to 9·5 per cent.

An illustrated "Circular of Information," distributed by the United States Bureau of Education, directs attention to a very general yet very sad deficiency, viz. imperfect hearing. Dr. Sexton in it points out that it causes, among other things, defects in pronunciation through children not knowing the correct sound; and failure and ill temper among teachers who may be unaware of their pupils' defects or their own. He urges that the hearing of all pupils should be examined each session, and no one accepted as teacher who has not passed an aural test. He strongly recommends dental inspection of pupils, as from diseased teeth especially arise deficiencies of hearing, from which follow, first, the appearance of stupidity, and eventually the reality. Among the practical precautions recommended, abstinence from bathing seems a very costly one, but few pamphlets could show so clearly the interaction of physical and mental education.

Mr. Mattieu Williams points out that on p. 350 of Nature, the dimensions of Jan Mayen are stated in "geographical miles," but that it is evident the old Norwegian sea mile is the measure used. This is equal to four English geographical miles. The length of the island is, therefore, thirty of *our* geographical miles. Mr. Williams says the "old" sea mile and the long land mile have, since July 1879, been legally superseded by the kilometre.

Vol. III. Part VII. of the *Transactions of the Essex Field Club* contains several long papers of interest, besides a considerable number of shorter ones. Among the former are "The Ancient Fauna of Essex," by Dr. H. Woodward; "The Macro-Lepidoptera of the District around Maldon," by Mr. G. H. Rayner; "Deneholes," by Mr. T. V. Holmes; "Primeval Man in the Valley of the Lea," by Mr. W. G. Smith; "On the Species of the Genus *Primula* in Essex," by Mr. Christie; with Mr. Meldola's presidential address, and a notice and portrait of the late Sir Antonio Brady. The society has also issued in a sepa-

rate form a collection of papers and memorials on the protection of wild animals and plants, and on the present condition and future management of Epping Forest.

AT the recent examination for the Licence ès Sciences Physiques in Paris, an English student, Mr. P. J. Hartog, B.Sc. Vict. Univ., passed first of the sixty-six candidates, though by three years the youngest of any.

DURING the latter part of this and the early part of next month a geographical congress and exhibition will take place in Douai. M. Ferdinand de Lesseps has been elected president. Belgium, Holland, Denmark, and Sweden will be represented.

THE United Steamship Association of Copenhagen has offered a free passage by their vessels to all Danish fishermen desirous of visiting the Fisheries Exhibition.

ON August 2, at about 10 p.m., a brilliant meteor passed from south to north over the town of Linköping in Sweden. When in the north-west it burst, spreading an intense pale blue light, and leaving a light smoke in the air which could be distinguished for several seconds. On July 23, at 10.15 p.m., a magnificent meteor was observed at Södertelje in Sweden. It went in a north-westerly direction, leaving a luminous track on the sky.

As the representative of Sweden in the International Phytopathological Association, recently formed, Dr. J. Eriksson, botanist at the Academy of Agriculture, has been chosen. He is now engaged in collecting statistics and examples of diseases of plants, which it is the object of the Association to study and eradicate.

THE additions to the Zoological Society's Gardens during the past week include a Bonnet Monkey (*Macacus radiatus* ♂) from India, presented by the Hon. Mrs. Pigott Carleton; two Maholi Galagos (*Galago maholi*) from South Africa, presented by Dr. Hugh Exton; a Rude Fox (*Canis rudis*) from Demerara, presented by Mr. Clement J. Bateman; a Suricate (*Suricata zenik* ♀) from South Africa, presented by Mr. Chas. H. Wootton; a Collared Peccary (*Dicotyles tajaçu*) from South America, presented by Mr. Fritz Zurcher; three Peregrine Falcons (*Falco peregrinus*), European, presented by Mr. J. Snowdon Henry, F.Z.S.; two Javan Adjutants (*Leptoptilus javanicus*) from Java, two Indian Tantalus (*Tantalus leucocephalus*) from India, presented by the Hon. W. H. Ravenscroft; a Sclater's Curassow (*Crax sclateri* ♀) from South America, presented by Mr. John Ardran; a Wood Owl (*Syrnium aluco*), British, presented by Mr. G. Carrick Steet; two Ring-tailed Lemurs (*Lemur catta*) from Madagascar, a Black Bear (*Ursus americanus* ♂) from North America, a Greater Sulphur-crested Cockatoo (*Cacatua galerita*) from Australia, deposited; three Indian Pythons (*Python molurus*) from India, purchased; two Mule Deer (*Cariacus macrotis*), born in the Gardens.

OUR ASTRONOMICAL COLUMN

THE SATELLITES OF SATURN.—Dr. W. Meyer has published corrected, or what he calls definitive, elements of the satellites *Enceladus*, *Tethys*, *Dione*, *Rhea*, *Titan*, and *Japetus*, chiefly founded upon his observations at the Observatory of Geneva in 1881, the mean motions however being determined from a comparison of the Geneva observations with the elements assigned by Jacob from measures of the satellites made at Madras during the years 1856-58. The mean distances and periods resulting from Dr. Meyer's investigations are as follow:—

	Mean distance in equatorial radii of Saturn.	Period. d.	h.	m.	s.
Enceladus	3·8661	1	8	53	6·92
Tethys	4·8116	1	21	18	25·62
Dione	6·1629	2	17	41	9·29
Rhea	8·6082	4	12	25	11·57
Titan	19·9111	15	22	41	23·16
Japetus	57·9303	79	7	49	24·84

The other elements of the orbit of the outer satellite *Japetus* a e subjoined; those of Jacob are added for comparison. Meyer's epoch is 1881 Nov. 0·0 G.M.T.; Jacob's is 1858 Jan. 0·0 G.M.T.

	Meyer.	Jacob.
Mean longitude	200° 8′ 53″	294° 31′·1
Longitude of peri-Saturnium	353 5 7	349 20
,, ascending node	142 17 27	143 1·3
Inclination to ecliptic	18 26 50	18 37·9
Eccentricity	0·028916	0·028443
Semi-axis major (for mean distance of Saturn)	514″·711	514″·96
Mean diurnal motion	4°·538273	4°·538042

TEMPEL'S COMET OF SHORT PERIOD (1873 II.).—Prof. Krueger, in transferring to the *Astronomische Nachrichten* the few positions lately given in this column, mentions that M. Schulhof of Paris, who has undertaken the calculations for the comet, promises an ephemeris in due course for that periodical.

THE BISCHOFFSHEIM OBSERVATORY AT NICE.—M. Perrotin, Director of the Observatory of Montgros, near Nice, lately founded by the munificence and scientific spirit of M. Bischoffsheim of Paris, has made an excellent beginning in the proposed work of that establishment. The Observatory is provided with a refractor of 15 inches aperture, and about 18 feet focal length, the object glass by MM. Henry of the Observatory at Paris, the mounting by Eichens and Gautier. This instrument M. Perrotin applied in June last to measures of a number of the more interesting double-stars, and amongst them several very difficult objects. The magnifying powers most frequently employed were 750 and 1000, with occasional use of 400 and 650. We make a short selection from M. Perrotin's results:—

Star.	Epoch.	Angle.	Distance.
42 Comæ Berenicis	1883·51	11°·5	0″·535
44 Boötis	— ·47	240·6	4·925
γ Coronæ Borealis	— ·53	138	(0 17 elong.)
ξ Scorpii	— ·52	11 5	1·16
ζ Herculis	— ·52	99·5	1·49
Σ 2107	— ·49	231·2	0·57
Σ 2173	— ·53	35 *e*	0·15 *e*
τ Ophiuchi	— ·51	252·1	1·66
70 ,,	— ·49	45·6	2·28
λ Cygni	— ·51	80·3	0·65

With respect to Σ 2173 M. Perrotin remarks that his results confirm M. Otto Struve's opinion that the star revolves in about forty-six years. The above measures of the rapid binary ζ Herculis are closely represented by Dr. Doberck's last orbit.

THE LATE TRANSIT OF VENUS.—The last number of the *Comptes Rendus* of the Paris Academy of Sciences is almost wholly occupied by the preliminary Reports from the various expeditions sent by the French Commission for the observation of this phenomenon, and one or two expeditions acting in co-operation with the Commission. The observations of contacts, &c., appear in these Reports. The stations included are Petionville, Hayti; Puebla, Mexico; Fort Tarten on, Martinique; St. Augustin, Florida; Santa Cruz, Patagonia; Cerro-Negro near San Bernardo, Chili; Chubut, Patagonia; Rio Negro (4h. 21m. 20s. W. of Paris and 40° 47′ 51″ S.); Hoste Island, Orange Bay, Tierra del Fuego; and Bragado, Buenos Ayres. It is gratifying to note the general success which attended these expeditions, even at the most southern station in Orange Bay, the latitude of which was 55° 31′ 28″.

A CONTRIBUTION TO THE STUDY OF THE TRANSMISSION EASTWARDS ROUND THE GLOBE OF BAROMETRIC ABNORMAL MOVEMENTS [1]

II.

WHEN the fact of these simultaneous movements is recognised, the irregularities in the transmission eastwards of the abnormal movements can be in great part explained. For instance, taking the movements B″ of the Zanzibar curve, it is found to recur at B′ and B in the Belgaum and Bombay curves after an interval of six months, that is to say, about one month longer than the average, and is moreover of much greater magnitude in these curves than at Zanzibar. But it is noticeable that in the

[1] Concluded from p. 356.

month of November there was a very prominent simultaneous downward movement at the three stations, a movement which must have bent the curves very considerably out of the shape they would have taken had it not occurred, and it is allowable to suppose that the proper minimum in the Belgaum and Bombay curves corresponding to B″ of the Zanzibar curve took place in the month of October, 1881, that is to say, after the normal interval of five months, but was masked by the greater minimum in November, due to the simultaneous movement. Then again in the case of the maximum movement C, C′, and C″, the period between C and C″ is, if five months be assumed to be the normal, quite regular; but between C′ and C″ it is only four months, that is, one month shorter than usual. A reference to the dotted lines shows that in the month of January, 1882, all three curves were upheaved by a simultaneous movement, while in the following month they were all three depressed simultaneously. By the co-operation of these two simultaneous movements, the maximum C′ was apparently quickened in its course by one month, and hence the irregularity. Again, with regard to the double oscillation D, D′, and D″ (1 and 2) in the Zanzibar curve the first downward bend D''_1 is greater than the second D''_2; but in the Belgaum curve they are very nearly equal, and in the Bombay curve the first is even less than the second. On glancing down at the Zanzibar curve for the month of April, it is observable that an upward movement took place then; and if it be supposed that the upward impulse was felt at all the three stations simultaneously, but that this impulse was not so great at Bombay and Belgaum as the downward impulse due to the travelling movement coming from Zanzibar, then the actual effect at those two stations would be the resultant of the two impulses, that is to say, a downward movement of less amplitude than would have occurred had there been no simultaneous movement in that month.

The apparent acceleration of the movement A, A′, and A″ is susceptible of a similar explanation, though not quite so satisfactorily, and it may perhaps be admissible to reserve for it an explanation which will present itself hereafter.

The existence of these simultaneous movements seems not only to afford an explanation in great part of the irregularities observable in the eastward transmission of the travelling movements, but also to clear away an objection that was brought forward by Mr. E. Douglas Archibald to the acceptance as an established theory of the eastward movement of abnormal variations. He asked (*vide* NATURE, vol. xxiii. p. 400) "Why the barometric waves should commence on onr meridian rather than one another." Now it is very noticeable (if a reference be made to the curves) that all the marked features of the curves—those features that are transmitted eastwards—occur in months when there are simultaneous movements at all the three stations, that in fact the simultaneous movements are the initial ones. And in the light of this fact the answer to Mr. Archibald's objection is that they do not commence on one meridian rather than on another, but (so far at any rate as the three stations under consideration are concerned) on all meridians simultaneously. But it is likely enough that they may be greater on one particular meridian, or at one particular point on that meridian, than on those on either side of, or about it, that in fact they result from a slight heaping up or withdrawing of the atmosphere over, or from, one part of the earth's surface, in which case the heap, or depression, will have its greatest altitude or depth at one particular place, but of course will be felt over a more or less considerable area around that place, and the degree in which it will be felt will be less as the length of the radius from the centre is increased. And that this is not altogether a fanciful idea is apparent on a reference being made to the smoothed curves, when it will be observed, for instance, that in July, 1880, and also in June, 1881, the upward movements were much greater at Bombay, the most northern of the three stations, than at Belgaum, a more southerly one; and at this, again, they were much greater than at Zanzibar, the most southern.

Mr. Archibald brings forward another objection. He asks: "If, as Mr. Chambers thinks, the waves of pressure travel slowly round the earth, why they do not reappear at the place where they started, after an interval of about one year and eight months (calculated from the lags given in Mr. Chambers's paper). At present there does not appear to be the slightest evidence that they reappear at all, and if they do not, when and where do they disappear?" One answer to this question is that they must, in the course of their eastward journey, get completely masked by other simultaneous movements of the atmosphere that are constantly taking place. Another answer to this question, and the one not requiring the supposition of the simultaneous movements, is that, as the travelling waves get further away from the place of their origin, and consequently widen out, their amplitude gets constantly less, until at last, like the waves caused by dropping a stone in a pond, they become imperceptible. If it were possible to eliminate the effect of the simultaneous movements, and examine only the curve produced by the travelling waves, one might then see this gradual decrease in their amplitude as they proceeded along their journey. It is impossible, however, at present to separate the effects of the two movements. An alternative method, however, to eliminating the effect of the first-mentioned movements is to pick out a period during which they were small or imperceptible. If such a period can be found, it will then doubtless be possible in some degree to trace the comparatively undisturbed action of the travelling movements. Such a period occurred from March to August, 1882, during which time the simultaneous abnormal movements were not easily traceable. And it is then seen how the amplitude of the double oscillation D″ (1 and 2) of the Zanzibar curve has diminished at D′ and D (1 and 2) of the Belgaum and Bombay curves.

Another question which may be raised with regard to the matter, and a question which is not so easily answered is, why these waves should travel in an easterly direction and not in a westerly? It would be imagined that they should be transmitted equally in both directions, or if they are transmitted in one direction rather than in the other, it should have been a westerly one; in which case their motion might have been accounted for readily enough by supposing the atmosphere to lay behind in equatorial regions in a westerly direction due to the influx of air of a lower velocity from the polar regions; and perhaps also by supposing the sun to exercise an influence in the matter. The fact is, however, that the motion is in the same direction as, and ahead of, the earth's rotation. It would be interesting, however, to see if there is any evidence of a westward motion, and referring to the curves with this object in view, such evidence is perhaps discoverable. For instance, the simultaneous movement in July, 1880, causes a very marked upward bend of the Bombay and Belgaum curves; if, then, there is any motion westwards, this upward bend should make its appearance in some succeeding month in the Zanzibar curve; and, as a matter of fact, there is an upward movement shown by the dotted line in the month of September. It is difficult to find many instances of this westward transmission, owing to the somewhat intricate mixture of movements presented by the curves; but the following instances may be adduced as lending some support to the hypothesis: there is a simultaneous downward movement in May, 1880, and there is an independent downward movement at Zanzibar in the month of August in the same year, that is, three months later; there is again the instance already cited of the simultaneous upward movement of July, 1880, recurring at Zanzibar in September, that is, two months later. It may be that the excessive downward movement at Zanzibar in the month of March, 1881, was in part due to the recurrence there of the simultaneous movement which occurred, especially developed at Bombay, in January of the same year; that is, two months before. Again, the very large downward movement at Zanzibar in October and November, 1881, may have been in part due to the arrival there from Bombay and Belgaum of the wave produced by the simultaneous downward movement which occurred in August, two and a half months earlier. And lastly, two instances less difficult to trace, owing to the absence of any marked simultaneous movements during the period of their occurrence, are the upward and downward movements at Zanzibar of the months April and May, 1882, which may be regarded as due to the arrival there from the west of India of the waves resulting from the simultaneous impulses received at all stations in January and February of the same year; that is, at periods of three months for each.

And here may be given the explanation previously referred to of the apparent acceleration in the rate of movement of the wave A, A′, and A″. It is possible that A and A′ may not be due to the arrival at Bombay and Belgaum of the maximum A″, but of the wave caused by the simultaneous movement which occurred in July six months before, a period much nearer the normal than are the periods three and three and a half months; in which case the maximum A″ would be due to the arrival at Zanzibar from Bombay and Belgaum of the wave caused by the

simultaneous movement in July, 1880, together with the simultaneous movement of October, 1880.

If this be a correct analysis of the curves, then there is the remarkable fact to be noted, that the motion of these waves in a westward direction takes place at an average rate of two and a half months, that is to say, twice as rapidly as in the eastward direction. And this fact would readily accord with the supposed westward lagging of the atmosphere due to its inertia; and also with any supposed influence of the sun. The presence of this westward transmission is not so apparent, however, as that of the eastward. And whether it be present or not, there still remains the difficulty, substantially the same as at the outset, that the motion eastwards is by far the most defined and most readily traceable; a difficulty for which I cannot even guess at any solution. Facts, however, should not be overlooked because they cannot be explained, but rather an explanation sought; and in the explanation of this fact theoretical matters of considerable interest may perhaps be involved. The only hints at any facts which might by any possibility suggest an explanation are to be found in Mr. Chambers's summary of his discovery, where he speaks of the direction eastwards being like that of "the cyclones of extra-tropical latitudes"; and in the very interesting and more suggestive statement of Dr. Balfour Stewart (*vide* NATURE, vol. xxii. p. 151), in which he says, speaking of terrestrial magnetism, "that we have some evidence which leads us to suspect that particular states of declination range, like particular states of weather have a motion from west to east, the magnetical moving faster than the meteorological."

As to the cause of these widely-distributed simultaneous movements of the barometer, movements which I consider to be in the main the initial impulses of the complication of abnormal movements visible in the curves, I have no evidence of any value. The most natural idea is that a connection, direct or indirect, may be traced between them and changes in the state of solar energy; the downward movements perhaps being due to an excess of energy, and the upward movements to a deficiency. In some points, perhaps, they may bear analogy to magnetic storms. I have not a sun-spot curve for the years under consideration, and cannot therefore make the necessary comparisons.

As a working hypothesis to serve as a guide in further investigating the matter, I should be inclined to suppose that the atmosphere, if it could, without stopping the earth's motion, be divested of its regular diurnal and seasonal movements, and the eddies and storms resulting therefrom, would present to observation a somewhat intricate mixture of motions consisting of the following elements:—

1. Certain initial movements, resulting mediately or immediately from changes in the state of the sun's energy, and affecting very wide areas, and being of the form of heapings up or drawings away of the atmosphere over these areas, the movements attaining their maximum height or depth at the centre of these areas. The centres of these areas would be immediately under the sun, that is to say, within the tropical latitudes.[1]

2. Waves resulting from the propagation in eastward and westward (and perhaps, though in a less marked degree northward and southward) directions of the impulses given by the first movements; the waves which travel eastward being for some unexplained reason more pronounced than those travelling westward, but their rate of motion over the earth's surface being, on account of the rotation of the earth and the atmosphere's inertia, slower in the eastward direction than in the westward.

3. Small local movements over more limited areas resulting from the chance conjunction and interference of any two or more of the first and second movements.

An extensive and detailed examination of the barometric records of stations scattered over the globe will bring to light facts either favourable or unfavourable to this hypothesis; and after this examination has been made, it will then be time to decide whether or not it is worth while undertaking the labour of dealing with the subject mathematically.

The matter seems important even theoretically, for in it and investigations of a like kind are to be found attempts at a rational arrangement of the very complex collection of facts contained in the various records of barometric abnormal movements; and practically also, for on the results of further investigation into it depends the confirmation or dismissal of a hypothesis which has given promise of furnishing a useful method of weather forecasting.

A. N. PEARSON,
Acg. Meteorological Reporter for Western India

Bombay, January 10

[1] Mr. H. F. Blanford's discovery of "a barometric see-saw between Russia and India in the sun-spot cycle" (*vide* NATURE, vol. xxi. p 477) seems to support this hypothesis.

SCIENCE IN RUSSIA

THE Kieff Society of Naturalists was opened in 1869, and soon had more than a hundred members, mostly belonging to the University. Like other Societies of Naturalists at the Russian Universities, its chief aim has been the exploration of Russian natural history in the neighbouring provinces, these explorations proving that though the region around the Dnieper was not quite unknown in its geological, botanical, and zoological aspects, still there were wide lacunæ to be filled up before arriving at a thorough knowledge of it. Prof. Feofilaktoff, who had already published a geological map of the province of Kieff, assisted by several young geologists, busily explored, therefore, the surrounding provinces, especially on the right bank of the Dnieper, and published in the *Memoirs of the Kieff Society* a series of valuable papers on the Cretaceous, Tertiary, and post-Tertiary of the region, as well as on brown coal on the Dnieper. The Phanerogamic flora of the Dnieper region being sufficiently well known from the former works of Professors Andrzeiovski, Trautvetter, Rogowicz, and several others, the chief attention of the Society has been devoted to the Cryptogamic flora; and numerous papers by MM. Borschoff, Plutenko, Wältz, Rishavi, Timofeeff, Ryndovsky, Moshinsky, and Sovinsky, on the algæ, mosses, lichens, and fungi of the Dnieper region, as well as of Caucasus, appeared in the *Memoirs*. In zoology the chief researches were directed towards the exploration of the invertebrate fauna of the Black Sea, and whilst M. Bobretzky thoroughly studied the Annelids of the Black Sea, M. Krichagnin carried out special studies of the Copepoda, and M. Paulson studied the Crustaceans of the Red Sea, in order to compare them with those of the great interior sea of Russia and Turkey. Several valuable papers were published at the same time on the anatomy and physiology of animals and plants, whilst the researches in chemistry and physics which were made at the Kieff University were mostly sent for publication to the *Journal of the Russian Chemical and Physical Society* at St. Petersburg.

Finally, the Kieff Society has undertaken, since 1873, the yearly publication of a most valuable systematic catalogue of papers in mathematics, in natural science, pure and applied, and in medicine, published throughout Russia in the numerous scientific publications which have grown up during the last ten years. These catalogues, which have reached during the last few years the size of large octavo volumes two hundred pages in extent for natural sciences and the same for medicine, are most valuable, as the number of provincial publications rapidly increases in Russia, and scientific papers of great value are virtually buried among the publications of the statistical committees, provincial assemblies, local scientific societies, and so on. The last (tenth) volume of this catalogue contains an index for the whole series of ten volumes.

The two last volumes of the *Memoirs* (*Zapiski*) *of the Kieff Society of Naturalists* (vols. v. and vi. 1879-1882) contains, like the preceding ones, a good many valuable papers. In geology we find several papers by Prof. Feofilaktoff and Schmalhausen. According to the former, the Eocene formation of the region has its central parts in the Government of Kieff, on the banks of the Dnieper. It consists of two series of deposits, the sandstones and sands of Traktemiroff, which only contain remains of Mollusks; and the Spondylus deposits which cover the former, and consist of sands, Spondylus clay, and greenish sands with plants (vol. v. fasc. 2). These plants, according to M. Schmalhausen's researches, which will soon be published by the Society, are the Alga *Chondrites*, similar to the Eocene *Chondrites Targionii*; a Conifer similar to the *Araucarites Duchartrei*; fruits of *Nipadites*, similar to those of the London clay; and pieces of Coniferæ and Palms and of a Bromelite (*Br. Dolinskii*, Schmalh.), fruits of tropical Leguminosæ (*Leguminosites Rogowiczi* and *L. Feofilaktowi*), and leaves of *Ficus prisca*. All these plants have been found in the upper parts of the clay, whilst in the sands that cover it M. Schmalhausen found a great number of stems and leaves of marine Monocotyledons, such as *Caulinites Rogowiczi* (a new species akin to the *Caulinites parisiensis*), and a new species of *Zorterites*, as well as parts of a new species of Graminea, *Polocapyrum incertum* (vol. vi. *Pro-*

ceedings). In another paper Prof. Feofilaktoff gives a description of the diluvium of Poltava (vol. vi. fasc. 1). It consists of three different series of deposits, namely, the lower boulder clay, the loess, and the upper boulder deposits. The yellow loess of Poltava is a quite characteristic loess, and contains the usual *Helix hispida*, *Pupa muscorum*, and *Succinea oblonga*, but it is well stratified at certain places, as it contains intermediate deposits of sandy clay. The upper boulder clay reaches a thickness of forty to fifty feet, and contains boulders five to ten feet in diameter. It consists of materials brought from the north, with a mixture of local materials—chiefly of the underlying loess—without any kind of stratification of the different elements of which it consists. M. Schmalhausen gives a description, with a plate, of the stem of the *Protopteris punctata*, Sternb., from the Government of Volhynia. This sample seems to be the best known up to the present time, and M. Schmalhausen doubts whether this cretaceous fern has been found anywhere in Western Europe in so well-preserved a state. The incomplete samples which were often found in Western Europe led to its being described under the names of *Filicites punctatus*, *Sigillaria punctata*, *Caulopteris punctata*, and *Protopteris Sternbergi*. A note by Prof. Borschoff, on the downs of the Kyzyl-Koum Steppe, has been previously noticed in these columns. We notice also several analyses of Caucasian mineral waters.

The zoological papers are numerous and important. M. Krichaguin gives an account of his dredgings on the north-eastern coast of the Black Sea, and describes the following new species of Copepoda: *Monstrilla intermedia*, *Monstrilla pontica*, *Longipedia pontica*, *Tachidius Abrau*, *Canthocampus æguipes* and *longicaudatus*, *Liljeborgia pontica*, *Cleta brevirostris armata*, *C. Thalestris*, and *C. Liljeborgia*, *Westwoodia pontica*, *Thalestris filifera*, and *Oithona minuta*. His conclusions are: that the fauna of the Black Sea has great originality, owing to the large number of original genera it contains; that the cosmopolite forms either appear as original species, or have a resemblance to the Mediterranean ones, and that those species which are common to the Black Sea and northern seas have undergone important modifications (vol. v. fasc. 1). M. Sovinsky's paper on the Amphipods of the Bay of Sebastopol (vol. vi. fasc. 1) contains a complete monograph of the twenty-seven species he has found in this bay, and a description of four new species of *Sunamphitoë*, *Dexamine*, and *Microdeutopus*. Another paper by the same author (vol. vi. fasc. 2) contains a comparison, with plates, of the Red Sea species *Virbius proteus*, as well as the genera *Nikoides* and *Alpheodes*, established by M. Paulson, with the Black Sea forms *Virbius gracilis*, Hell., *Nikoides pontica*, and the Mediterranean *Alpheus dentipes*, which are nearly akin to the above. M. Bobretzky, who published, in 1870, in the *Memoirs of the Kieff Society of Naturalists*, a systematic description of forty-three species of *Annelida Polychæta*, has recently revised his determinations on the ground of new observations, as well as of the researches by MM. Claparède and Marion; and, without seeking to establish new species, he has preferred to establish a comparison between the Black Sea and Mediterranean forms, and to maintain only the three following new species: *Polynoë incerta*, *Ophelia taurica*, and *Terebellides carnea*.

In the department of comparative anatomy we notice an elaborate paper by M. Rumshewich, on the development of the eye among Vertebrates, accompanied by numerous plates; on the internal muscles of the eye of Reptiles (*Lacerta agilis*, *L. viridis*, *L. Stirpium*, *Chelonia fluviatilis*, and *Ch. midas*), by the same; on the reproductive organs in Annelids, and on the origin of the blastoderm in insects, by M. Bobretzky; and on the structure of the brain in man, by M. Betz.

Botany is represented in volumes v. and vi., only by lists of Phanerogams and of Algæ in the district of Radomysl, on the Teterev River, by M. Sovinsky; and chemistry by an elaborate paper, by M. Barzilovsky, on the nitrotoluols.

After having largely contributed during the years 1855 to 1865 to the purely geographical exploration of the unknown parts of Siberia and the adjacent countries, the East Siberian branch of the Russian Geographical Society entered upon a period of more thorough scientific exploration of Siberia itself. The merely geographical expeditions, such as that of MM. Czekanovski and Müller to the land of the Chuckches, became few and rare, and we now find the members of the Society engaged in a complete exploration of the natural history of Siberia, so that the two last volumes of the *Izvestia*[1] of the East Siberian branch bring us a series of researches into the geology and anthropology of Siberia. The first rank among these undoubtedly belongs to the geological explorations around Lake Baikal, by M. Chersky. The young geologist of Irkutsk publishes for the first time a most interesting geological map of the coasts of Lake Baikal. It appears from this map that the great mass of the mountains on the western shore of the lake consists of Laurentian crystalline slates, mostly chloritic schists and gneisses, overlying the aphanite schists and amphibolitic slates, with intercalations of granites, granito-syenites, and porphyries. The upper horizon of the same formation consists of the same slates and gneisses, with thick intermediate deposits of limestones. The whole is covered to the west with Silurian deposits, a large Jurassic freshwater basin occupying the depression of Irkutsk. Smaller depressions are occupied by freshwater Miocene deposits. The most important result of M. Chersky's researches is that (as was foreseen on the ground of orographic and architectonic data) the depression of Lake Baikal is not a longitudinal valley, as might be supposed at the first aspect. The chains of mountains we see on its western shore reappear on the eastern shore, maintaining the same direction from south-west to north-east, and *crossing* the lake in the shape of submerged low ridges. On the south-eastern shore of Lake Baikal M. Chersky found the continuation of the high plateau of Eastern Siberia consisting of the same two parts of the Laurentian formation, and covered with lower Silurian deposits, the depressions of which were occupied during the Tertiary period with freshwater lakes; there are also numerous traces of great lakes which covered wide tracts during the Post-Glacial period. As to the glacial period, the number of accurate observations published by the East Siberian geologists is unfortunately not in proportion to the amount of theoretical discussion, the only sure and new facts we have to mention being the presence of *roches moutonnées*, due to glaciation, on the northern shore of Lake Kossogol, that is, on the high plateau at the foot of its border-ridge, the Sayan Mountains (they were described by the late M. Czekanovski); traces of glaciation in the higher parts of this ridge; polished *roches moutonneés* at several places of the high plateau in the basin of Selenga, requiring, however, a more careful examination; and glacial deposits in the valley of the Irkut, due to local glaciers, whose extremities reached a height of less than 2000 feet above the present sea level.

The Siberian branch of the Geographical Society has taken, during the last few years, a lively interest in anthropology and archæology, and we notice in the two last volumes of its *Izvestia* a series of papers on this subject. M. Vitkovsky's excavations of grave-mounds of the Stone period on the left bank of the Angara, at the mouth of the Kitoy, and also of the sand-hills which were inhabited by prehistoric man, have yielded a very rich collection of bones and implements. No less than twenty complete skeletons were dug out, twenty-five nephrite hatchets, numerous nephrite, jade, and quartzite arrow-points, bone needles, and implements for fishing. The most interesting feature of these implements is the presence in very great numbers of carved pieces of slate, pretty well polished, and representing seals. They occur in large quantities (160 in M. Vitkovsky's collection), and are of all sizes, from 150 millimetres to 15 millimetres long. These carvings of seals, as well as other implements, are illustrated in the plates which accompany M. Vitkovsky's paper. The skulls testify that the inhabitants of the Downs were a mixture of dolichocephals and brachiocephals, the former seeming to have predominated. The jade of which the hatchets were made was probably taken from the jade boulders which are found in the valley of the Byelaya River in the Government of Irkutsk. We notice, also, most valuable papers by M. Agapitoff on the hieroglyphics on cliffs on the western shore of Lake Baikal; and on the remains of prehistoric man in the province of Irkutsk, and on Olkhon Island. The hieroglyphic inscriptions on cliffs which are so numerous in the district of Minusinsk (they were lately figured in the St. Petersburg *Izvestia* of the Geographical Society) were supposed to be very rare towards the east; but simply because they remained unknown. Those on Lake Baikal (reproduced in the Siberian *Izvestia*) represent several men, of two different sizes, reindeer, deer, birds, and, most probably, a horse with a man upon it. The old graves are very numerous, too, on Olkhon Island, and they belong (according to the measurements of the skull) to Mongolians, as well as the remains of stone walls which were discovered on the shore of Lake Baikal. They contain iron implements, as well as glass globules and amber pearls.

The Siberian branch of the Geographical Society has also,

[1] *Izvestia* of the East Siberian branch of the Russian Geographical Society, vols. xii. and xiii. Irkutsk, 1881 to 1883.

during the last few years, devoted much attention to the meteorology of Siberia, and, besides the meteorological observations made at its stations, it has collected materials for ascertaining the dates of the freezing and breaking up of the ice in Siberian rivers. The list of these dates for the rivers of Siberia for the years 1874 to 1880 will certainly be consulted with profit, as also several brief notes on amber in Siberia, on chemical analyses of salt from various salt lakes, and of coral from the Nerchinsk district, and from the banks of the Amur, as also other smaller notes.

EXPERIMENTAL RESEARCHES ON THE ELECTRIC DISCHARGE WITH THE CHLORIDE OF SILVER BATTERY[1]

THE authors recall that at the conclusion of the third part of their researches (*Phil. Trans.* for June 11, Part I. vol. clxxi.) they stated that they intended to make an investigation on the dark discharge, and the special conditions of the negative discharge; this paper contains a number of experiments, more especially on the latter subject, and also others intended to throw light on the general nature of the electric discharge through gases.

The first part of the paper describes some experiments made with vessels of different forms in order to ascertain whether the dimensions and shape of the vessel have any effect on the pressure of minimum resistance to the electric discharge. This was found to be the case; for example, with a residual air charge in a spheroidal vessel 7 inches (17·8 centims.) long, and 5 inches (12·7 centims.) diameter (Fig. 1), the pressure of minimum resistance was as high as 3 millims., 3947 M; while in a tube 22·5 inches (57 centims.) long, and 1·625 inches (4·1 centims.) diameter, it was only 0·69 millim., 908 M; again in a smaller tube 23 inches (58·4 centims.) long, and 0·75 inch (1·9 centims.) diameter, it was 1 millim., 1316 M. It is evident, therefore, that not only the dimensions of the tube, but possibly also the shape of the terminals, have an influence on the pressure of least resistance, and it is very probable that in the atmosphere, where lateral expansion is practically unlimited, the conditions of minimum resistance are different from those which exist even

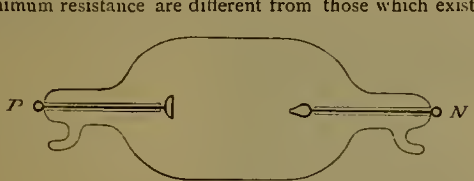


FIG. 1.

in very large tubes, and that this may influence the height of the aurora.

The paper next deals with the discharge in miniature tubes $\frac{7}{8}$ inch (2·2 centims.) long, and $\frac{1}{4}$ inch (0·63 centim.) diameter, with terminals nearly touching; at first it required 2400 cells to pass, then a single cell would do so, but after standing a short time it required 4800 cells to reproduce a discharge. In another tube $1\frac{3}{4}$ inch (4·4 centims.) long and $\frac{3}{8}$ inch (0·95 centim.) diameter), with the terminals distant 0·00104 inch (0·0264 millim.), it required 2240 cells to produce a discharge, then the potential had to be increased to 11,240 cells to do so. Ultimately even this number failed, but after the tube had lain by for some days 600 cells could pass. It is very possible that the strong discharge in the first instance volatilised a portion of the terminals which were of platinum, and that this volatilised metal condensed afterwards, or else that the terminals absorbed the gas so completely as to produce a vacuum too perfect to admit of a discharge taking place; and that ultimately sufficient of the occluded gas was again given off to render it again possible.

In connection with the occlusion of gas by terminals a case is described in which the terminals are of palladium and the charge hydrogen (Fig. 2). After a few discharges the terminals occluded some of the gas, and when a fresh one was produced a volatile compound of hydrogen and palladium was given off, especially from the negative, and produced a dense, mirror-like coating on the inside of the tube (Fig. 3); this was reoccluded by standing for a couple of days, leaving the tube free, and again given off to form a new mirror-like coating with a fresh discharge; this property has continued since March, 1875.

The paper next describes experiments to ascertain the length of the spark in dry air and in air saturated with moisture. It was found to be practically the same in both cases. With 10,860 cells the mean length of the spark between two paraboloidal points was found to be in dry air 0·45 inch (1·1 centims.), in moist air 0·447 inch (1·1 centims.).

The next subject taken up is the discharge in a tube from two batteries, first in the same and then in contrary directions. In the tube are two terminals at each end, one pair at opposite ends being inclosed in two short pieces of tube 9 inches (22·8 centims.) long and $\frac{1}{2}$ inch (1·27 centims.) diameter; the main tube being 31 inches (95·2 centims.) long and $1\frac{3}{4}$ inch (4·4 centims.) diameter. The various phases of the stratified discharge are represented in an engraved mezzotint steel plate copied from photographs, and show the effect of the one stratified discharge on another stratified discharge produced by a second battery. It is seen that two discharges in contrary directions may take place in the same tube, and that the one modifies the aspect of the other.

Experiments are also described in a tube in the form of a cross with four arms at right angles (Fig. 4), with two separate batteries connected in various ways with the different members.

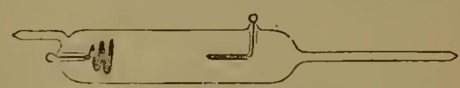


FIG. 2.

The experiments were made both in air and in hydrogen. By the introduction of external resistance of one of the batteries, the discharge could be readily identified as belonging to that battery by the effect of the resistance on the character of the stratification. In one of the mezzotint plates are several figures copied from photographs which show clearly the phenomena produced. Calling the poles P and N of one battery, A, and P′ and N′ of the other, B, it is shown in one case when two currents were equal 0·0083 ampere, that a discharge from A battery goes from P in the direction of N only so far as the junction at the cross, and then turns off to N′, the negative of the other battery B; while, on the other hand, the discharge of the B battery goes from P′ to N of the A battery. The case is different if an external resistance is introduced in one of the discharges, reducing it to 0·00087 ampere, then the discharge of the A battery goes from P to N, and that of the B battery from P′ to N′. There is a bending down, however, of the strata of the weaker discharge of the cross junction, in consequence of the action of the stronger one.

The authors remark that one cannot but be impressed, from the experiments described in the paper, and others in their former papers, by the apparent plasticity of the aggregate assem-

FIG. 3.

blage of molecules constituting a stratum which yields to external influences that modify its form.

The authors describe and figure a case of complex strata in the form of an outer bracket convex towards the negative (Fig. 5), and close to it an inner chord; also discharges in various gases in tubes of large dimensions, 37 inches (94 centims.) long, and $5\frac{13}{16}$ inches (14·8 centims.) diameter. In these the stratification, which is comparatively narrow at the terminals, extends in a conical form from the terminals to the full diameter of the tube.

They have found that the dark space in the discharge in vacuum tubes is only relatively actinically dark in comparison with a stratum, and they succeeded in obtaining a photograph of the dark space in thirty-five minutes as strong as that from a stratum in two and a half seconds; consequently they conclude that the dark space is 840 times less actinically bright than a stratum.

The authors next describe a number of experiments, by means of a Thomas-Becker electrometer used on a method, to avoid leakage, proposed to them by Prof. Stokes, to ascertain the difference of potential in different parts of a vacuum tube having a number of rings sealed within it, also in other tubes of special construction. These bring out instructive information, in reference not only to the relative resistances of different lengths of a

[1] Abstract of a paper read at the Royal Society on June 14, by Warren De La Rue, M.A., D.C.L., F.R.S., and Hugo W. Müller, Ph.D., F.R.S.

column of gas at various pressures, but also forcibly to the impediment presented by the terminals themselves to the passage of a discharge from gas to terminal and terminal to gas.

It is shown that, at moderate exhausts, the resistance to the passage of the discharge is uniform along the length of the column of gas, and that at high exhausts it is not so, and that the total resistance increases but slightly with an additional length of the column; moreover, that, at these low pressures, the main impediment is in the passage of electricity between gas and terminal or terminal and gas; this is much greater at the negative than at the positive terminal.

The authors have next studied the electrical condition of a gas in the immediate vicinity of the negative terminal. In order to do this they constructed a tube 4½ inches (11·4 centims.) long and 1⅞ inches (4·8 centims.) diameter. One terminal is in the form of a point, the other in the form of a ring. The positive pole of the battery was connected with the point, and the negative either to the ring alone or to earth as well; the ring terminal of the tube was, when the battery was insulated, connected with earth either by means of a stout wire or 3 feet (91·4 centims.) of fine platinum wire, 0·002 inch (0·005 centim.) diameter, and offering a resistance of 81 ohms, or a moistened

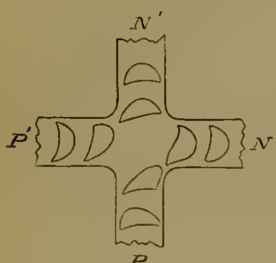


FIG. 4.

FIG. 5.

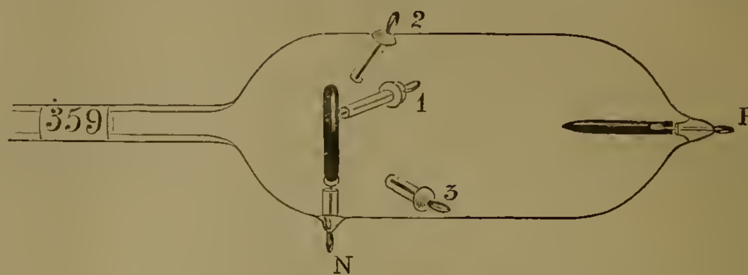


FIG. 6.

cork offering a resistance of 4,300,000 ohms. In the tube were sealed three idle wires, 1, 2, 3, covered with the exception of their extremities with fine glass tubing (Fig. 6). No. 1 idle wire is 0·002 inch (0·005 centim.); No. 2 0·2 inch (0·5 centim.); and No. 3 0·6 inch (1·5 centims.) from the ring. The ring terminal, when connected to earth, was found to be always at zero potential; notwithstanding this there was frequently observed, more especially as the exhaust was increased, a negative potential when the idle wires were connected successively with the electrometer, amounting in one case with an air charge, pressure 0·01 millim., at wire No. 2, to 1068 cells, at wires 1 and 3 to 912 cells. At other times a plus potential was observed. Many experiments were made to determine the precise conditions which developed a negative potential or a positive potential, but unsuccessfully, and it was inferred that this depended on the condition of the discharge itself within the tube. It is certainly very remarkable that, while the potential of the negative ring was absolutely zero, a high negative potential should be developed in its near vicinity.

The authors remark that every one familiar with the appearance of a stratified discharge will have noticed when the negative terminal is a ring, that as the exhaust proceeds a spindle of light

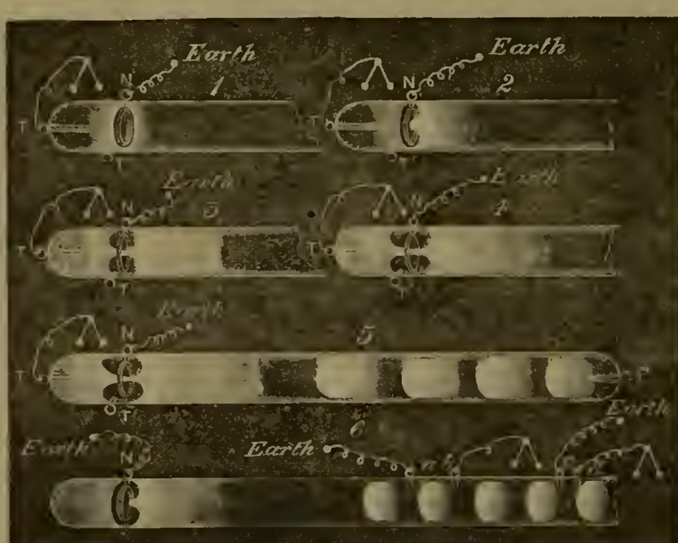


FIG. 7.

approaches and at last protrudes through the interior of it (Fig. 7, 1, 2, 3, 4, 5); this spindle they regard as a visible exponent of strong action among the molecules of gas composing it. In order to probe its electrical condition they prepared a tube with a central idle wire surrounded by a minute glass tube, except its extremity, and projecting to a distance of ⅜ inch (0·95 centim.) from the plane of the ring, which was made negative. Another idle wire was sealed in the tube 0·15 inch (0·38 centim.) from the periphery of the ring. As the exhaust proceeded with a charge of carbonic anhydride, the spindle approached the ring and ultimately protruded through it. It was found that the potential of the central idle wire increased with the exhaust, until it nearly or quite equalled that of the whole tube; while that of the external idle wire was only 0·054 that of the tube.

A great number of experiments were made to test the potential across a stratum *a*, *b*, and across a dark space *c*, *d*, respectively, by two idle wires sealed in suitable positions in a tube, one of which was connected with earth, the other with the electrometer (Fig. 7, 6). The gases used were carbonic anhydride and hydrogen respectively. As a mean of a great number of experiments it was found that when a dark space was straddled,

the potential being reckoned 1, then when a stratum was straddled the potential was 1·243, 1·229.

On testing two idle wires distant ⅝ inch (1·6 centims.) apart with a Thomson-Becker galvanometer, the current in this fractional part of a tube was found to go frequently in the reverse direction to that of the main current, and when the galvanometer was connected to two idle wires diametrically opposite, currents

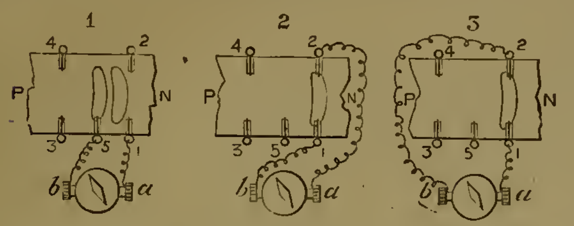


Fig. 8.

were indicated sometimes in one direction, sometimes in another across the tube (Fig. 8). These experiments seem to indicate that there are eddies in the gas during a discharge, as if the motion of the molecules conveying an electric discharge was of an epicycloidal character. The authors conclude by saying that it is possible that the eddies may be connected with the production of strata.

UNIVERSITY AND EDUCATIONAL INTELLIGENCE

Oxford.—We are requested to announce that the Savilian Professorship of Geometry is vacant, and an election to the office will be held before the end of Michaelmas Term (December 17). A Fellowship in New College is now annexed to the Professorship. The duty of the Professor is to lecture and give instruction in Pure and Analytical Geometry.

The combined emoluments of the office from both sources will be, for the present, 700*l.* a year, but may possibly hereafter be increased to an amount not exceeding 900*l.* a year.

Candidates are requested to send to the Registrar of the University their applications, and any documents which they may wish to submit to the electors, on or before Wednesday, October 31.

SCIENTIFIC SERIALS

Bulletin of the Belgian Royal Academy of Sciences, June.—On the action of amygdaline during the germination of bitter almonds, by M. A. Jorissen.—Determination of the specific heat of some organic bodies; variations experienced by this quantity through change of temperature, by M. de Heen. Of the eleven substances examined, three only—the formic salts of sodium, calcium, and barium—maintained a perceptibly constant specific heat within the limits of a temperature ranging from 10° to 93° C. A considerable increase of specific heat was shown by most of the other bodies tested.—Note on a double series of equations, by M. E. Catelan.—Anatomical study of the Æschnines (*Æ. grandis* and *heros*), by Baron Edm. de Selys Longchamps. Appended is a complete tabulated classification of the Æschnideæ (Æschna of Fabricius and Latreille).—On a deposit of *Oldhamia radiata* (Forbes) recently discovered in Tubize, Brabant, by M. C. Malaise. From its position in the Brabant schistose system the author is induced to refer this rock to the Lower Cambrian formations.—Attempted determination of the relation $\frac{C}{A}$ of the principal momenta of inertia in the terrestial spheroid, by M. E. Ronkar. In this paper a twofold series of calculations are made, based respectively on the hypotheses of Lipschitz and Laplace regarding the mean density of the crust of the earth.—Note by the editor on the explanation of the prevailing blue colour in large volumes of pure water advanced by M. Montigny.

Annalen der Physik und Chemie, July.—Electrical researches, by G. Quincke.—Researches on the slow discharge, by Heinrich Hertz, with six diagrams.—On the difference in the discharge from the positive and negative electrodes, by H. Hellman of Riga.—New observations on the thermo- and actino-electricity of rock crystal as a reply to a memoir of C. Freidel and J. Curie, by W. Hankel.—On the variation of the magnetic coefficient with the hardness of steel, by Hugo Meyer.—On the coefficient of friction of mercury and its variation with the temperature, by Synesius Koch, with three diagrams.—Theory of light for perfectly transparent light, by W. Voigt.—Concerning the theory of light, by E. Lommel.—On the sound of impinging flames, by K. Noack (three diagrams).—A new apparatus for showing Foucault's streams, by Dr. A. von Waltenhofen.—On the relation between the fundamental note and overtones of transverse vibrations in open metal cylinders, by Hugo Feukner.—On the reciprocal of the strain of closely-tuned elastic bodies, by Dr. G. Krebs.

SOCIETIES AND ACADEMIES

Sydney

Linnean Society of New South Wales, June 27.—Prof. W. J. Stephens, M.A., in the chair.—The following papers were read:—Descriptions of new genera and species of fishes by Charles W. De Vis, B.A. Two genera are described, *Dactylophora* of the family *Cirrhitidæ*, and *Leme* of the family *Amblyopina*. The new species described are: *Girella carbonaria*, *Girella neuralis*, *Dactylophora semimaculata*, *Platycephalus semermis*, *Polynemus specularis*, *Leme mordax*, *Sphyræna strenua*, *Trochocopus sanguinolentus*, *Labrichthys dux*, *Plagusia notata*, *Synaptura cinerea*, and *Crossorhinus ornatus*.—A fourth paper on plants indigenous in the immediate neighbourhood of Sydney, by Mr. E. Haviland.—Localities of some species of Polynesian recent mollusca, by John Brazier, C.M.Z.S., &c.

Paris

Academy of Sciences, August 6.—M. Blanchard, president, in the chair.—Preliminary reports on the transit of Venus, December 2, 1882, at the Transit Stations of Haiti, by MM. D'Abbadie, Callandreau, and Chapuis; of Mexico, by MM. Bouquet de la Grye, Héraud, and Arago; of Martinique, by MM. Tisserand, Bigourdan, and Puiseux; of Florida, by M. Perrier; of Patagonia, by M. Fleuriais; of Chili, by MM. de Bernardières, Barnaud, and Favereau; of Chubut, by M. Hatt; of Monte Video, by M. de Penfentenyo; of Rio-Negro, by M. Perrotin; of Cape Horn, by M. Courcelle-Seneuil; of Bragado, by M. Perrin. These reports, deposited with the Secretary of the Academy on the return of the several expeditions, are here collected together for the convenience of astronomical students.—Active or dynamic resistance of solids. Graphic representation of the laws of longitudinal thrust applied to one end of a prismatic rod, the other end of which is fixed (concluded), by MM. de Saint-Venant and Flamant.—In reply to a recent communication by M. Jamin on the critical point of liquefied gases, a letter was read from Mr. W. Ramsay, who claims priority of discovery, and points out that he had already determined the critical point in a memoir which appeared in the *Proceedings of the Royal Society* for April 22 and December 16, 1880.—On the application of Ampère's method to the investigation of the elementary law of electric induction by variation of intensity, by M. Quet.—On boron, by M. A. Joly. In this paper the author determines the existence of a combination of boron and carbon, reserving for a future communication a study of the various compounds containing these two elements.—On the blood plaquettes of M. Bizzozero, and on Norris's third or invisible blood corpuscle, by M. G. Hayem. It is shown that the so-called "plaquettes," claimed by Bizzozero as a new discovery in the Italian *Archives of Biology* for January, 1882, *et seq.*, are simply the "hæmatoblasts" already described by M. Hayem. On the other hand Norris's "third or invisible corpuscle," which had been identified with the hæmatoblasts, appears not to be a new element at all, but merely an artificial product resulting from the various manipulations to which the blood had been subjected by the English observer.—Experimental researches on some phenomena relative to the absorption of animal fats, by M. A. Lebedeff.—On the true character of the ophthalmic affection known as astigmatic keratite, by M. G. Martin.—New researches on the curve of the muscular shock in various affections of the nervo-muscular system, with three illustrations, by M. Maurice Mendelssohn.—Influence of sea water on freshwater animals, and of fresh water on marine fauna, by M. Felix Plateau.—On barometric pressure in connection with igneous eruptions, by M. Fr. Laur. It is argued that gaseous and other eruptions are due exclusively to rapid variations of atmospheric pressure.

BERLIN

Physiological Society, July 20.—Prof. Kronecker reported a number of investigations recently carried out in the division of the Physiological Institute under his care: Dr. Openschewsky had continued his observations, communicated at the meeting of June 15 (NATURE, vol. xxviii. p. 264), regarding the influence of the vagus on rhythmical movements of the cardia produced by artificial anæmia. As the result of his further examination he found that the vagus sent two branches of nerves to the cardia: one causing its contraction, the other, when alone stimulated, its dilatation. In the vagus trunk the enlarging nerves were in the preponderance, and, on the whole of the vagus being stimulated, induced an interception of the contractions of the cardia. In a demonstration of the experiment it was shown that after destruction of the stimulating branch of the vagus the irritation of its trunk invariably provoked dilatations of the cardia.—Dr. Jacub had made experiments regarding the strength and rhythm of the movements of the uterus, and regarding the influence on these movements of a number of substances, such as secale, ether, chloral, strychnine.—Herr Aronsohn had instituted a long series of observations on the physiology of smell, observations which he himself communicated to the meeting. It is well known that Weber, from experiments made with eau-de-cologne, had laid down the statement hitherto universally accepted that gaseous substances were alone capable of stimulating the extremities of the olfactory nerves. In opposition, however, to this doctrine there was the fact of fishes being able to smell, a fact Herr Aronsohn conclusively established. Ants' eggs, which are greedily devoured by goldfishes, he saturated with a strong flavour of asafœtida, and on placing them within reach of a number of hungry goldfishes they all darted away from the otherwise savoury food. He therefore repeated Weber's experiment exactly in the manner prescribed, and had, like him, his sense of smell affected only during the infusion of the eau-de-cologne solution. Immediately, however, such an intense sensation of pain was experienced, that the experiment had very soon to be abandoned. It was evident that Weber's solution was much too concentrated, and that in order to achieve trustworthy results dilutions of much larger proportion would have to be made use of. Moreover, for the purpose of solution, instead of the water which produced so powerful an effect on the tissue, the common salt solution of ·6 per cent., which was of indifferent effect, would require to be employed. Finally the due temperature would have to be imparted to the fluid. Under these conditions a long series of experiments was now instituted with oil of nettles, camphor, eau-de-cologne, and other smelling substances. In far the greater number of cases these experiments yielded positive results. Granted that the solutions had the necessary degree of dilution (which among the different materials varied from ·1 to ·001 per cent.) and the due temperature (which might range from 37° to 62° C., though from 40° to 44° C. proved the most suitable), then on their application to the nostrils a decided and lasting smell was perceived. These experiments were not only carried out by Herr Aronsohn himself, but were repeated by other competent observers, the due degrees of dilution and temperature, which differed according to the different observers, producing always the same effect. The result in the one case as in the other was invariably positive, and went to refute the hitherto current notion that gaseous substances alone affected the sense of smell and that fluids had no effect on the olfactory nerves. On emptying out the fluid there was mostly always left a scent of which one remained sensible for a very considerable time. Contrary to former declarations, the breath emitted from the lungs also decidedly affected the olfactory nerves, provided the experiment were conducted in such a way that the particles to be smelled on expiration could reach the upper parts of the nostril. Herr Aronsohn finally made observations tending to establish the liability to weariness of the sense of smell, a fact of which any one might readily convince himself by the following experiment:—Let him take two roses, A and B, as like each other as possible; let him now first smell A for fifteen consecutive seconds, and then on trying B he will find it has very much less scent, or none at all. Let the olfactory sense now recover itself, and then let him, conversely, first smell B for fifteen seconds, and pass to A; he will now find the same defective or negative scent in A as formerly in B.—Dr. Kireef directed his observations towards the discovery of the conditions determining the fact that now and again, by the cutting of one carotid animals could not be bled, but in order to this end a second carotid must also be cut. In the pursuit of this problem a series of important facts came to light demanding further searching study, and which therefore can here for the present only be alluded to. In all the larger arteries it has been observed that on the cutting of a blood vessel only a certain fraction of the total blood, from about two-thirds to five-sevenths, runs away, and then without any visible cause the bleeding stops, though the wound is still gaping wide, and no trombus is forthcoming. Let another equally large artery be opened, and a quantity of blood, often considerable, will issue from it in turn, and then of itself cease; and still a third artery may be cut, which will again yield a further bleeding. The quantity of blood circulating in the body has no influence on this phenomenon. From a certain artery the same quantity of blood was discharged, alike whether a ·6 per cent solution of common salt was beforehand largely injected into the animal, or a portion of blood withdrawn from it beforehand. Just as little influence has the blood pressure on the quantity of blood shed through the cutting of a larger artery. In an animal one *arteria femoralis* was freely cleared out of its integuments for a considerable extent of its surroundings, while another was left in its natural position. The last on being cut shed a certain quantity of blood at double the speed, *i.e.* in half the time taken by the freely cleared artery. The vagus showed a very remarkable influence on the bleeding from a cut artery, an influence to be further traced and demonstrated in the continuation of the experiments.

CONTENTS

THURSDAY, AUGUST 23, 1883

DECENTRALISATION IN SCIENCE

THE increasing recognition of the importance of the natural sciences in education, the daily augmenting numbers of those who devote themselves to the practical cultivation of these sciences, and the still more rapid growth of a widespread general interest in and sympathy with such pursuits have been noticed with no small satisfaction by all to whom the progress of natural knowledge is dear. It is impossible even plausibly to conjecture what changes this awakening may eventually involve. At the conclusion of the disastrous Prusso-Austrian war one of the members of the Austrian Reichsrath began his speech by insisting that the first question to be decided in the reconstruction of his country was whether the doctrines of Darwin were true or not. This may have been an exaggerated way of putting the matter, but already we see in how many directions the doctrine of evolution is capable of application to social problems.

There is one aspect of the increasing attention to the cultivation of science which perhaps the students of science have not sufficiently considered, but which certainly merits their careful attention—the growing tendency to decentralisation which is in progress among us. To realise what this tendency is and what it is leading to we should contrast the present condition of things with what existed twenty years ago or more. For one school in which science was taught then, there are a hundred wherein it is taught now. New colleges have been founded in various centres of industry for special instruction in science. New professorships for the cultivation of different branches of science have been established at some of the older seats of learning. Parliament votes an annual sum of 4000*l.* for the encouragement of original research. New journals for the illustration of scientific progress have been started. Almost every large publishing firm has organised a series of science class-books.

As a result of this accelerated activity a great stimulus has been given to local effort in the prosecution of scientific studies. Field clubs and societies have sprung up all over the country. From modest beginnings some of these organisations have attained not inconsiderable importance. Their membership has steadily grown. Their funds have proportionately increased. They have not contented themselves with merely meeting for pleasant gossip, though this too they have been far from despising. They have encouraged original observation among their members, and have published in their annual volumes of *Proceedings* some really valuable contributions to science. Year by year these volumes make their appearance, until they now form a notable feature in the scientific literature of our time. The local character of the organisation stimulates a local *esprit de corps.* The flora, or fauna, or geology of the locality attracts the activity of the members, who are proud to add to what may already have been known on the subject. But topics of a more general kind are likewise included, and sometimes a paper of high importance makes its appearance side by side with the local contributions. In this way an outlet is furnished for the scientific ardour of the district. The meetings and discussions keep alive a general interest, and the publication of the *Proceedings* encourages the working members to continue their researches.

The rapid appearance and multiplication of these local centres of scientific activity must materially influence the future progress of science among us. In what various directions this influence may make itself felt remains to be seen. But there is one in which it cannot but be potent, and to which brief allusion may be made here. Not many years ago the metropolitan scientific societies were justly regarded as the great centres of progress in science—the heart that sent its intellectual life-blood to the remotest parts of the kingdom. But even their most devoted champions must admit that in this respect they do not now generally fulfil the part they formerly played, and that they are doing so less and less every year. Of course the Royal Society has always stood and will always stand alone and without rival. But such a society as the Geological has competitors all over the country, which, though they may not be individually formidable, yet collectively withdraw not a little of the energy which would otherwise have gone to recruit the parent society here. Every English geologist is proud of the part which the Geological Society of London has taken in the progress of geology, and would like to see the Society retain its influence and position. But the circumstances under which it was founded seventy-six years ago have entirely changed, and its preeminence and continued usefulness must depend upon other conditions than those which gave it so honoured a place in the early part of the century.

It would of course be absurd to speak of the existence of any rivalry between the provincial and metropolitan societies. There is ample room for all. But if there is no rivalry among them there is just as little cooperation. They all act with the most complete independence of each other, and if in some cases they occupy the same ground and do the same work, there is no means at present of preventing this. Now the question arises, whether the general progress of science could not be benefited by the establishment of some concert between the older or mother societies here and the numerous societies, institutes, field clubs, and other organisations of the provinces. These provincial associations have increased and are increasing so rapidly, they are becoming so important a factor in the cultivation of the natural sciences throughout the country, absorbing as they do so much of the talent, energy, and money of the well-wishers of these sciences, that the time has probably come for asking whether some scheme of cooperation might not now be devised whereby they and the London societies would in some way be conjoined for the furtherance of their common objects. Obviously subjects which are preeminently local should be left in the hands of the local organisations. On the other hand, general questions, especially those bearing on scientific theory or classification, might be most effectually dealt with by the more important metropolitan bodies. We refer of course mainly to publication. The local societies would feel justly aggrieved were they asked to deprive themselves of the pleasure of starting new hypotheses and running down old ones. But they might be content with this pleasure at their meetings without wasting their funds and loading scientific literature by printing their vagaries in the *Proceedings.* The central societies also, by giving up the publi

cation of unimportant and especially of local details, would be better able to concentrate their strength on large questions, to the notable increase of the value of their *Transactions* or *Proceedings*. That such a re-arrangement of effort would involve many practical difficulties is sufficiently obvious, and that the machinery might never be made to work smoothly may likewise be granted. Yet surely it would be well worth while to try whether some of the energy which at present is wasted or misdirected could not be utilised to the manifest advantage of that progress which all have sincerely at heart.

Students who have occasion to keep themselves acquainted with the current literature of their respective sciences naturally grumble at the constant increase in the number of journals, *Proceedings*, *Transactions*, &c., which they must painfully look over. But this increase is inevitable. What we should aim at is not its curtailment so much as its methodical arrangement. If certain societies would only publish papers in particular departments of a science, it would be infinitely easier to follow the yearly advance made in that science. The metropolitan societies might annually issue with their own *Proceedings* brief digests of the additions to our knowledge made by the country organisations and otherwise, so as to comprise within the boards of one volume a view of the whole progress in theory and detail achieved by each science in this country. At all events some means should be devised of enabling the older and the younger and less ambitious societies to draw together into concerted action, either by formal arrangement or by informal and friendly correspondence.

ESSAYS IN PHILOSOPHICAL CRITICISM

Essays in Philosophical Criticism. Edited by Andrew Seth and R. B. Haldane, with a Preface by Edward Caird. (Longmans, Green, and Co., 1883.)

ONE of the most interesting among the intellectual movements now taking place in this country is the growth and development of that system of philosophical thought which began with Kant, flourished in Germany, and, spreading to England, has only just begun to take root in the minds of some of our ablest thinkers. It is a curious thing to see this exotic springing up thus vigorously side by side with our endemic productions—the one like a vine creeping with the tendrils of its subtle and sensitive analysis; the others, like our British oaks, contented sturdily to rest in the stiff soil of experience without seeking for any supports in the thin air of metaphysics. So rarely has this foreign plant found its way across the Channel that until within the last few years it was scarcely ever to be met with even in the more cultured of our philosophical pleasure-grounds. Probably the last of all the gardens into which it is likely to find its way is that of natural science, and therefore we publish this short notice in order to inform any of our readers who may desire to see the plant in question where they may profitably go to see it, and have all its main features explained to them in admirable English and with the least possible expenditure of time. For these "Essays in Philosophical Criticism" only cover 277 pages, are all written by men of marked ability, who are well saturated with the philosophy which they undertake not only to expound but to extend.

The pages of NATURE, however, are not adapted to a criticism of such a "Criticism" as a whole; were such the case we should of course have taken the works of Professors Green and Caird as the representative expositions in this country of the German school of philosophical thinking. But there is one important point of contact between this school of thinking and that of natural science which does come within the province of the latter to examine, and it is because this point is prominently put forward in the book before us that we have chosen these "Essays" as the subject of our review. The point to which we allude is the doctrine that science can no longer afford to disregard the revelations of transcendental analytic; that if any considerable progress is henceforward to be made in the investigation of the facts of nature, it can only be done in the light which is shed by the "theory of knowledge," and that if "a man of science" does not happen to be acquainted with the use of the "categories," his education is in as sorry a case as that of a young lady who has never been taught the use of the globes: "he perpetually raises difficulties insoluble for himself in his own department by the dogmatic application of mistaken categories." Now we have had the good fortune to meet no small number of young ladies who know their geography sufficiently well without ever having attained to the use of the globes, and we have met with a still greater number of "men of science" who have done exceedingly good work "in their own department," without ever having heard of the "categories." May it not be that both the schoolmistresses and the philosophers are alike in somewhat unduly magnifying their office? As regards the philosophers, this is the only point with which we are here concerned.

In the concluding paragraph of a highly interesting and ably written essay by Mr. R. B. and Mr. J. S. Haldane, on "The Relation of Philosophy to Science," it is said by way of summary: "Such considerations point towards what seems to be becoming the conclusion of the present time—that science and philosophy can no longer be kept wholly apart from one another." The considerations which lead to this conclusion briefly stated are as follows:—Science has hitherto been concerned only with the lower categories of substance, quantity, causation, mechanism, &c., to the exclusion of those higher conceptions of organism and teleology, without which it is impossible to take a full or comprehensive view of all the facts which fall to be explained. Thus, for instance, if biology restricts itself to investigating the phenomena of life only under the categories of mechanism and causation, it can never attain to the all round understanding of the facts of its own subject-matter as afforded by that changing of the points of view which is rendered possible by the use of the conceptions above mentioned. These conceptions amount to regarding an organism as something more than a mechanism which stands to be investigated by measurement and the tracing of physical causation alone—to regarding an organism as that which exhibits the peculiarity of every part being acted on by the other parts, and by the environment, so as to form a self-conserving system, of which it is "the essential feature of each part that it is a member of an ideal whole"—morphological structure, physiological function, growth, development, decay, and death

being all teleological factors in the expression of this "ideal."

Now in the first place we do not require a revelation from another sphere to tell us that "there's ne'er a villain dwelling in all Denmark but he's an arrant knave," and biologists may similarly remark that they do not require any transcendental analytic to inform them that an organism is something more than a mechanism. But it is indeed a startling announcement to be told that in the investigation of an organism we are to rise above "the category of causation," and carry into our inquiry the conception of teleology. And still more startling is this announcement when we are told that the teleology which we are thus to embrace is not in any way connected with the hypothesis of a designing mind, but is a something which we ourselves are, as it were, to read into the facts which we investigate, by means of a "creative synthesis of thought." It is here, we think, that the "men of science" ought to take their stand; we are all agreed that an organism is something more than a mechanism, but we are not agreed that in any department of science are we justified in quitting the category of causation. On the contrary, for our own part we decidedly maintain that this is the category the limits of which mark the limits of all scientific research, and that in whatever degree science presumes to overstep these limits, in that degree has it ceased to be science and become metaphysical speculation. Moreover, we should say that the speculation is, so far as science is concerned, of an exceedingly vicious kind. It was bad enough to have the "final causes" of the older teleologists posited as the ultimate touchstones of scientific truth; but it seems to us much worse to have a system of teleology of our own manufacture put into its place. Thus, to take an illustration, it is the outcome of a judicious "application of the categories" to assert that a great gulf is fixed between the living and the not living in nature, and therefore that "we can never hope to find a case of *abiogenesis* as a matter of fact." Now we conceive it is the part of a man of science as such to entertain no such bold statement as this. It is, to use a term of which this school of philosophers is particularly fond, the worst form of "dogmatism" thus to affirm, on grounds of metaphysical speculation alone, the antecedent impossibility of any discovery in science—most of all with reference to a matter touching which we are so much in the dark. If our working biologists were ever to adopt the categories as guides to their methods of inquiry, here is a case in which all attempts at inquiry would be barred by an *a priori* dogma; and the same is true of every other case where the "category of causation" is sought to be overshadowed by "the higher categories." Thus, to take another example, in order to show the necessity for the employment of these higher categories in science, it is argued that the regeneration of the amputated limb of the newt is "wholly unintelligible," save as an expression of the teleological impulse to the reconstruction of our ideal type or organism. But, without waiting to ask what becomes of such an impulse in the case of any of the higher Vertebrata when similarly mutilated (perhaps the matter in this case *is* "wholly unintelligible," but whether or no the illustration can scarcely be deemed a happy one), we object, in the first place, that by discarding the category of causation an *a priori* barrier is arbitrarily set up against any scientific inquiry into the facts; and in the next place, that the higher categories cannot possibly furnish any semblance of what may properly be termed an "explanation." To say that "each cell is directly determined in its action simply by what it has to do in order that the vital activity of the newt may be restored to its normal condition," is not to explain the process; it is merely to restate the fact. And in all similar cases the so-called "explanation" which is furnished by the higher categories amounts to nothing more than saying that the thing to be explained is what it is The truth, in short, is that outside the category of causation we cannot explain anything in a scientific sense. We may change our "point of view" as often as we choose by regarding a thing now as mechanism, now as organism, now as beautiful, again as moral, and (if we may be allowed to add to the categories) lastly as comical. But by thus changing our point of view we are in no wise adding to our knowledge in the way of explanation; we are merely regarding one aspect of a thing to the exclusion of its other aspects.

Let it not be thought that in making these remarks we are actuated by any *animus* against the transcendentalists. In the region of philosophy their "Copernican change of thought," which makes the universe revolve round the philosopher, may be a change fraught with all the importance which its adherents claim for it. With this, as we have said, we are not here concerned; we are only considering the system "from one point of view," or in its relation to science, and here we find that its teaching appears to be seriously at fault. We have endeavoured to show that it is not only of no use to puzzle the "plain man" of Locke in the person of the modern biologist by telling him that "the organism, *quâ* organism, is not in space at all;" but that even if the biologist could be made to understand what is meant by such a statement, his acceptance of the meaning would be worse than useless to him in his work. Far, therefore, from feeling with our authors that for "such a class (*i.e.* specialists in science) the mastery of the critical investigations of Kant and Hegel, or at least of conceptions which have been profoundly influenced by these writers, will in the near future be absolutely essential," we believe that the less men of science, in their capacity as such, have to do with these investigations the better will it be for the progress of their own. And, on the other hand, seeing that the critical philosophers are so ready with their advice, we may in our turn conclude with a word of advice to them, by observing that it will be the better for the credit of their system if they cease from their kindly endeavours at teaching our Hannibals to fight.

GEORGE J. ROMANES

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications.*

[*The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to insure the appearance even of communications containing interesting and novel facts.*]

Simultaneous Affections of the Barometer

I HAVE just read in the columns of your journal the very interesting communication (Part II.) from Mr. A. N. Pearson

regarding the transmission eastwards of barometric movements, and in which he likewise alludes to simultaneous affections of the barometer. Speaking of these latter he says: "As to the cause of the e widely distributed simultaneous movements of the barometer . . . I have no evidence of any value. The most natural idea is that a connection direct or indirect may be traced between them and changes in the state of solar energy. . . . I have not a sunspot curve for the years under consideration, and cannot therefore make the necessary comparisons."

I may mention that simultaneous affections of atmospheric pressure were fir t observed by the late John Allan Broun, and that I have compared his instances with sunspot records. The results of this comparison were published last year in the *Proceedings of the Literary and Philosophical Society of Manchester*. I have not the volume here with me, but if my memory does not deceive me an increase of pressure was found to be associated with increasing unspots and a decrease of pressure with decreasing sunspot. BALFOUR STEWART

Devonshire, August 17

Dreaming

INSTANCES have lately been described in NATURE of remarkable formation or perversion of dreams at the instant of waking. Allow me to offer you the following, which was vividly impressed on my mind, and which I still remember with the utmost accuracy:—

In the summer of 1822, when an undergraduate of Trinity College, Cambridge, I was permitted to reside in College rooms during the summer long vacation. As fires were not wanted in our sitting-rooms, it was customary for each resident's bedmaker or other officer to carry his water-kettle for breakfast and tea to the College kitchen, and bring it back with water boiling. On one occasion I had overslept my usual hour, and I dreamed a dream. I was at the gate of a country farmyard well known to me, and there came a long procession of horses, asses, oxen, hogs, sheep, and all the animals usually to be found in a farmyard, followed by a north country drover with his plaid or maude crossed over his shoulder, who walked up to me and said, "Sir, I have brought your cattle." In an instant I perceived and actually heard (so intimately were the auditory sounds and the intellectual interpretation intermixed) that my bedmaker was at my chamber door calling to me, "Sir, I have brought your kettle." The hearing had been confused; there had been no reasoning; but there had been instantaneous vigour of creative imagination.

An admirable instance of the same kind is described in the last chapter of Scott's "Rob Roy." Scott appears to have been in some measure a student of dreams. I refer with pleasure to the description of FitzJames's dream, after a day of labour and an evening of excitement, at the end of the first canto of "The Lady of the Lake." A. B. G.

August 18

Thunderstorms and Auroræ

I WOULD like to ask if any observer has ever suggested a possible connection between thunderstorms and the aurora? Last evening a very heavy shower, accompanied by much lightning, passed to the north of this place. Other black clouds were seen to the south and west, and at nine o'clock flashes of lightning might be seen darting across the clouds in nearly all directions. It was evident that the air was heavily charged with electricity. Just before retiring, about midnight, I looked from my window to see if a shower was still threatened at this point. I found the heavens quite clear except in the north, where a dark mass of clouds still hung. At the eastern extremity of this cloud-bank a rift several degrees wide commenced and extended nearly to the north-western horizon. Frequent flashes of lightning lit up the edges of this rift, while beyond the clouds the clear sky was seen to be brightly illumined by a steady auroral glow. The glow continued nearly unchanged during the several minutes which I watched it, and it was quite evident that it was a genuine aurora, and not a reflection of the lightning flashes. Is it not probable that the same electrical state of the atmosphere which produces the thunderstorms may also cause the aurora, and that the two phenomena may often occur together?

Lewiston, Maine, U.S.A., July 6 E. R. CHADBOURN

"Elevation and Subsidence"

QUOTING Prof. Geikie's "Text-book," Mr. Starkie Gardner says: "Strata of sedimentary origin which have accumulated to thousands of feet in thickness, may be depressed deep beneath the surface and brought within the influence of metamorphosis. . . ." He continues: "This is an absolute admission that at some depth, relatively not great, pressure converts solid into viscous or fluid strata." A few lines further: "If the mere pressure of overlying strata can anywhere or at any depth render rocks molten or fluid, they will become molten or fluid wherever the required pressure occurs." But is not the supposition the exact reverse of what is really the case, viz. that not only does pressure *not* liquefy rocks, but actually *prevents* their melting at a temperature at which they *would* melt were the pressure removed? Mr. Gardner himself admits this in the case of the nucleus, *i.e.* when we come to very extreme pressures; how then can pressures of an intermediate order of magnitude have the opposite effect? This is surely not the view of Prof. Geikie. The passage quoted by Mr. Gardner from his work refers to the fusion of rocks by the *high temperature* found at great depths.

If Mr. Gardner means to imply, as some of his expressions strongly suggest, that the *cause* of the high temperature of the interior of the earth is the pressure of the superincumbent rocks, it would be interesting to know how he reconciles his theory with the principle of the conservation of energy. Heat is energy, pressure is force. Force can only give rise to a manifestation of energy by acting through a finite distance, the energy manifested, or "work done," being the product of the force and the distance through which it acts. If either factor be zero, the other not being infinite, the product is also zero. The application to the case in hand is too obvious to require statement.

Trinity College, Cambridge, August 4 F. YOUNG

Insects and Flowers

HAVING this morning received the last number of the *Proceedings of the Linnean Society* containing Mr. A. W. Bennett's and Mr. Christy's observations on the constancy of insects in their visits to flowers, it occurred to me, after reading only the first page, to see how insects behaved in my own garden, where there is a great variety of plants. I had not read the conclusions of either author, and had no preconceived opinion on the subject. The results were noted at once, and were as follows:—

1. *P. rapæ* (small white butterfly) on a bed containing white and rose-coloured double and single stocks also, scarlet pelargoniums and pink phlox; visited single white stocks only, going all round the circle in which they were planted; then flew off, made a dive at a white phlox, but did not alight, hovered about some little time without alighting, and finally went out of sight.

2. Same species; two individuals on a bed of scarlet pelargoniums edged with sulphur-coloured pansy (? *Viola lutea*, var.). One butterfly kept to the pelargoniums, paying repeated visits; the other did the same with the pansies.

3. Same species on a bed of dark purple pansies with bright yellow eye, crossed and edged with orange French marigolds. Two individuals visited both plants promiscuously, but the marigolds oftenest. A *P. napi* (green-veined white butterfly) did the same. *Vanessa urticæ* (small tortoiseshell butterfly) on the same bed visited only the marigolds. This species seems remarkably partial to yellow.

4. *P. rapæ* on a bed of scarlet pelargonium and pale blue pansy with dark centre and pale yellow eye intermixed. Visited the pansies very often; the pelargoniums once only during observation.

5. Same species on *Lythrum salicaria* remained a long time visiting different spikes, then flew round, neglecting all other flowers till it found another plant of the same kind, which it continued on as long as I watched it.

Bombus lucorum. This bee was very abundant, both workers and females. I watched them on a mixed bed which contained *Pentstemon barbatum* (dull scarlet), African marigolds (yellow), *Antirrhinum majus* (crimson), pansies, both dark purple and yellowish white, and mignonette. The favourite plant was the pentstemon, especially with the ♀. They got at the nectary by inserting the proboscis in a hole cut near the base of the corolla. The next favourites were the marigolds. One individual confined himself exclusively to the antirrhinum. In one case only did I observe a bee to change from one kind of flower to another, though I looked out especially to see if they did so. This was a bee which went from a crimson petunia to an antirrhinum of very nearly the same shade of crimson.

Apis mellifica (hive bee) on the above bed confined itself to the mignonette. This remark applies to several individuals.

Chester, August 17 ALFRED O. WALKER

A Meteor

A BEAUTIFUL meteor was seen from this place on Sunday evening, August 19, at 10.3 precisely. Owing to the brilliancy of the moon, stars of the first magnitude were but faintly seen. I should say the size and brilliancy of the meteor was greatly in excess of the planet Venus at its best. It was visible as far as I could conjecture about three seconds, and pursued a course of probably 45 or 50 degrees, proceeding from a point a few degrees to the eastward of, and higher than, the north star. It moved almost in a straight line downwards with an inclination to the left. When it had got about half of its whole visible course, it seemed to get blue in colour, and threw off a mass of red sparks, and continued for the rest of the distance, when it appeared to burst, and the disjected fragments were red and visible for a few moments. The colour for the most part was very much like that of Venus, indeed, for the whole of the course, except where it seemed to turn blue. A. TREVOR CRISPIN

Lansdowne Road, Wimbledon, S.W., August 21

I SAW a very brilliant meteor from the promenade here last night (Sunday, August 19), at 10.3 p.m. It passed along the eastern sky and vanished over the summit of the Little Orme. The meteor was, I think, more brilliant than Venus at her brightest, though the full moon was shining not far off and very few stars were visible. The path was northward, nearly horizontal, inclined a little downwards, about 10° or 12° above the horizon, apparently much foreshortened, for the motion was very slow—not faster than that of balls falling from a rocket; white light, slightly tinged with blue. The meteor divided, and left one large and I think several smaller portions behind it, all vanishing together. It should have been seen overhead towards the coast of Yorkshire. ALBERT J. MOTT

Llandudno, August 20

Animal Intelligence

A CIRCUMSTANCE exceedingly illustrative of the sagacity of the horse was witnessed by myself in the neighbourhood of Nottingham. I had been out for a stroll by way of recreation, returning home across some fields by the Trent side, and when nearly opposite Clifton Grove I stopped a short time to watch a man angling in the river, when suddenly my attention was drawn to a mare with her foal, not many yards distant from where I was standing, open two gates which were *vice versâ*, closing with a strong spring. Her *modus operandi* was to place her nose in between the two gates and force one gate open with her side, while she had no little difficulty in opening the other for the purpose of getting through. I have learnt that the animal had not been trained to do this, but taught by natural instinct, and so cleverly was it done that man could scarcely have performed the action better. Thinking this instance of sagacity might be interesting to some of our naturalists, I take the liberty of forwarding same in order that you may insert it in your valuable paper.

9, Charlotte Street, Nottingham F. WELCH

MR. H. CECIL's communication respecting the cat and the chicken, at p. 320 of your present volume, reminds me of an instance of the attachment of a cat to its natural prey which is still more remarkable, as there was no "maternal στοργή" in question.

Some years ago we had a young emasculated tom cat. When it was nearly full grown we had two young white rabbits brought in which had lost their mother. These were kept in the kitchen, and fed by pouring milk into their mouths with a spoon. They were placed in a basket at night and covered up to protect them from the cat, which was in the habit of catching wild rabbits. One morning the cover was found to have been removed by the cat, which was lying in the basket with the little rabbits. From that time he took charge of them, teaching them to lap milk and watching over them like a mother, even to the extent of driving them home when they grew older and rambled out from the kitchen. The friendship continued till the rabbits grew up, when we lost them by disease. ALFRED O. WALKER

Chester, August 17

"Birds and Cholera"

IN refererence to "H. M. C's." letter in this week's NATURE (p. 366), it is interesting to recall how the traveller Jackson, speaking of the plague that occurred in West Barbary when he was there, says, "The birds of the air fled away from the abodes of men." Thomas Moore, in "Paradise and the Peri," refers to this fact. E. S. T.

August 18

LIQUID FILMS AND MOLECULAR MAGNITUDES

SIR WILLIAM THOMSON'S lecture on "The Size of Atoms," which has recently been published in NATURE, will undoubtedly increase the interest felt in measurements which throw any light upon the values of molecular magnitudes.

We have for some time been engaged in investigating the properties of very thin liquid films, and in our last communication to the Royal Society (of which only an abstract has been hitherto published, but which will appear in a forthcoming number of the *Philosophical Transactions*) we have described two independent methods by which we have obtained concordant measurements of the thickness of soap films in the last stage of tenuity, viz. when exhibiting the black of the first order of Newton's rings.

The paper had not been sent in to the Royal Society at the time when Sir Wm. Thomson's lecture was delivered, but, on receiving the abstract, he has been good enough to express his approval of our methods and interest in our results, and to raise some questions as to the relation between the observations of Newton and ourselves, the further discussion of which he thinks would be interesting to the readers of NATURE.

We propose therefore briefly to discuss the facts which bear upon the points raised by Sir Wm. Thomson, and to describe our methods of experiment so far as may be necessary to make the discussion intelligible.

For thicknesses greater than those which correspond to colours of the first order, the tint displayed affords to a practised eye (when combined with a knowledge of the angle of incidence and refractive index) a very accurate measure of the thickness of a film. In some experiments of our own, in which on more than 500 occasions two independent but simultaneous measures were made of film-thicknesses by means of two beams of light, incident at different angles, we found that the two values obtained agreed to within 1 per cent. in 52 measures out of every hundred, to within 2 per cent. in 84, and to within 3 per cent. in 95. All these observations were made in the second and higher orders. The colours of the first order vary from point to point too slowly to enable trustworthy estimates of the thickness to be made, and when the black of the first order is reached the eye informs us only that the thickness must be less than a certain value, but affords no further indications as to what it really is. The fact that it is extremely small, and the possibility that it may be related to the magnitude of the so-called "radius of molecular attraction," invest the problem of the determination of this thickness with special interest. We have succeeded in solving it by two methods. In each case an assumption has to be made for which there is no direct experimental evidence. In each case, however, the assumption is different, and the fact that the mean results obtained by the two methods are in close accord is sufficient to show that, although there is still room for further inquiry, the mean thickness of the black soap films examined was correctly determined to within a fraction of a millionth of a millimetre.

The first method consisted in measuring the electrical resistance of a cylindrical black soap film, and deducing the thickness from Ohm's law, on the assumption that the

specific resistance of the liquid, when drawn out into so thin a film, is the same as that determined under ordinary conditions.

We have, by direct experiment, proved that this assumption is true for films the thickness of which exceeds 374×10^{-6} mm. (*Philosophical Transactions*, 1881, p. 447). The investigation was considerably protracted by the great difficulty experienced in maintaining the constitution of the films even approximately constant. Every change in temperature, every slight alteration in the hygrometric state of the air in the glass chamber in which the bubbles were formed, involved a loss or gain of water which affected the specific resistance so largely as to make any certain conclusion impossible. It is only in our latest apparatus that we have secured the requisite constancy in the conditions. In it the films are formed in a chamber surrounded by water to keep the temperature constant. The base of the inclosed space is covered by the solution used, and the complete saturation of the air is further secured by an endless band of linen passing over rollers which can be worked from the outside. The lower roller is immersed in the solution employed, and thus every part of the linen can in turn be dipped into the liquid and kept completely saturated without opening the case. The films are blown as spherical bubbles with air which has been caused to pass over some of the liquid in order to insure saturation; they are converted from spheres into cylinders adhering to two rings, and are further put in communication at any desired point with the electrical apparatus without opening the case, and thus without affecting the temperature or saturation of the air with which they are in contact. A thermometer and a hair-hygrometer, placed in the closed chamber, serve to detect any change of conditions which these precautions fail to obviate.

The earlier form of apparatus described in our paper "On the Electrical Resistance of Thin Liquid Films" (*Phil. Trans.*, *loc. cit.*), was in some respects less perfect. By it, however, we were able to show that the specific resistance of a film differed less and less from that of the liquid from which it was formed, as the temperature and hygrometric state of the air become more and more constant, and that in the case of the six films in which the desired constancy had been most successfully attained, the difference amounted only to 1·8 per cent.

It was also shown that there was no indication of any change in the specific resistance between thicknesses corresponding to the middle of the red of the sixth and of the yellow of the second order respectively. As the smaller of these thicknesses is nearly the same as the wave-length of the rays which bound the spectrum at the blue end, this result may be roughly stated as proving that the thickness of a film may be reduced to the length of the shortest visible light wave without any change in the specific electrical resistance of the liquid of which it is composed.

In the course of some of our earlier experiments (*Proc. Roy. Soc.*, 1877, p. 334) we had been fortunate enough to make a soap solution, giving very persistent films, which frequently thinned to the black of the first order. The resistance of the black portion was measured on several occasions, and it was found that the thickness was in all cases nearly the same (the variations amounted to about 5 per cent.), and differed but little (if the specific resistance was assumed equal to that of the liquid in mass) from 12 millionths of a millimetre (12×10^{-6} mm.).

We were anxious to try this experiment again with our improved apparatus and methods of measurement, but great difficulty was experienced in obtaining a liquid which would both thin and last sufficiently for our purpose. We have not, in fact, succeeded in again making a solution, containing the proportion of glycerine recommended by M. Plateau, which would behave in the desired way, but we find that a liquid of similar constitution, in which the glycerine is replaced by water, will allow a measurement of the resistance of the black to be made in the case of about one film out of every three or four.

Films which do not contain glycerine generally exhibit greater irregularities of behaviour than those which do, and thus our later experiments are not in as close agreement as the earlier ones. They indicate that, whereas the thickness of the black portion of a film remains constant however much its area may alter, it is different in different films. All the values obtained lay between $14·5 \times 10^{-6}$, and $7·2 \times 10^{-6}$ mm., and the mean value $11·7 \times 10^{-6}$ differed only by two ten-millionths of a millimetre (2×10^{-7} mm.) from our previous result.

In spite of this close agreement these results were open to criticism. It is a long way, in terms of molecular magnitudes, from the yellow of the second to the black of the first order. We had no right to argue from results on the specific resistance at the greater thickness to its constancy at the less. It was, therefore, very important to attempt to check our observations by some independent method.

We had often observed that plane circular films formed in a glass tube thinned very readily to the black. This was perhaps due to the fact that the small aggregation of liquid all round the film affords a channel by means of which the liquid can readily escape. However this may be, it occurred to us that, though it was probably impossible to measure the thickness of a single black film by any optical method, it might nevertheless be possible to determine the total thickness of a number of parallel films in a tube. This we have succeeded in doing. The tube and its contents were placed on an apparatus for producing interference by thick plates. One of the interfering rays passed through the tube. A few steel sewing needles were included within it. When the films became black, a number of them were broken by moving the needles with a magnet, and the thickness could be calculated by observing the positions of the interference fringes before and after the rupture. By this method the mean thickness of the films was measured, on the assumption that the refractive index of the films was the same as that of the liquid in mass. Various considerations led to the conclusion that this was probably correct, but in any case the complete independence of the electrical and optical methods made each a valuable check on the other, though—if the fundamental assumption was correct—the former was by far the more accurate.

The result showed the two methods in most satisfactory accord. The mean of all the electrical observations gave a thickness of $11·8 \times 10^{-6}$ mm., that of all the optical $11·4 \times 10^{-6}$ mm., an agreement which places it beyond doubt that the mean value for all the films observed was really about $11·6 \times 10^{-6}$ mm.

The methods employed then afford a definite measure of thicknesses much smaller than the smallest that Newton's scale of colours allows us to estimate. That scale is very uncertain when colours of the first order are employed. The difficulty or impossibility of obtaining perfect contact between the lenses in the fundamental experiment, and the possible distortion of their form in the neighbourhood of the points of closest contact, make colour estimates of thickness in the first order very doubtful. The few observations we have made, on films exhibiting the red and orange of the first order, show a discordance with Newton's results in striking contrast to the agreement obtained in the case of most greater thicknesses.

Our estimate of the thickness of the middle of the red of the first order (284×10^{-6} mm.) differs from Newton's by 20 per cent. In the blue of the second order our own observations on Newton's rings differ from those on the soap films by 6 per cent., and we were obliged, when aiming at an accuracy of 1 per cent., to discard all observa-

tions below the border of the yellow of the second order (374×10^{-6} mm. when the light is incident at 45°).

On the other hand, our electrical observations of a black film often give the same thickness to within 1 or 2 per cent., again and again, in a series of observations extending over an hour or more.

This constancy may be taken as proving that the film is not absorbing nor losing moisture, and if its composition thus remains unaltered it is not too much to say that the electrical method extends to $7 \cdot 2 \times 10^{-6}$ mm. (the smallest thickness measured by us), with an accuracy previously attainable only above 374×10^{-6} mm. In other words, it carries the accurate measure of thickness fifty times nearer molecular magnitudes than Newton's scale of colours does.

We now come to the interesting point raised by Sir William Thomson, which we may perhaps be allowed to state in his own words as follows:—"Newton, in the passage I have quoted (NATURE, vol. xxviii. p. 250), being Observation 17 of the Second Book, Part I., of his 'Optics,' says (1) he found in the *large* black spot smaller black 'round' spots which were blacker still; (2) he saw sunlight reflected from even the small darker spots; (3) the black spots would break out in the middle of white, without any intervention of blue, and sometimes within the yellow or red or blue of second order. This (3) agrees with your (1) of p. 151. But the (1) above of Newton's shows a higher grade of thinness than that of the main black spots, which I presume is that which you have found as $\cdot 1 \times 10^{-5}$. I do not know if you have noticed these smaller and blacker spots. It would be exceedingly interesting if possible to find their thickness, and to see how they seem to be related to the main black spots."

It may be well when answering this inquiry as to whether we have observed the smaller black spots, to state such facts as we have observed connected with the formation of the black.

In the first place we have noticed that the boundary between the film proper, and the small aggregation of liquid which connects it with the solids by which it is supported, is the place where, under ordinary circumstances, discontinuous spots, *i.e.* spots having a thickness different from that of the surrounding film, are most readily formed. The small circular masses of liquid which surrounded the gold wires by which the film was connected with the electrometer were sometimes themselves surrounded by a very narrow ring, showing the white of the first order when all the film immediately outside it was much thicker. Small specks of white would frequently break off from the topmost point of this ring, and either rise through the film to its highest point, or if, as was often the case, the liquid of the film was in a state of internal motion, the white flecks would be carried round the cylinder in spiral paths. Some liquids almost invariably gave films which, shortly before rupture, became thus covered with white flecks. Occasionally a white band, several tenths of a millimetre in breadth, was formed all round the upper ring which carried the cylindrical film, when the portion of film next it showed colours of the second and higher orders, and it was owing (among other reasons) to the frequent presence of this ring that we abandoned the Wheatstone's bridge method used in our first experiments (*Proc. Roy. Soc.* 1877, p. 334), and adopted the electrometer method which we now always employ. The necessity of having to make an allowance for the resistance of the white ring, the thickness of which was much more uncertain than that of the coloured portion was thus avoided. We may remark that irregularities of all kinds are more likely to occur if all parts of the apparatus are not frequently and scrupulously cleaned. We have also examined the lines of discontinuity between the black and the coloured portions, using a microscope with a three-inch object glass. In many cases the discontinuity was seen to be only apparent. Bands of colour were visible, which proved that the missing tints were really there, but on so small a scale as to be invisible to the naked eye.

The phenomenon of the white band was sometimes still further complicated by the presence of spots in the white, differing in colour both from it and the film next it. Thus on one occasion when the colour next the white was the green of the fourth order (mean thickness 893×10^{-6} mm.) we made the following remarks in our notebook:—"At the top a narrow film of white was observed between the green and the solid cylinder. In this, small pieces of deep blue were moving slowly backwards and forwards. The lower part of the white was marked by two small rings of colour, so narrow that the colours were indistinguishable." Later on, the green at

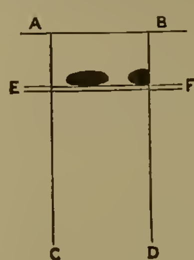


FIG. 1.

one point broke through the white and completed contact with the top. This contact was in turn broken, and, after a while, the white appeared continuous without spots or bridges. A rough, highly enlarged sketch of the spots was made at the time, of which Fig. 1 is a copy.

A B is the lower edge of the platinum cup which supports the film. A C and B D are the boundaries of the bright line produced by the light thrown upon the film at a known angle for the purpose of measuring its thickness. E F is the edge of the white. Two of the blue spots which appeared to float in it are shown, and the narrow line of colour is indicated. Below E F the thickness was about nine times greater than that in the white space above it.

Our reason for describing this observation at length is to draw attention to the curious phenomenon of the blue spots separated by an apparent discontinuity from both

FIG. 2.

the white and the green. From the green they were no doubt separated by a thin line of white, and through this frail band, perhaps a few hundredths of a millimetre broad and one ten-thousandth of a millimetre thick, they were unable to sink into the green below.

In the same way white flecks have been observed to rise and to be separated for some seconds from the white ring above by a thin band of colour. Such flecks, examined by the microscope, sometimes show colours of higher orders within them, arranged in curves owing to a regular vertical circulation. The appended sketch (Fig. 2), drawn from memory, gives some idea of the appearance displayed, the size being of course greatly exaggerated.

Turning now from the formation of the white to that of the black, many of the phenomena observed in the

case of the cylindrical films are closely similar. Specks of black also readily form in the neighbourhood of the solid in contact with the film. They, too, rise through the surrounding liquid, and the growth of the black ring at the top of the film is sometimes caused as much by additions of black spots from below as by a downward motion of the lower edge. These phenomena are only observed on a large scale shortly before the rupture of the film.

The black appears at times in other ways. Sometimes when the white of the first order was in contact with and below the black, a small portion of it would rapidly disintegrate. It would become streaked with black lines, the white portions would fall down through the rifts thus formed, and a sudden extension of the black would thus take place. In films containing small quantities of glycerine this phenomenon is sometimes observed on a very large scale.

There is also a third way in which the black appears, namely, in cases where there is no discontinuity between the white and black. Here the thinning takes place in the normal way, but, as in Newton's observations, specks of a deeper black frequently appear. This phenomenon may easily be shown as a lecture experiment. If a few drops of water be placed on the surface of a piece of yellow soap, and the end of a glass tube ground plane be dipped into them, a film can be removed. On throwing a magnified image of this on the wall, it is observed to thin rapidly. The white often passes through gray into black, and then the deeper black spots appear and rise to the top of the film. Within our experience, however, this phenomenon occurs only in the case of transient films formed of a liquid which does not allow any high degree of persistence. It is for this reason that in the summary of results with which we conclude our paper, and which is given in the abstract (see NATURE, vol. xxviii. p. 142), we limit our statements to "persistent soap films." It is on these only that we have been able to make measurements, and of these only that we have any certain knowledge.

While, therefore, in answer to Sir Wm. Thomson, we are able to say that we have often observed the same phenomenon as Newton, viz. that of a deeper black separated by a line of apparent discontinuity from the less intense black which surrounds it, this observation has only been made in the case of liquids like that used by Newton, which he describes as "water made tenacious by dissolving a little soap in it."

We have made use of two liquids in the experiments on which our published results are based. In the case of the "liquide glycérique" the black was under continual inspection, the colours of the remainder of the film being frequently noted during the experiments, and when the film became very thin and uniform in colour, the observer had plenty of time to study its appearance. We have no recollection of ever having observed any black specks deeper than that of the main mass of black, either stationary, or moving about in it. Had they been formed in large quantity, our electrical measurements must have detected them. They would have risen through the thicker black as the white or black specks do through the coloured parts of the films, and would have congregated in the upper part and formed a ring of greater tenuity at the top. If, as analogy would lead us to suppose likely, they had appeared in greatest quantity towards the end of the film's existence, the resistance of the black area would have increased more rapidly than its length. We tested this by grouping our experiments according to the length of the black area (*Proc. Roy. Soc.*, June 21, 1877, p. 344), and found that the resistance per millimetre was, to within the limits of the errors of experiment, constant, whether the black was less than two or more than ten millimetres in length.

The second liquid, which was formed only of oleate of soda and water, was more similar to Newton's and more likely to give similar results. With this we could obtain such large areas of black that the electrometer method enabled us to measure the resistance of a portion of the black alone, without regard to that of the coloured portions of the films. These films were therefore observed much less closely than those formed of "liquide glycérique," but no eye observation or electrical measurement ever gave any indication of more than a single thickness of the black for each particular film.

Coming now to the optical observations, we have indeed noticed in the earlier stages of the history of the black films a bending of the interference fringes in the lower parts of the black region, which might indicate that near the coloured part of the film it was somewhat thicker than at some distance from it. It is, however, very doubtful whether in this part of the field the light was passing through black films only. The area of the black was not exactly the same for all the fifty or sixty films inclosed in the tube, and thus near the boundary of the black the light might pass through a few white films, which would account for the apparent thickening. We were unable to satisfy ourselves as to which of these explanations is the true one, though the latter is the more probable. The question is fully discussed in our paper, in which we show that if the apparent thickening were really in the black, that colour must begin to show itself at a far greater thickness than that ordinarily assigned to the "beginning of the black," which is unlikely, though not, in view of the great uncertainty which attaches to this part of Newton's scale, impossible.

On the whole, then, we incline to the opinion that the number given by our experiments is the least thickness of the black in the liquids we observed. We also think that the tint our persistent films displayed is decidedly deeper than that of the less intense black shown by comparatively non-persistent films, though to make certain of this would require careful comparative observations. It is possible that the spots of deeper black in non-persistent films may be thinner than that we have measured, and the very fragility of the films in which they appear gives some colour to the supposition that it is so. It is, however, significant that, in two liquids differing so much in composition as those we employed, the one containing two parts of glycerine out of five, and the other no glycerine at all, the means of the optical and electrical measurements give results differing so little as $11{\cdot}13 \times 10^{-6}$ and $11{\cdot}9 \times 10^{-6}$ mm. It would be very interesting to settle the question by direct experiment, but the nature of the films which show the two kinds of black would make it no less difficult. We are, however, at present studying the composition of what we may perhaps call black-forming liquids in the hope of extending our investigations further, and if we can obtain one suitable for the purpose we will certainly attempt the measurement suggested by Sir William Thomson.

In conclusion we may point out two deductions from our measurements. The first refers to their connection with the subject of Sir William Thomson's lecture. If the size of the molecules of which the liquid is composed is between 2×10^{-6} and 1×10^{-8} mm. (the limits given by him), it follows that the thinnest film measured by us, which was $7{\cdot}2 \times 10^{-6}$ mm., must contain not less than three and not more than 720 molecules in its thickness. The smallness of the smaller of these numbers tends to show that the real size of the molecules is considerably below Sir W. Thomson's superior limit.

The second deduction is a good illustration of the magnitude of the stress in a liquid surface. The surface tension of Plateau's "liquide glycérique" is about fifty-seven dynes per linear centimetre (cf. "Statique des Liquides," t. i. p. 200). This force must not be considered as acting on a mathematical line, but as the resultant of forces which are in play in the thin layer of liquid which

constitutes the surface, the thickness of which is the so-called radius of molecular attraction. If the magnitude of that radius were known, the average longitudinal tension per unit of area parallel to the surface in the outer layer of liquid could be calculated. We hope before long to apply several tests as to whether the thickness of a black soap film is or is not less than twice the radius of molecular attraction. Various considerations, the discussion of which we defer, indicate that it is not much less, while if the size of an atom approaches Sir William Thomson's lower limit it is probably much greater. If, however, we assume that the thickness of the thinnest film measured by us, say $7 \cdot 2 \times 10^{-7}$ cm, was just equal to twice the radius of molecular attraction, it follows that the average stress parallel to the surface must be $2 \times 57/7 \cdot 2 \times 10^{-7} = 1 \cdot 6 \times 10^{8}$ dynes per square centimetre. This tension is eight times greater than that required to tear brick or cement asunder (cf. Everett's "Units and Physical Constants," p. 56), and one-half of that required to tear cast tin. If the radius of molecular attraction is the same for all substances, the stress in the surface of mercury in contact with air must be nearly ten times greater than in liquide glycérique, or one-fifth of the tension required to rupture steel bars. If the radius is less than half the thickness of the black films, these tensions would be greater.

In many of the ordinary calculations on capillarity the surface tension is treated as acting in a surface of infinite tenuity. In reality it acts in the matter of a liquid shell of small but definite thickness. Our experiments prove that the average magnitude of the stress in this shell is at least of the same order as that required to rupture the less tenacious metals.

A. W. REINOLD
A. W. RÜCKER

JAPANESE LEARNED SOCIETIES

NEARLY two years ago we described in NATURE a few of the principal of the scientific and learned organisations which had sprung up in recent years in Japan, in imitation of the societies of western countries. The faculty for combination and organisation would appear to be possessed in a high degree by the Japanese, for on all hands we find them establishing societies for political, self-help, philanthropic, industrial, commercial, scientific, and literary purposes. The comparative infancy of the press, and the consequent slowness and difficulty of the interchange of ideas, have rendered these organisations of great value in the social and political life of the country. The extent to which they have spread into every department of national life is well shown by a paper recently contributed by Herr P. Mayet to the *Transactions of the German Asiatic Society of Japan*, to which we are indebted for most of the facts in this article. Societies for philanthropic and political purposes, though probably more numerous and powerful than any others, are entirely omitted as beside the present purpose, which is to show how the thirst for knowledge and research is penetrating everywhere amongst this interesting people. It is important, too, to note that these societies are everywhere fostered and promoted by the leading men of the country, including most of the Imperial princes and the Ministers of State, and that they appear to be due in all cases except one to native initiation, unassisted by foreigners. The exception is the Seismological Society, which owes its existence and its excellent work to the efforts of Prof. Milne of the Engineering College of Tokio. Recently, as we learn from Herr Mayet, a Japanese section of this society has been formed, with numerous native members, papers in Japanese, and a native journal containing original as well as translated contributions. Three of the societies at present in existence have come down from ancient times. These are the Numismatic and Archæological Societies, and an association of *Go* players, similar to our own chess clubs. A society for the protection and restoration of ancient buildings, nearly all of which are naturally temples, has recently been founded, with the energetic support of the present Foreign Minister. As might have been expected, there is a society for the cultivation of Chinese literature; but the more practical spirit of Young Japan is exhibited in the association for propagating the employment of the *kana* or syllabaries in Japanese literature. The importance of the object of this society will be evident when it is mentioned that a Japanese boy of the scholarly class takes fr m five to seven years to learn the sounds of the Chinese characters, and then he has to commence to learn their meaning. Herr Mayet well observes that so long as the Japanese youth are so heavily handicapped in the race for knowledge they can hardly hope for victory against western lads, who, according to this writer's estimate, are at twelve years of age nearly six years in advance of the Japanese boy of the same age. To remove this obstacle by the employment of the system of forty-seven syllables, now in use in books intended for the common people, is the object of this society, which has for president the Vice Finance Minister. Passing over some art societies, we come to two intended for the cultivation of the French and German languages respectively. One of these is honoured by the support of an Imperial prince. The French Society is working on a French-Japanese dictionary, while both aim at the publication of translations from useful works in these languages. Those hitherto published appear to deal chiefly with political science, a study which appears to attract much of the energy and intellect of the rising generation. A Statistical Society appears also to be very successful, with its periodical publication. The Polytechnic Association has for its object the extension of knowledge with regard to mechanical inventions, and their application to the increase of production in Japan. Agricultural, dendrological, and forestry societies are also in existence, and we may specially note, as a result of the recent Fisheries Exhibition in Berlin, the establishment of a society for the study and improvement of the Japanese fisheries. Many of these associations are, it will be observed, exceedingly practical in their aims, and if the members can succeed in having their discussions and researches circulated among the people, much good will undoubtedly result. The Geographical Society of Tokio has been frequently mentioned in these columns, but there is also a Biological Society under the presidency of the native Professor of Zoology in the University of Tokio. Medical societies also are numerous, whether for purposes of study or to afford aid and relief to the indigent sick. The society for the collection and publication of books with regard to domestic industries must be of much public utility. Of a more purely scientific cast is the association for publishing a dictionary of technical terms in various departments of science and the mechanical arts. The process of finding these *termini technici* is far from an easy one. They have to be obtained from the Chinese, and have frequently, perhaps generally, to be *manufactured* by combinations of the Chinese ideographic signs, which often have but a strained or fancied resemblance to the object to be named.

In concluding his paper Herr Mayet says: "Our glance at the Japanese societies of Tokio exhibits a wealth of active ideal life and earnest endeavour. A warm patriotic pulsation is perceptible everywhere, and gives an assurance of the healthiness of the Japanese popular mind. We have here, it is true, only the beginning of association, but it promises much, and the movement will undoubtedly be a constantly growing one." After all, however, the ultimate value of any learned society is measured by the work which it has done, and we have as yet but little opportunity of applying this test to the associations of Japan.

RESEARCHES ON THE DEEP-SEA FAUNA FROM A ZOOGEOGRAPHICAL POINT OF VIEW

DURING recent years surprising and very remarkable discoveries have been the result of expeditions despatched from various countries by official and private bodies, in order to examine the zoological condition of the oceans of the globe. Thus, below the line of three hundred fathoms' depth, where biologists for many years believed with Edward Forbes that all animal life ceased, a fauna rich both in forms and individuals has been brought to light, and the theories once common enough among *savants* of a total absence of life at a certain depth in the sea have thereby in two decades suffered a complete revolution. Many objects which had previously been looked upon as biological impossibilities have been discovered, and the systematic science of zoology has been enriched with copious materials, from which hitherto unknown animal varieties have been described, recorded, and placed in their true position in the system, whereby many a gap in the zoological scale has been filled up, and science in a remarkable space of time made rapid progress. Besides this merely scientific gain, which can only be fully realised by men of science, the zoological museums have obtained valuable and fruitful treasures. The researches of the fauna of the oceans have been of double advantage, viz. as both enriching science and museums, and zoologists are delighted at both. The interest which various countries have taken in the study of the fauna of the sea has been shared between England, the United States, Sweden, Norway, and Holland, while lately even France and Italy have taken steps to assist in promoting this branch of biology, and there can be no doubt that similar researches will in the immediate future be carried on as indefatigably as heretofore.

It is my intention, with this prospect in view, to point out in these columns some methods of research in studying the fauna of the sea, which I believe will be of great advantage to science.

The manner in which the dredging of the sea is carried on from a vessel is generally this. The deposit on the bottom which the trawl or other similar appliance brings up is carefully sifted, and its animal contents placed in suitable vessels filled with spirits or other fluid for preservation. If time serves, a sorting of the various objects takes place at once, so that animals of various groups are deposited in separate vessels. These latter are either fully marked or else simply ticketed with a number, which is interpreted by an entry in the "dredging-log" kept for that purpose. The object of this log is, in the first instance, to fix exactly the spot—latitude and longitude—where the sample was taken, the time of the capture, the condition of the bottom, the depth, the temperature of the water, and if possible also the contents of salt both at the depth from which the sample was taken and at the surface. These are the annotations which have up to the present time been made by scientists when dredging. When the samples or collections thus obtained reach *terra firma* their scientific examination commences, and it becomes a matter of great moment to extract from these laboriously collected fragments a scientific whole which will be of value to zoology. The various groups of animals are consigned to different hands, *i.e.* taken in hand by specialists, the result of whose researches will naturally vary according to the lines of study they pursue. In nearly every instance the result of the same is a descriptive or anatomical work, as well as a work of the fauna; varieties and forms new to science are described, delineated, and placed in their true position in the system, while some previously known are shown to exist in places where they had hitherto been unknown. Science has thereby made a double gain, viz. a systematic and a zoogeographical. With regard to the systematic gain, it is no doubt considerable. The descriptions, with or without illustrations, may be long or short, and refer either to the exterior forms or interior construction, *i.e.* its anatomy in a limited sense; still they are invariably fruitful if they are only sufficiently complete and, what is of most consequence, methodical. Both anatomy and morphology will in most instances obtain from them what is demanded by these sciences. But on the other hand the zoogeographical gain is very unsatisfactory. What do we thus, for instance, learn from such a statement as this, that *Yoldia arctica* has been met with in lat. 73° 0′ north, and long 68° 15′ east? Nothing more nor less, in fact, than that it has been found in this particular place along with many others. By comparing, however, this locality with the others where it has previously been found, I no doubt gain a certain knowledge of its horizontal distribution, but I do not in the least degree learn from this statement the laws which govern the same. If, on the other hand, I am informed that the bottom in the place of discovery is brown sand mixed with clay, that the depth is eight fathoms, that the temperature of the water was $-2°$ C., and its specific weight 1·0273, I have at once materials for a far wider knowledge. These particulars furnish me with a basis for ascertaining the external conditions which regulate the existence of this species; and if I, besides these particulars, also learn with what animals of the same and other genus the *Yoldia* has been found in that particular place, I obtain a certain imperfect idea of the animal life existing there. I said imperfect, as, in order that the description should be complete, it is necessary I should also know the number of each species found. If I had thus information of how many individual *Yoldia* were taken in this place, and how many of the other species of animals were taken, and also if specimens of *every one* of the animals existing in this place had come up in the trawl, then I should possess an approximate knowledge of the animal fauna existing in such a place. The knowledge of the relative number of the species in a certain place is, in my opinion, a factor of essential importance to the science of zoogeography.

The example I have just quoted shows sufficiently how very incomplete the zoogeographical statements are which only record the exact place where certain species were taken. On such a basis nothing of any scientific value can be founded.

It would, I consider, be of immense value to zoology if dredgings during the larger expeditions were effected by men skilled in every branch of this science. It is clear that the more copious and varied the knowledge of the zoologist is the greater will the gain be to science on this point, especially if the student is able at the moment to take full note of what is brought to the surface. If this be the case, he would be able there and then to classify the varieties caught and particularly record the number of individuals taken, which is naturally of most consequence where it is not possible to preserve all species. Such records would be of great value to students of zoogeography, and I am under the impression that as yet no zoologist has conceived this idea, or at all events not carried it into execution.

It would undoubtedly be a matter of some difficulty, from the copiousness of the existing zoological literature, and the consequent impossibility of mastering the same, to find men who are experts in every branch of descriptive zoology, and at the same time prepared for such work as I have indicated here. While the mere mechanical act of dredging must necessarily be effected by younger men, the careful sifting of the deposit brought up is of such importance that it should only be done by a zoologist of advanced years and study; but as it seems an impossibility to combine the two conditions, the only possible way out of the difficulty is for the zoologist to preserve all

which comes up if the material shall be of any use to zoogeography.

Having indicated my views in general on this subject, I will proceed to state those cardinal points of which the zoologist should always give exact and as detailed particulars as possible, which I consider essential to the development of zoogeographical science. They are :—

1. *Place of Discovery.*—This should preferably be fixed by latitude and longitude, but, if this is not possible, by other exact means. In works describing certain sea fauna I have often found expressions as vague as these: "Bohuslän (province of Bohus)—Bergen," or "Kullen—Finmarken," "Norway and Greenland." The former of these descriptions may be satisfactory enough, if thereby is meant that the species in question are to be found between Bohuslän and Bergen, and from Kullen to Finmarken, although it would have been of more value if, even with the commonest kind, *each* place of discovery had been enumerated. It is a well-known fact that both common and rare species alike are found in smaller or larger quantities in different places, and it is information of this circumstance which it is necessary to have if a student shall be able to determine the horizontal extension of a certain species and its numerical relation to others within a certain area. With regard to the latter it implies, I suppose, that the species in question may be found along the whole coasts of Norway and Greenland, but the real meaning is, however, that they have been found somewhere, perhaps in several places, within the specified limit, and information of such vague character is to say the least of it imperfect. The physical conditions of the coasts of Greenland below the level of the sea *may* be the same from the most southern to the most northern point, but, on the other hand, it must not be forgotten that the known extent of Greenland from south to north is 23 degrees, *i.e.* 345 geographical miles, and that it is, therefore, just as likely that what applies to the development of the fauna in Davis Sound does not apply to that in Baffin's Bay; less still in Smith's Sound, not to mention that of the east coast. The extent of Norway covers 11·5 degrees, or 172 geographical miles, and the physical conditions around the coast are very variable, and as regards the fauna of the sea here it is a fact that there is a great division in the southern and the arctic element.

An exact fixing of the place of discovery has only been quite recently effected. Thus, K. Moebius's work, "Die wirbellosen Thiere der Ostsee," F. Meinerts's "Crustacea isopoda, amphipoda, et decapoda Daniæ," and A. W. Ljungman's "Förteckning öfver Spetsbergens Holothurider" leave nothing to be desired in this respect; but these cases are only exceptional, as most zoologists, whether treating the anatomy or the fauna, are satisfied with a mere enumeration of places of discovery. It is, however, true that zoological literature, as well as all other, deals with many extraneous matters, while some writers are anxious to adopt a very brief style; but in the matter of detailing the place of discovery no brevity should be observed. If zoogeography is to be something more than a mere knowledge of the horizontal distribution of the species, the places of discovery must be exactly detailed.

2. *The Depth.*—The depth at which the sample was taken should also be exactly stated as, while the place of discovery teaches us the horizontal distribution of a species, the depth indicates the vertical one. It is a well-known fact that most species are confined within certain vertical limits, which are in some instances not far apart. It certainly was to be expected that information of this nature would be found in modern works, but this is not the case. G. O. Sars' "Mollusca regionis arctica Norvegia," F. Meinerts's above-mentioned work, and O. Harger's "Report on the Marine Isopoda of New England and Adjacent Waters" are, however, remarkable exceptions to this fault. The accuracy and minuteness of these authors on the vertical distribution of the species deserve every commendation, while it must be regretted that such a work as A. Boeck's "De skandinaviske og arktiske Ampbipoder," which is undoubtedly the fruit of many years' practical study and research, gives in most cases no account whatever of the vertical distribution of a species. One attempts thus, for instance, in this work unsuccessfully to learn within what limits such a common species as the *Gammarus locusta* occurs on the Scandinavian coast. R. M. Bruzelius, in his work, "Skandinaviens ampbipoda gamonaridea" (1858), and A. Göes, in his "Crustacea amphipoda maris Spetsbergiam allnentis, &c." (1865), had both set excellent examples in the way of describing the distribution of species in the deep; still Boeck has paid no attention to this important question. He has only dealt with the synonymy, genus, and the horizontal occurrence of the species, and even as regards the latter his statements are very summary. With such statements as these. that *Diastylis Rathkei* has been found between 3 and 540 fathoms, *Idothea Sabieni* between 4 and 1215 fathoms, *Axinus flexuosus* between 3 and 450 fathoms, *Xylophaga dorsalis* between 10 and 650 fathoms, and *Caryophyllia Pourtalesi* at 100 and 980 fathoms depth respectively, it may at first sight appear a matter of little importance to state at what depth they have in each individual case been found. This is, however, one of great importance. The vertical distribution of species is variable in different seas, and it must depend on subsequent research to determine on what this variability depends. The causes may be several, and are no doubt complicated ones, as the pressure of the water, which for a long time was considered one of them, does not in any way affect their existence. The causes must be of a very different nature, and before any of them can be ascertained it is necessary to obtain exact particulars of individual instances. The following comparison of the vertical distribution of a few species in various seas may illustrate this :—

	Novaya Zemlya		Norway	
Tellina solidula appears in the Arctic waters of—	at from 4– 26	fathoms	at from 0– 10	fathoms
Cardium ciliatum	,, 2– 60	,,	,, 5– 10	,,
Cardium grönlandicum	,, 2– 20	,,	,, 5– 10	,,
Rhynchonella psittacea	,, 5– 60	,,	,, 20– 80	,,
Margarita obscura	,, 2–120	,,	,, 120–300	,,
Fusus tornatus	,, 8– 10	,,	,, 20–100	,,

The difference in five of these cases is not very great, but in one—*Margarita obscura*—it is very considerable, and even if we are unable to explain it, it should nevertheless be recorded.

3. *The Nature of the Bottom.*—This is a factor of great moment in the fauna of the sea and the division of the species. I have thus on the coast of Novaya Zemlya and in the Siberian seas personally observed that a clean sand bottom without admixture of clay is very poor in fauna, but if mixed with some clay somewhat richer, while where the clay predominates it is greatly richer. The most copious and varied is, however, that of the pure clay bottom. In shallow water on the coasts of Scandinavia the student has many opportunities of observing the variations in the copiousness of the sea fauna, both as regards the numerousness of the species and their individuals on bottoms of various natures. Possibly the nature of the bottom at greater depths, below the line where the higher orders of Algæ cease to exist, is not of such influence as above the same, but that it is in most instances of great moment to the fauna I am firmly convinced. To animals which do not live on prey the quantity of the organic elements in process of decomposition in their place of vegetation must be of consequence; to most of them the organic composition

of the bottom must be of the greatest consequence. The colour of the bottom does also, I believe, affect the existence of certain species. I therefore recommend that the nature of a bottom is not *alone* recorded, as, for instance, thus, "clay bottom," "clay mixed with sand," "stones with Algæ," and "globigerina ooze," but that a sample of the bottom is also in every case taken for future chemical analysis. It should, however, be seen that the sample is from the surface layer, and not from those below, which may of course be of a quite different nature.

4. *The Temperature and Saltness of the Water, as well as its Chemical Composition near the Bottom.*—Particulars of these circumstances should always be given exactly, as they do, no doubt, have a considerable share in the production of a species. This is so evident that it requires no further discussion.

5. *The Period of the Research.*—This is a point which zoologists in most instances fail to record, and yet it appears to me in several respects to be of great interest. Everything in life is subjected to a gradual organic change, and I believe that the fauna of the sea in this respect does not differ. Those species of animals which to-day appear within a certain locality are undoubtedly not the same which were found there, say, a hundred years ago, and still less the same as those which existed there a thousand years ago, and what applies to the past applies with equal force to the future. The struggle for existence causes the immigration of new forms, while others must, so to say, make room for the newcomers and thereby disappear. This lies in the progress of historical development. For this reason it is necessary to state the period of the research, and although science may have no immediate gain from such dates, it will no doubt come in course of time, and it is the duty of the student of zoogeography to work as much for posterity as for the present. It is with these particulars as with those of meteorological observations, viz. that one must possess a number of observations, extending over a long period, before the deductive result becomes of scientific value.

But apart from the ultimate benefit which may be derived in the future from these details, disputed perhaps by some, the record of the time when the specimens are taken is of great importance to modern science. It is thus well known that many of the inhabitants of the sea, not only those which possess perfect organs of locomotion, but also those which live a somewhat stationary life, undertake, during certain periods of the year, shorter or longer migratory wanderings. This is, however, as regards the lower Invertebrates, a circumstance which has been so little attended to, that hardly any information exists on this point in print. In connection with this peculiarity the records which I advocate would be of great use. The causes of the migratory movements may be very difficult to ascertain; but it is necessary in the first instance to demonstrate a fact—the explanation will follow in course of time. I have further indicated under (7) why I consider these statements as to time of such importance.

For the study of zoogeography in general it may be sufficient alone to know what species occur within a certain area, whether large or small. The student of zoogeography compares those species which are to be found either near or far from one another, he shows that some of them are common to all those parts which he has under consideration, that others belong to a few, and with these data before him he attempts to discover the causes of their appearance or absence in certain places. If possible, he takes the most recent palæontological phenomena into account too, he views the fauna of the present day by the light of the past, and obtains thereby remarkable and perhaps unexpected results.

6. *The Relative Plurality of the Individuals and the Colonies.*—It cannot, however, be denied that, should the mode of research indicated above be fruitful in some respects, it will not give a complete account of the animal life existing within a certain sphere. To obtain this it is necessary to know the relative plurality of the individuals and their colonies in every individual locality within the sphere. On this point my opinion is that, in order to understand correctly the composition of a fauna, it is not enough to know those species which it embraces, the zoologist must not be content with a mere enumeration either with or without descriptions of the various species and their distribution within a certain sphere, but he must also take into consideration the relative plurality of the individuals of each, *i.e.* he must, in other words, study the *statistics* of the species. It is clear that zoogeography must be based on these two propositions, as the science would not advance far, if it should, for the comparison of the fauna of two localities, rest on a mere enumeration of the species occurring in such localities. A case might certainly occur in which two localities could approximately possess the same species, while their fauna were very different in composition. To the student of zoogeography this is no improbability.

It cannot, of course, be demanded that the zoologist occupied in dredging should immediately record the number of every species the trawl brings up, as he must for this purpose possess special qualifications, but science is greatly benefited too by the course that, when the various groups of species are distributed for research and classification, the specialists in question in their works on the same record exactly the number of individuals taken of each species, and, with regard to animals forming colonies, also how many colonies were found in each place. When all the groups of the species had thus been dealt with, the student specially interested could compare the various species in every place investigated, and also the individuals and colonies in each, and by such a comparison we should obtain a really complete knowledge of the animal life in the locality investigated. If, however, no notice be taken of the relative plurality of the individuals, whether from want of study or attention to the importance of the point, the picture which the reader of his work obtains of the animal life in a certain locality will be very vague and unsatisfactory indeed.

7. *The Relation between Males and Females in the Same Place, and, if possible, at Various Times and Seasons.*—The attention of zoologists should, in connection with the study of the relative plurality of individuals and colonies, also be directed to this interesting circumstance, which of course is not related to zoogeography, but to biology. No doubt investigations of this relation would lead to valuable discoveries. In some species, with sexual difference, the males predominate, in others the females, while in some they are evenly balanced. Another point also of interest connected herewith is at what period of the season the process of fertilisation takes place, how long the pregnancy lasts, and when the females cast their eggs; whether these functions are confined to certain seasons or not. Investigations of this point show that in some cases these functions are regulated most punctually, but in others not, and consequently it would be a matter of great scientific interest to ascertain the relations of the species on this point. For this reason it is also of importance to state the exact time when the examination of a certain locality took place.

I consider that the points I have here discussed are the principal ones for which the zoologist should, in order to advance zoogeography, collect materials when making researches on the fauna of the sea. They form in my opinion the basis on which this science shall be founded for a higher and more extended knowledge, and if the researches I have here indicated are executed in a systematic manner and with due care, my belief is that zoogeography will in a short space of time reap excellent benefits and fruits therefrom; but here, as in every other branch of study, it

is necessary to work with patience and in co-operation with others, as the labour is one demanding both time and exhaustive study. ANTON STUXBERG

Gotbenburg Museum

DR. TROMHOLT'S AURORAL OBSERVATORY AT KAUTOKEINO

WE are indebted to Dr. Sophus Tromholt for the photograph from which our engraving has been made of his auroral observatory at Kautokeino in Finmarken, Norway.

The Norwegian *savant* has, as may be remembered from his communications to NATURE, during last winter sojourned in Lapland for the study of the aurora borealis, simultaneously with which observations of this remarkable phenomenon have been made at the Norwegian and Finnish Circumpolar Stations at Bossekop and Sodankylä.

Dr. Tromholt writes :—" Since September last I bave, for the sake of the aurora borealis, been residing here in North Finmarken (69° N. lat., 23° E. long.), in a zone therefore where the auroræ attain their maxima, and where the phenomena, consequently, are so frequent and on such a scale that there cannot be a question of selecting and analysing one in particular.

" My winter sojourn here has two objects in view, viz. firstly to frame a pendant to the observations of the aurora borealis made at Bossekop, 1838-39, by the French Commission du Nord under Lottin and Bravais ('Voyages en Scandinavie,' &c.), and, secondly, by means of altitudinal measurements corresponding with those being made at the Norwegian Meteorological Station at Bossekop, to procure sufficient materials for fixing the parallax of the aurora borealis. I choose the remote Kautokeino for my observatory for several reasons, viz. : that the place is almost due south of Bossekop, while the distance between the two is very nearly a degree, a distance which

Auroral Observatory at Kautokeino.

is exactly suited to the theory I have formed of the height of the aurora borealis—150 kilometres ; and also because it possesses a remarkably free horizon and an inland climate insuring favourable weather conditions.

" As previously stated, observations are made simultaneously here and at Bossekop on a common prearranged plan, and measurements made in the same vertical plane by the so-called auroral theodolite constructed by Prof. Mohn. A similar arrangement has also been made with the Finnish station at Sodankylä, which is, however, situated at a great distance from this place, and in a direction somewhat unfavourable (about 45° S.E.). We shall not of course be able to compare notes before the spring, so I am unable at present to give the final results of my observations ; but judging by own researches here I feel convinced, in spite of scientists' assertions to the contrary, that the height of the aurora borealis may be measured by the method I advocate, and that from the observations made at these three stations we shall obtain the materials required for the solution of a problem hitherto deemed an insoluble one.

" The photograph which I forward you shows the little 'scientific temple' I have raised in these lonely tracts, which have hitherto only seen Lapps and reindeer. In the centre stands my most important instrument, viz. a combination of the auroral theodolite and the passage instrument, fixed on a stone column and inclosed in a small wooden box, the upper half of which may be lowered at will. Here are, besides the necessary apparatus for meteorological observations, also to be found every requisite instrument and appliance required for my researches, such as chronometers, spectroscopes, lanterns, &c. Between them all stands the writer himself, clad in the tasteful summer costume of a Lapp, viz. pointed leather shoes, breeches twisted around the leg at the ankle, the blue frock ornamented with red and yellow borders ; and to crown it, the smart cushion-shaped cap.

" I have several times attempted to photograph the

aurora borealis, but without success. Thus, not even by using the most sensitive English 'dry' plates, and exposing them from five to seven minutes, have I obtained a trace of a negative. The cause of this is, I believe, the exceedingly limited substance of light possessed by the auroræ; were thus the entire heavens flooded by the most intense auroræ their aggregate lighting power would not equal that of the moon when full. I may therefore assume that to photograph the aurora borealis is an impossibility."

On a later occasion Dr. Tromholt informs us that he obtained no negative of the aurora borealis throughout his stay at Kautokeino, while he found also, on visiting Bossekop and Sodankylä, that neither had any been obtained at these observatories.

As to the results of Dr. Tromholt's researches on the aurora, we may add that, as soon as he has received certain comparative tables of the observations made at Sodankylä from Prof. Lemström, he will immediately communicate the same to NATURE. In the beginning of October next the intrepid *savant* starts for North Iceland, which he has chosen as his station for the coming winter. He will here chiefly experiment with the "utströmnings" apparatus invented by Prof. Lemström for producing an "artificial" aurora borealis.

We have also received from Dr. Tromholt an excellent photograph, taken by himself, of the Circumpolar Observatory which Norway, participating in the programme of international Polar research, has established at Bossekop, in North Finmarken. The station is situated on an eminence by the Alten Fjord, and the photograph shows clearly the various huts, &c., erected for meteorological, astronomical, and terrestrial observations.

ALDABRA ISLAND TORTOISES

THE following report by the Hon. W. Littleton, addressed to Sir John Pope Hennesy, Governor of Mauritius, has been forwarded by His Excellency in answer to a memorial presented by the late President of the Royal Society, and several other gentlemen, relative to the preservation of the gigantic tortoises on the Island of Aldabra :—

Memorandum on Aldabra Island Tortoises

TO HIS EXCELLENCY THE GOVERNOR,—With reference to your Excellency's request for a report on the Aldabra Island tortoises lately placed on Flat Island, I have been able to get very little information about them.

The Mauritius Acclimatisation Society recently handed over six tortoises to this Government, on condition that they should be placed on Flat Island and taken care of. The Government accepted the charge, and they were accordingly placed there about two months ago. The Storekeeper-General (Mr. Schmidt), who is much interested in them, tells me that they are completely at liberty, that they feed themselves, and are apparently doing well.

Only five of them are Aldabra tortoises; the sixth is from Madagascar. They are all young, and of comparatively small size.

But I may perhaps mention here that there are several specimens of the Aldabra tortoise, besides these, both here and in Seychelles. There is the well-known large one in the garden of the Royal Artillery mess in Port Louis. He was here before the English occupation of Mauritius in 1810. The largest circumference of his shell measures 9 feet 3 inches. He stands 2 feet 6 inches high.

In the Botanical Gardens at Pamplemousses there are two belonging to Mr. Cockburn Stewart, who brought them from Seychelles. They are about ten years old. The largest circumference of their shell is 7 feet 2 inches, and they stand 1 foot 8 inches high. Mr. Schmidt tells me of a very large one belonging to Mr. Castel, at Rivière Sèche, and of a very large pair on the estate "Mon Trésor," near Mahebourg, belonging to Mr. Daruty; but their measurements, which have been promised to me, I have not yet received.

A considerable number are kept by various people of Seychelles, including a pair at Government House, Mahé, the female of which recently laid eggs, and I am told that many of the tortoises kept on the Seychelles Islands frequently breed.

I am sorry not to have been able to collect for your Excellency's information more details of these creatures; but I have stated enough to show that there are many specimens well known and in good keeping.

I have also been unable to ascertain whether there are any of large size known to remain on Aldabra Island; but I am told that it is supposed there are in the thick scrub of the interior.

(Signed) W. LITTLETON

Colonial Secretary's Office, Port Louis,
7th July, 1883

THE METEOROLOGY OF THE ARCTIC AND SUBARCTIC PORTION OF THE ATLANTIC OCEAN[1]

UP to the publication of this work by Prof. Mohn, our knowledge of the diurnal meteorological phenomena of this important part of the ocean was nearly altogether a blank. The interesting results here detailed are deduced from three series of hourly observations made during the Norwegian Expeditions in the summers of 1876, 1877, and 1878, which Prof. Mohn organised and carried out with a skill and a completeness that leave nothing to be desired. The new facts thus brought before us largely extend our knowledge of the physics of this portion of the North Atlantic.

The diurnal phenomena dealt with are atmospheric pressure, temperature, and aqueous vapour, the force of the wind, and the temperature of the surface of the sea. Of these the discussions of the atmospheric pressure and temperature are the most important and satisfactory. The results of the atmospheric pressure present several points of the highest interest. The general curve for the three seasons, if a scarcely perceptible dip about 8-9 p.m. be neglected, shows only one minimum at 4 a.m. and one maximum at 2 p.m., thus roughly approximating to the curve of temperature. The curves for the separate seasons 1876 and 1878 exhibit an evening minimum with greater distinctness. The observations made by the *Challenger* Expedition in the Antarctic Ocean give a curve with only one minimum early in the morning and one maximum early in the afternoon; and it is highly probable that if the observations made by the Norwegian Expeditions quite in the open Atlantic were alone included, the resulting curve would give no sign of a dip in the evening.

Prof Mohn then examines the observations made at the stations on the coast of Norway at 8 a.m., 2 p.m., and 8 p.m., and it is concluded that the diurnal variation of the barometer during the summer months on the adjacent coasts of Norway, as well as in the Norwegian Sea, has its minimum in the morning and its maximum in the evening, and that possibly there is a tract in the Norwegian Sea including the bounding coasts of Norway and Greenland, thence crossing Iceland, and passing to the west and south of Faroe, where the lines of barometric variation would represent values with plus signs instead of minus signs as elsewhere. In other words, over this region there occurs a state of things the reverse of what obtains over the lower latitudes of the ocean and the land

[1] "The Norwegian North Atlantic Expeditions 1876-78. Meteorology." By H. Mohn. With 13 woodcuts and 4 plates. (Christiania, 1883.)

surfaces of the globe from the time of the morning maximum to the afternoon minimum.

The following are the four phases of the diurnal variation of the pressure in summer at a few of the more strictly insular stations :—

	1st Min. Inch.	1st Min. Time.	1st Max. Inch.	1st Max. Time.
Amsterdam	− ·013	4.30 a.m.	+ ·007	11.30 a.m.
Falmouth	− ·019	4 ,,	+ ·009	0.30 p.m.
Valentia	− ·018	4 ,,	+ ·006	0.30 ,,
Helder	− ·018	4 ,,	+ ·008	1.30 ,,
Sitka	− ·006	6 ,,	+ ·006	2.30 ,,

	2nd Min. Inch.	2nd Min. Time.	2nd Max. Inch.	2nd Max. Time.
Amsterdam	− ·004	5·30 p.m.	+ ·010	11 p.m.
Falmouth	− ·001	6 ,,	+ ·011	10 ,,
Valentia	− ·000	5 ,,	+ ·014	10 ,,
Helder	+ ·001	6 ,,	+ ·010	9.30 ,,
Sitka	+ ·000	7.30 ,,	+ ·002	11 ,,

The chief points to be noted here are the large amounts of the 1st min , and the small amounts and retardation in the times of occurrence of the 1st max. and 2nd min. All these peculiarities are presented in a still stronger form by the results of June taken by itself. Thus at Sitka the times of the four phases are 7 a.m., 3 p.m., 7.30 p m., and 11 p.m., and the 2nd min. and 2nd max. become very small. It is only, however, over the open sea in the higher latitudes where the 2nd min. and 2nd max. disappear, resulting in one minimum in the early morning and one maximum in the early afternoon. This afternoon maximum therefore really represents the a.m. max. of the lower latitudes of the ocean and of land surfaces—which phase of the pressure occurs at different hours from 7 a.m. to 3 p.m. according to latitude and geographical position—and hence subsequent to the a.m. or 1st max. the lines representing the diurnal barometric variation are everywhere to be represented by minus signs.

The highly important result remains that over the open sea in the higher latitudes of the Atlantic and the Southern Ocean the diurnal curve of pressure, as shown by the observations of the Norwegian and *Challenger* Expeditions, exhibit only one minimum and one maximum and that the curve generally resembles the curve of temperature. Hann's remark that "in the daytime the air in the upper strata above the land flows towards the sea, occasioning an increase of pressure, which even on the coast asserts itself by retarding the morning maximum and the afternoon minimum ; in the evening and at night this process is reversed, a current of air in the higher strata flows from the sea to the land ; hence the pressure increases, diminishing on the coast, and the evening maximum becomes inconsiderable," simply accounts for part of the phenomena as observed near the coast and at no great distance out a. sea. It leaves, however, the outstanding feature of the diurnal atmospheric pressure over the open seas of high latitudes untouched and unexplained. To this point we shall return on an early occasion.

The curves of the diurnal distribution of the pressure of the aqueous vapour of the air are very interesting. Grouping the three series together and bloxaming the results, we obtain a curve of great simplicity, showing one maximum and one minimum, the maximum rising 0·1 mm. above the daily mean from 11 a.m. to 3 p.m., and the minimum falling 0·1 mm. below it from 10 p.m. to 4 a.m. In other words, the curve of the force of vapour substantially agrees with the curve of temperature, and it agrees with the same curve obtained from the open sea observations of the *Challenger*. The curve for the *Challenger* observations taken near land shows a dip from about 11 a.m. to 3 p.m. which is quite decided, though not of so pronounced a character as is seen over land during the summer months. A slight dip occurs in the separate curves of the Norwegian Expeditions for 1876 and 1877, which doubtless is due to the comparative proximity to land where several of the observations were made. This reduction in the amount of the aqueous vapour which is observed to occur during the hottest hours of the day is due to the descent of the drier air of the upper regions to take the place of the air which ascends from the heated surfaces of the earth. This diminution of the aqueous vapour of the air is not restricted to the air over the heated surfaces, but, as shown by the *Challenger* and Norwegian observations, it extends for some distance out at sea, probably as far as the indraught of air from the sea towards the land heated by the noonday sun is felt.

The curve of the diurnal velocity of the wind deduced from the whole of the observations and bloxamed reveals the fact that the influence of the lower pressure which obtains over the interior of Scandinavia, as compared with that round its coasts during the hottest months of the year and the hottest hours of the day, extends much further out at sea than might have been supposed, and the curve of the force of the aqueous vapour just referred to corroborates this view. The curve of the diurnal velocity of the wind substantially agrees with that of the temperature.

The same overpowering influence of the sun is equally seen in the diurnal distribution of the temperature of the surface of the sea, the curve for which agrees with that for the temperature of the air. The curves for the separate years show, however, such striking differences in the mean amounts of the diurnal variation, and particularly in the hours of occurrence of the maximum excesses above the day means, as to show that a less close approximation to the true diurnal curve has been arrived at for the temperature of the sea than for any of the other meteorological elements. In carrying out this work and discussing the results, Prof. Mohn has clearly made a contribution of the greatest importance to the physical geography of the sea. ALEXANDER BUCHAN

VIENNA INTERNATIONAL ELECTRICAL EXHIBITION

THE Vienna Electrical Exhibition was opened to-day to the public. The patron of the Exhibition, the Crown Prince Rudolph, the Portuguese Crown Prince, the Princes of the Imperial family, the higher functionaries of the State, and the Foreign Commissioners were present at the opening ceremony. The attendance of the public was small ; only 4000 persons have visited the Exhibition to-day, the weather being rainy. The Crown Prince, in replying to the address delivered by Baron Erlanger, the president of the Exhibition Commission, said that it did not seem to be only by chance that the third and greatest Electrical Exhibition is held in Vienna—in the town in which, in 1833, lucifer matches were invented by Preschel, from which, in 1837, the stearine candle found its way through the whole world, and where the lighting of streets by gas had been suggested by the Moravian, Zinser, before it was carried out in England.

The Exhibition, though still incomplete, promises a good display illustrative of the great progress made in practical electricity during recent years, and showing how the application of electricity for the various purposes of industry and of daily life is becoming more and more common. So far as we can see now, although the work of installation of the machinery and apparatus is not yet completed, the Exhibition will stand comparison with previous exhibitions as to the number and variety of exhibits and the arrangement of the whole. From the official catalogue published to-day we learn that there are 579 exhibitors, 223 of whom are from Austria, 133 from France, 68 from Germany, 27 from Russia, 16 from Italy, 10 from Denmark, 13 from America, and 27 from England. Thus the Exhibition is rather a Continental one,

and it is generally much regretted that so few exhibits have been sent from England, which has played a leading part in the development of applied electricity.

A special feature of the Vienna Exhibition is the building itself—the Rotunda, built by Scott Russell, the eminent engineer, in 1873, covering with its annexes and courts a space of 33,000 square metres. The vast dome is 79 m. in height, and three galleries, the highest—the lantern gallery—being 66 m. above the ground, make it well adapted for illumination by electric lamps. Everything has been done to make the Exhibition as interesting and attractive as possible. Between the Rotunda and the Praterstern an electric tramway will run. The Rotunda is brought into telephonic connection with the Opera. A gallery of the Exhibition building contains a model theatre lighted by incandescent lamps, where ballets will be performed and scientific lectures given by eminent specialists, while another gallery contains beautifully arranged and furnished interiors and the picture gallery. In the machine rooms the great boilers make a gigantic impression; they will supply the various motors with 1400 horse-power to drive the electric machinery for lighting and transmission of motive power. In the nave are arranged the exhibits of different railway companies and also various scientific apparatus, of which further details will be given in subsequent communications.

Vienna, August 16

NOTES

We understand that Her Majesty's Government having through the Foreign Office been invited to appoint delegates to the International Geodetic Congress to be held at Rome in October next, at which the adoption of an international common meridian and common time for railway and telegraph purposes is to be discussed, the Lords of the Committee of Council on Education appointed a Committee to report on the subject. The Committee consisted of the Astronomer-Royal, General Cooke, C.B., R.E. (late Director-General of the Ordnance Survey), General Strachey, C.S.I., R.E. (Member of the India Council), and Col. Donnelly, R.E. (Secretary of the Science and Art Department). In consequence of their Report, the Treasury have consented to provide the travelling and personal expenses of two delegates. We are glad to say that the Science and Art Department, in concert with the Foreign Office, have appointed the Astronomer-Royal and Col. A. R. Clarke, C.B., R.E., F.R.S., to represent this country, and that they have consented to act.

The local secretaries at Southport have been exerting themselves to make the visit of the British Association a success. Excursions are arranged for Saturday the 22nd and Thursday the 27th of September. The Association has not met in Lancashire since the meeting at Liverpool in 1870 under the presidency of Prof. Huxley, and it is believed that the industries of the county have since then so developed and expanded as to open up fresh sources of interest to the chemist, the engineer, and the economist. It is believed that ample and convenient accommodation for a full meeting of the Association has been secured. The Winter Gardens have been engaged for the exclusive use of the Association, and in them will be given the Presidential Address and evening lectures, and in them will also be held the *conversazioni*. The spacious assembly room in the Cambridge Hall will be devoted to the purposes of a reception room, and suitable halls have been acquired for the use of the various sections. Numerous excursions are in process of arrangement. Among these is a visit to Stonyhurst College, the observatory, museum, library, collection of ecclesiastical vestments, and grounds, which are extremely interesting. The Abram Colliery, near Wigan, will be open to inspection, as will also the Wigan Coal and Iron Company's pits and ironworks. Messrs. Platt Brothers and Co. have offered to show a party of members of the Association over their extensive machine works at Oldham, and certain large cotton mills in the same town will be open to visitors on the same day. A geological excursion will be made to the neighbourhood of Clitheroe and the Victoria Caves, which it is hoped will be personally conducted by Mr. R. H. Tiddeman, M.A., F.G.S., who made the geological survey of the district. Another party will visit Furness Abbey and the Lake District. The Earls of Derby, Crawford and Balcarres, and Lathom, and Mr. Weld-Blundell of Ince Blundell, will throw open their grounds to members of the Association, and at some of these places garden parties will be given. There is abundant hotel accommodation of the best kind, as well as good hydropathic establishments and numerous excellent lodging houses. A list of all these has been prepared and published in pamphlet form.

The French Association began its meetings at Rouen on Thursday last, when the president, M. Frédéric Passy, gave an address on the history of political economy. The revenue of the Association during the past year amounted to 85,677 francs, of which 13,900 francs were devoted to purposes of research. The capital of the Association reaches the large sum of 454,526 francs. On Friday evening M. Hatt, hydrographer to the French navy, lectured on the transit of Venus in December, 1882, while another lecture on the transmission of force was given by Prof. Comberousse. Considerable time was devoted on Saturday in the Engineering Section to proposals for improving the navigation of the Seine. Various excursions have been made during the week, and will be continued to-morrow and following days.

Mr. Belt has been commissioned by the *employés* of the late Mr. William Spottiswoode, President of the Royal Society, to execute a monument to his memory, and the site for its erection will be in front of Her Majesty's Printing Office.

Vienna papers announce the death at Botzen, in the Tyrol, on August 10, of the Austrian Vice-Admiral, Baron von Wüllerstorf-Urbair, one of the most learned and scientific officers that the Austrian navy has ever possessed, and who has contributed greatly to its professional improvement. He was not originally intended for a naval life, and was educated in the engineering officers' school at Tulla, where he acquired a great reputation, especially on account of his mathematical talents and proficiency. But a combination of circumstances led to his being transferred to the navy at the age of eighteen. He was almost at once allowed leave of absence to continue his scientific studies at Vienna, where he pursued astronomy and meteorology under Littrow, at that time director of the Vienna Observatory. In 1839, when only twenty-four years of age, Wüllerstorf was appointed to organise the marine observatory, and from that time till 1848 he acted as director of that institution, and as professor of astronomy and navigation at the Naval Academy in Venice. In 1848 he returned to active service, becoming commodore in 1857, when he took command of the *Novara* on the celebrated expedition around the world, the first of the kind undertaken by the Austrian Government. In 1861, being then rear-admiral, he became commander at the Venice station, and in 1864, during the Danish war, he was appointed to the command of the combined Austrian and Prussian squadron in the North Sea. In 1865 he became Minister of Commerce, a position which he held till 1867, when failure of health compelled him to retire from active life at the early age of fifty-two. He was a member of the Austrian Academy of Sciences, and of many other scientific bodies. He was sixty-eight years of age when he died.

The liberality of Finland to science is exemplary. The Senate has voted a sum of about 8000*l*. for hydrographical

researches and measurements in the Gulf of Bothnia. A suitable steamer is to be purchased and fitted with the necessary appliances and instruments. We have received a communication from Prof. Lemström, in which he informs us that the Senate has also voted him a sum of 1500*l.* for the continuance of his experiments in connection with the aurora borealis during next winter at Sodankylä. In a few weeks he will forward the programme of his intended researches to NATURE.

THE arrangements for the autumn meting of the Iron and Steel Institute, to be held on the 18th, 19th, and 20th of September, at Middlesbrough, are now almost completed. An influential local committee has been formed in that town, under the chairmanship of Mr. Bolckow, and has organised a series of excursions and entertainments in honour of the Institute. The new Basic Steel Works of the North-Eastern Steel Company, and the new and very extensive works of Bolckow, Vaughan, and Company, at Eston, will be the chief works to be visited, and as they are the first works that have been established in this country for carrying on the manufacture of steel by the Basic process, it is likely that they will be examined specially by the various members. Another interesting excursion will be made to the South Durham coal district, where a new system of manufacturing coke, admitting of very considerable economy in the yield as well as in the collection and utilisation of all the by-products obtained by the distillation of coal, has been for some time successfully at work. A very good list of papers has been formed for reading and discussion, and a fund of several thousand pounds has been raised to cover the expenses of entertaining the members of the Institute.

PADRE DENZA, the Director of the Observatory at Moncalieri, expresses, in a letter to the Bishop of Ischia, the opinion, based on the information thus far obtained, that no ulterior disasters are to be feared in Ischia for the present; and especially if the forces at work under Mount Epomeo continue to find vents in the two active *fumaroli*. At the same time he adds:—"We have, however, to do with capricious and uncertain phenomena which are still a mystery to science. They are matters which require close study, and I have recommended them strongly to de Rossi's attention." Prof. de Rossi, in his second report, a brief summary of which appeared in NATURE last week, limits himself to the consideration of the many warnings that Nature gave of the catastrophe. His third report will treat directly of the phenomena connected with it. In the meantime he is emphatic in recommending to the Minister of Agriculture and Commerce the completion of that chain of observatories over all the volcanic districts of Italy, for the reception and consideration of the signs and movements noted in which the Roman Observatory was founded. Had that chain been at least more complete, and had the long-talked-of observatory in Ischia constituted a link in it, the Roman Observatory would have recognised the fact that the widely extended subterranean movement, manifested with augmentation during the ten days anterior to July 28, had its centre of greatest, most continuous, and most variously marked activity at Casamicciola, and would have given that timely warning of the approaching storm which might have saved many lives. But, he adds, there is a question as to whether such warnings should be given. The inhabitants of Albano might, for instance, have abandoned their houses in alarm, and have spent the night in the fields, had the extraordinary state of the Solfatara there been known publicly on the 28th. "To this I reply," writes Prof. de Rossi, "that the inhabitants of Casamicciola would also have spent the night in the open air, and many lives would have been saved." But it is evident, according to the *Times* correspondent, from de Rossi's first preliminary report, that there is but little enthusiasm in favour of a system of earthquake warnings, like the storm warnings sent across the Atlantic, being adopted in Italy, where in many districts the inhabitants depend chiefly on strangers for their existence. He does not hesitate to attribute to a selfish fear of frightening strangers away the opposition made to the establishment of an observatory at Casamicciola. It has now been ascertained that the signs of warning at Casamicciola were numerous, and well known to those most interested in concealing them. But the possibility of danger was ridiculed, and part of the performance in the theatre on that fatal evening was Polchinello flying from imaginary alarms of earthquake. Prof. Palmieri summarises his observations on the earthquake in Ischia as follows:—"A small or moderate earthquake causing immense disaster. The continuous wearing away of the soil by the hot subterraneous springs is sufficient to explain the immense catastrophe, which has been enhanced by the very bad construction of the houses. Some damaged by the earthquake of 1881 had remained without repairs. The disaster of July 28 will be recorded more on account of the enormous loss of life and property than of its seismographic importance."

THE Island of Ometepec in the Lake of Nicaragua has just been utterly devastated by a volcanic outbreak, causing an overflow of several lava streams which filled up several valleys and ingulfed in its fiery current farmsteads, cattle, and all the cultivated fields. The eruption began on June 19, when a new crater opened. A continuous earth-tremor resulted in an overflow of lava directed towards Las Pilas. Two days later several other hills opened, pouring out lava in every direction, and the terrified inhabitants fled. Boats were sent from the neighbouring towns to save them. The whole island is described to be at present a heaving mass of molten lava, quite uninhabitable.

A SHOCK of earthquake sufficiently strong to move beds and displace crockery occurred last Thursday at Schuols, Pontresina, and Tarasp, in the Engadine. The shock was preceded by a violent storm and heralded by a peal of subterranean thunder.

A STATUE of Daguerre will be unveiled at his native village of Cormeilles on Sunday.

THE recent inquiry in the United States Patent Office concerning the invention of the telephone has had the following results:—Out of eleven interference cases, eight of them have been decided in Bell's favour, two in Edison's, and one in MacDonough's. MacDonough's award was for the invention of a "telephonic receiver," consisting of the combination of an electric current with a magnet and a diaphragm arranged close to the magnet so as to reproduce accurately the sounds as regards quality and pitch. Edison's awards are (1) for a "hydroelectric telephone"; (2) "for a spring carrying one electrode and constantly pressing against the other electrode, and the diaphragm to maintain the required initial pressure between the electrode and yield to the movements of the diaphragm." The most remarkable of Bell's awards is the art of transmitting and reproducing sounds at a distance by means of an undulating electric current. The remaining awards of Bell's consist of various forms of transmitters.

A NEW galvanometer has been brought out by M. Ducretet, which contains the valuable properties of being dead beat and being used for both strong and high potential currents. Its chief points consist in a movable compound coil, the fine wire coil being near 6000 ohms, and the framework of this coil, which consists of a copper ring, being the low resistance coil. The magnetometer part consists of a box with a very delicately balanced needle immersed in some transparent liquid. The needle is very small, and has attached to it a fine aluminium pointer by which the readings are made. The galvanometer can be used for all strengths of current in practical use.

AMONGST the candidates who have offered themselves to fill the place in the Academy of Sciences vacated by the recent

death of M. Lagournerie, we may note M. Bischoffsheim and Col. Laussedat, Director of the Conservatoire des Arts et Métiers.

M. DE FONVIELLE, writing from Annonay, informs us that at the unveiling of the Montgolfier statue Col. Perier, who was the official representative of the Government of the Republic, was in the chair, and spoke in praise of Montgolfier in the name of the French army. M. Dupuy de Lome delivered a written address, extending over two hours, being a detailed *procès-verbal* of our actual knowledge of aëronautics. Like other speeches delivered on the occasion it will be printed in full in the *Journal Officiel*. M. Tisserand, the astronomer, spoke during a very few minutes, admitting that it would be possible to see celestial bodies better if the observer were carried away from the earth nearer to the limits of the atmosphere. The effect of the statue, which has been cast in bronze, is very happy, M. Cordier having represented the Montgolfiers in the act of inflating a Montgolfière—Joseph is presenting the object to his brother Stephan, who on his knees has in his hand a bundle of burning straw and presents it under the hole. In the evening a banquet took place at the Hôtel de Ville, Col. Perier being in the chair.

THE last number (the 28th) of the *Mittheilungen der deutschen Gesellschaft für Natur und Völkerkunde Ostasiens* contains the first instalment of a paper by Dr. Baels, of Tokio, on the "Physical Characteristics of the Japanese." The writer refers to the extraordinary contradictions on this subject in the ordinary works on Japan. Thus Miss Bird says of the Japanese: "Their physique is wretched, leanness without muscle being the general rule;" while Consul General Van Buren speaks of them in his reports to the Department of State at Washington as "a race of people of good physique, of stalwart and well-proportioned frames." And so with other writers. This is the more surprising that life in Japan is very public, and the opportunities for accurate observation are accordingly very numerous. In fact, Dr. Baels says, a study of the literature on the subject shows that we know nothing certain about the physical qualities of this people. This is probably to be attributed to the fact that detailed and accurate observations and anthropometrical measurements have not been made; and this defect Dr. Baels's position as professor and surveyor in the principal Japanese hospital gave him ample opportunity for supplying. Accordingly we find that his conclusions are supported by large numbers of statistics. In some cases 1200 persons were measured, and as a rule at least 100 measurements and observations were taken. The whole paper is divided into two sections, the anatomical and physiological, of which we have only the first in the present number. In examining the interesting question as to the origin and position of the Japanese race, the author finds himself confronted with the most perplexing and contradictory assertions respecting the Ainos. In two columns, side by side, he places the statements of two countrymen of his own—Drs. Dœnitz and Scheube—as to the Aino characteristics, with the result that one is in flat contradiction to the other, in such apparently simple matters as the hair, its growth and quality, the shape of the nose, &c. After a long examination of the authorities, however, he comes to the following conclusions:—(1) The Ainos were the original inhabitants of Central and Northern Japan, and their influence on the modern Japanese race is small; (2) a Mongoloid tribe, similar to the better class of the Chinese and Coreans, emigrated from the mainland through Corea, and settled the south-western part of the main island, and from thence spread themselves over that island; (3) another Mongol tribe, bearing a resemblance to the Malays, first settled in the south in Kiushiu, and gradually conquered the whole country. This stem is represented now in its purest form in Satsuma, and gave Japan its Imperial House. It also forms the large mass of the Japanese of to day. He further surmises that the second factor, namely those Mongols with the fine features, came from far to the south and west, and were perhaps related to the Akkadians; but he finds no direct connection whatever between the Japanese and any Semitic race. The remainder of the paper is occupied with statistics respecting the size and proportion of the body and its single parts.

WE have received from the director of the Meteorological Observatory of Tokio some of the daily weather maps issued by that institution. The observatory, which is attached to the geographical bureau of the Home Department, is not new, although it was not till 1881 that a plan for a telegraphic weather service was suggested to the Government. From July 1, 1882, the introduction of millimetres and degrees Centigrade, and of three equidistant, simultaneous meteorological observations at 6 a.m., 2 p.m., and 10 p.m. Kioto time were sanctioned. Twenty-two stations were established from Kagoshima and Nagasaki in the south to Hakodate and Sapporo in the north. Each morning one telegram recording the observations of the previous day is despatched to Tokio, and appears in three weather maps. These latter are both in English and Japanese, and are printed with great clearness. It is intended, as soon as sufficient experience has been acquired, to supplement the reports and maps by the issue of warnings and indications.

THOSE interested in the origin and history of the telephone should read Prof. Silvanus P. Thompson's "Philipp Reiss, Inventor of the Telephone," a biographical sketch, with documentary testimony, translations of the original papers of the inventor, and contemporary publications. E. and F. N. Spon are the publishers.

WE have received the last volume of the *Memoirs of the Kieff Society of Naturalists* (vol. vii.), which contains, besides the proceedings and a note on chemical analyses of the Kieff clays, an elaborate memoir by M. Armashevsky, on the geology of the province of Chernigoff, with a geological map of the province. It is covered with Upper Chalk, more than a hundred feet thick, quite like that of the neighbouring provinces, and the fossils of which prove to be intermediate between the Senonian and Turonian. The chalk appears, however, only in the deeper excavations of the Desna and Sudost Rivers, as it is covered with a thick sheet of Tertiary deposits, which are found throughout the north-eastern part of the province, disappearing towards the south-west under the boulder clay. The Tertiary consists of two parts—the Glauconite sands and sandstones, and the quartz sands with intermediate beds of sandstones. It is most varied in colour and composition, and contains phosphorite, caolin, brown-coal, and boulders of chalk. It is a part of the immense Tertiary basin that covers Southern Russia from Kherson and Kieff to Saratoff and Simbirsk, and bears the characters of shallow-sea deposits, with banks of oysters. The fossils discovered by M. Armashevsky leave no doubt as to its belonging to the Lower Eocene. It is covered in its turn with Post-Pliocene pottery clays, and these last with an immense sheet of boulder clay, which is widely spread throughout the province, as well as throughout the whole of middle Russia. It is an unstratified and unwashed mixture of clay, sand, and boulders, partly of Scandinavian origin and partly brought from all those formations that are met with to the north of Chernigoff; that is, Silurian, Devonian, and Carboniferous. Huge masses of chalk and Cretaceous sands are also met with in it. The boulders reach sometimes the size of ten feet, and are sometimes polished and striated—the local ones as well as those brought from the north. The author, who is well acquainted with the recent literature of the subject, and especially with the numerous researches of German glacialists, does not hesitate to recognise that the province of Chernigoff, as far as Kieff, was covered by the ice-sheet that extended throughout

what is now middle Germany and middle Russia, the continental and glacial origin of the boulder clay being beyond doubt. His remarks on the extension and mode of formation of the loess, which appears with its characteristic features in the ravined parts of the province along the valleys of the larger rivers, are also worthy of notice. It has a continental origin, but rather aqueous than atmospheric.

A FISHERIES EXHIBITION will be held in Lysekil in Sweden early next month.

LAST week at Coblenz experiments were made with young ravens with a view of replacing carrier-pigeons by them. The ravens are not so subject to being attacked and destroyed by birds of prey. The ravens were sent from Coblenz to a small place on the Moselle near Treves, a distance of about forty miles. The experiments proved eminently successful.

THE additions to the Zoological Society's Gardens during the past week include two Silver-backed Foxes (*Canis chama* ♂ ♀) from South Africa, presented by Mr. John Maydon; a Syrian Bear (*Ursus syriacus*) from Thibet, presented by Mr. A. W. Hicks Beach; two Red-backed Shrikes (*Lanius collurio*), British, presented by Mr. D. Bowl; a Sparrow Hawk (*Accipiter nisus*), British, presented by Mr. F. Gunn; two Spotted Salamanders (*Salamandra maculosa*), European, presented by Miss Harris; two Russ's Weaver Birds (*Quelea russi*) from West Africa, three Java Sparrows (*Padda oryzivora*) from Java, two Saffron Finches (*Sycalis flaveola*) from Brazil, two Undulated Grass Parakeets (*Melopsittacus undulatus*) from Australia, a Gray-headed Love Bird (*Agapornis cana*) from Madagascar, a Goldfinch (*Carduelis elegans*), two Bullfinches (*Pyrrhula europæa*), a Chaffinch (*Fringilla cælebs*), a Lesser Redpole (*Linota rufescens*), a Siskin (*Chrysomitris spinus*), British, an Indian Python (*Python molurus*) from India, deposited; five Blue-headed Pigeons (*Starnœnas cyanocephalus*) from Cuba, purchased; a Quebec Marmot (*Arctomys monax*), two Gray Squirrels (*Sciurus cinereus*) from North America, a Plantain Squirrel (*Sciurus plantani* from Java, received in exchange; five Common Vipers (*Vipera berus*), born in the Gardens.

OUR ASTRONOMICAL COLUMN

VARIABLE STARS.—The following are Greenwich mean times of geocentric minima of *Algol* to the end of the present year, which fall between about 6h. and 15h.; advantage has been taken of the recent observations of Dr. Julius Schmidt, at Athens, in their calculation:—

		h. m.			h. m.
1883, Sept.	3	12 56·5	1883, Nov.	5	14 46·9
	6	9 45·0		8	11 35·8
	9	6 33·6		11	8 24·6
	23	14 36·5		28	13 18·2
	26	11 25·1	Dec.	1	10 7·2
	29	8 13·6		4	6 56·2
Oct.	16	13 5·6		18	15 1·5
	19	9 54·3		21	11 50·6
	22	6 43·0		24	8 39·7
				27	5 28·9

A geocentric minimum of U Cephei, Ceraski's short-period variable, falls about Dec. 1, 17h. 27m., and this phase takes place earlier in the night, through the winter. On February 19 the calculated time is 11h. 58m. We assume two periods of this star to occupy 4·98559d.

Mr. Chandler having found that the period of Sawyer's variable in Ophiuchus is only 20h. 7m. 41·6s., this object goes through its fluctuations in a shorter time than any other known variable, R Muscæ following next, according to Dr. Gould, with a period of about 21h. 20m. The variation of light of the former star is stated to be about three fourths of a magnitude. It is Lalande 31384, Weisse XVII. 143, and Santini + 2°, 200. Argelander and Heis call it 6m. The mean place for 1883·0 is in R.A. 17h. 10m. 35·7s., Decl. + 1° 20′ 32″.

The observations of Dr. Schmidt and Mr. Sawyer show that a maximum of χ Cygni occurred on September 2·5, 1882, and the mean period during the last six or seven years having been about 408 days, another maximum may be expected about October 16. The best position of this variable will be that given by a mean of Argelander's places in vol. vi. of the Bonn observations, viz. for

1855·0 ... R.A. 19h. 44m. 59·66s. ... Decl. + 32° 32′ 59″·4.

With Peters' constants we find—

Precession in R.A. 2·3065s. Secular variation + 0·0013s.
,, Decl. + 8″·870. ,, + 0″·298

Whence for the beginning of the year 1884 the position becomes R.A. 19h. 46m. 6·5s. Decl. + 32° 37′ 15″.

The confusion that has taken place as to the identification of the true variable χ Cygni is almost ludicrous. Flamsteed attached Bayer's letter to his 17 Cygni, being misled, as Argelander has shown, by the variable star being faint when he observed. In 1816 Olbers referred, in Lindenau's *Zeitschrift für Astronomie*, ii. 185, to the misunderstandings and complications that had taken place through Flamsteed's mistake, pointing out that Pigott first gave the correct position of Bayer's χ; it was soon after determined by Koch, and was observed by Lalande in his zone of August 13, 1793. Further, in 1818, Bessel in the *Fundamenta Astronomiæ*, in a note to 17 Cygni, wrote, "*Flamsteedius* hanc stellam per χ designat; sed stella a *Bayero* ita dicta alia est neque reperitur in catalogo." Notwithstanding these rectifications, Baily, in the British Association Catalogue, falls into Flamsteed's error, calling No. 6784, 17 χ Cygni, and to this circumstance is perhaps to be attributed the confusion in recent popular English treatises as to the identification of Kirch's variable. 17 Cygni is a double star (Σ 2580), without any claim to the letter χ; Bayer's χ is Kirch's variable, and totally distinct from Flamsteed's 17.

A minimum of R Leporis may be expected about December 14; Mr. Sawyer found the star at a maximum about January 25, 1882.

THE GREAT RED SPOT UPON JUPITER'S DISK.—Prof. A. Riccò, of the Observatory at Palermo, in a communication to the *Memorie della Società degli Spettroscopisti Italiani*, gives interesting details of his observations on the features of Jupiter's disk, during the last opposition. The red spot had become very faint, indeed barely distinguishable in April and May, and was *invisible* at the commencement of June. Mr. Marth, in his "Ephemeris for Physical Observations of Jupiter" for the approaching opposition, has retained the same daily rate of rotation adopted in the ephemerides for the last two oppositions, remarking that even if it should be found that the great reddish spot has entirely faded away, it is still desirable that its place should be specially watched, and hence it has not been advisable to make any alteration in the data for the ephemeris at present.

THE MINOR PLANET, NO. 234.—Prof. Peters notifies his discovery of a new minor planet on August 12, and strange to say he estimates it as bright as the ninth magnitude. Its place at 18h. 51m. Greenwich M.T. was in R.A. 21h. 20m. 50s., Decl. − 12° 29′.

No. 175 *Andromache*, to which reference was lately made in this column, has so far escaped observation, though carefully sought for at Rome.

GEOGRAPHICAL NOTES

PROF. ALPHONSE MILNE-EDWARDS, chief of the French deep-sea expedition in the *Talisman*, writes from St. Vincent, Cape Verde Islands, under date July 28, that the expedition had met with complete success. After having investigated the deep-sea fauna of the African coast to a distance of some leagues from Dakar, the expedition proceeded to Santiago and St. Vincent, sounding all the way. The island of Branco, where no naturalist had ever been, was investigated, the great lizards of the island receiving special attention in their native habitat. The coast is so rocky the naturalists had to swim ashore. The island is extremely volcanic, with scarcely any vegetation, although the lizards are herbivorous. The *Talisman* was about to proceed on the last section of her voyage, the investigation of the Sargasso Sea.

THE Austrian corvette *Pola* arrived at Hamburg on the 19th from Jan Mayen. The Austrians, who were entertained at a banquet by the Geographical Society of Hamburg, have brought home a large quantity of natural history specimens and photographs, and express themselves highly pleased with the results of their expedition.

THE northernmost of the international meteorological stations round the Pole is that of the United States, in command of Lieut. Greely. It is situated in 81¾° N. lat., close to where Nares wintered, on the coast of Smith's Sound in Lady Franklin Bay. Since 1881, when the expedition took up its quarters, no news of any kind has arrived, as the vessel despatched in order to communicate with the same last summer could not get up for ice. This summer a strong attempt to relieve the party will be made, for which purpose the steamer *Proteus* has just left Franklin Bay accompanied by the U.S. war vessel *Yantic*. Should, however, the condition of the ice also this summer be unfavourable, the relief expedition will be put ashore at a certain point on the east coast of Smith's Sound, and the *Proteus* will return. By the aid of Eskimo the expedition will attempt during the winter to relieve Lieut. Greely and his comrades, who have instructions to depart from their station if not relieved in the autumn. Depots with 1200 rations at each will be established along the route, and as Greely is provisioned up to the summer of 1884, there is no fear of his safety. During next summer a vessel will be despatched from the United States to bring home both expeditions, which will by then, no doubt, be found safe in some spot on the east coast of Smith's Sound.

HEFT VIII. of *Petermann's geographische Mittheilungen* contains a long and interesting report by Dr. W. Junker, dated May 1881, from the country of the A-Madi, in the region of the Upper Nile. In consequence of insuperable difficulties connected with the transport of his luggage, Dr. Junker was unable to reach Bakangai, the destination he had proposed for himself, and had to return northwards after crossing to the south bank of the Welle-Makua, in the country of the A-Barambos, to the south of the district of Bahr-el-Ghasal. The greatest difficulties travellers have to contend with is the carriage of their luggage, the natives to the south of that country, including the subjects of Ndoruma, the people of the largest part of the Niamniam region, and all further south being almost quite unavailable for that service. Expeditions sent south from Bahr-el-Ghasal in quest of ivory have, therefore, to take their own porters with them. The travellers Schweinfurth and Miani have generally been under the necessity of attaching themselves to such expeditions, as has also Dr. Junker in all his more extensive travels, though the disadvantages and in particular the delays connected with this mode of travelling are very great. From Palembata, where during ten days he had to live exclusively on sweet batates, Dr. Junker, crossing the watershed which divides the tributaries of the Werre in the north, from those of the Welle-Makua in the south, came, after two days' march, into the land of the A-Madi, a mountainous district, watered by a number of streams diffusing a constant moisture over the gentle declivities of their banks, and nourishing vigorous growths of bananas and oil-palms. Dr. Junker stayed with the Prince Masinde for several days, during which he made an excursion to a group of mountains immediately to the south-south east, ascending the highest peak, Mount Malingde, whence he had a view of three almost equidistant but topographically very diverse points of the Welle in its sweep from the west to the direct north. The A-Madi are described as a race largely resembling the neighbouring tribes in manners and customs, but whose speech shows not the least affinity to any one of the many languages of the wide surrounding region known to Dr. Junker. In structure they resemble the muscular and shorter figure of the A-Sandeh. They are brachycephalous, of medium stature, far below that of the tall Dinka, Nuehr, or even the Bongo. The A-Madi tattoo their breasts according to the most diverse patterns, though the face is generally left intact, with the exception of nose and ears. In the working of iron they are far behind the Mangbattu. The fruit of the banana is used at all its different stages as the principal and sometimes the exclusive food of the people. Letters of Dr. Junker to Dr. Emin Bey, extending in date from Jangasi, in the former Munsa's district, now Niangara's, July 17, 1882, to a provisional station in the land of Semio, November 8, 1882, give us the latest details regarding his stay in Mangbattu, and his plans for the future.

BARON MÜLLER, during his travels in the winter of 1881–82 through Eastern Soudan, was shown some new maps executed by the Egyptian staff, under the direction of Reschid Pasha, and gives an account of them in the present number of the *Mittheilungen*. Reschid Pasha was induced to undertake this work in consequence of the want of maps, available for military purposes, of the country on the borders of Egypt and Abyssinia. The survey of the triangular district defined by the three points, Massowah, Cassala, and Gallabat, was, according to Herr Müller's information, entered upo simultaneously by various surveying parties in 1875. No scientifically accurate set of maps, to be achieved with all the aid of theodolites, astronomical determination of places, and hypsobarometrical measurements, was aimed at, but only such a general plan as would satisfy military requirements. All the maps executed in this way, on the scale generally of 1 : 1,000,000, did not reach Herr Müller's hands, but only those representing (1) Annesley Bay, (2) Gebel Gadam, (3) the caravan road from Massowah *viâ* M'Kullu and Ain, (4) the descent of the land at Samharr from Debra-Bizen as far as Ain, including Sabba Guma, Ailet, the Motad Valley, Asus, and Gumhot, (5) Mensa, extending as far as the Northern Hamsen, Dembesan, and Karmeschim. The map of this country is altogether excellent. Particularly well given is the Bogos country, including the Rora Az-Geret with Zad-Amba, Atirba, and the Boggu Valley, as also Halhal and the district of the Red Marea. These maps, due to the admirable energy of Reschid Pasha, though at present studiously concealed from Europeans, and Englishmen especially, must, in Herr Müller's opinion, before long enable people generally to obtain a distinct idea of that most interesting group of plateaus to the north of Abyssinia.—Among other papers in the same number, Dr. H. Polakowsky gives, as a contribution to the geography and ethnography of Central America, a report of an expedition undertaken by the Bishop of Costa Rica (B. A. Thiel, a German by birth), in company with Lieut. L. Fernandez and D. José Ma. Figueroa, to the wild Indian tribes, the Chirippo Indians, of that Republic.—In a letter to Dr. Emin Bey, Lupton Bey, the Governor of Bahr-el-Ghasal, reports an important discovery made by him in the last months of 1882 in the course of travels in the district of the Kredj tribes—the discovery, namely, of a large river of the name of Parpi. Rising in the mountains to the south-west of Hofra-el-Nahass, it runs south through very fertile lands and receives many tributaries, among others the Wille (marked on Schweinfurth's map to the west of Dem Bekir).—The *Mittheilungen* further report a botanical collection made by G. Ruhner of the Berlin Museum, at Bengasi, a collection which, added to that of Schweinfurth, will materially increase our knowledge of the vegetation of Barka.

IT is announced that Dr. Emil Riebeck, who is well known in the geographical world for his successful travels and magnificent collections, is at present engaged in making the arrangements for an undertaking which promises to be of the greatest importance in the history of the exploration of Africa. The expedition is to be carried into execution by Herr Gottlob Adolf Krause, who is at present in Milan, and the immediate object is described as the investigation of the languages and social state of the inhabitants of the region about the Niger, Benue, and Lake Tsad. Herr Krause intends to follow the Niger from its mouth upwards for a distance of about 300 miles, and then probably to take up his position in some suitable spot, whence he can make a general survey of the surrounding country, decide on his further course of action, and await a favourable opportunity for an advance into the interior. He intends to make his first stop either at Ripo Hill, by Egga, an English mission station, or to choose Shonga, near Rabba.

ACCORDING to intelligence received at Copenhagen, August 18, from St. Petersburg, the Imperial Russian Geographical Society has informed the Danish Minister to Russia that a report is current among the Samoyede inhabitants of the Island of Waigatz that a foreign vessel has wintered on the eastern coast of that island. It was, however, at the same time pointed out that there was nothing to show that the ship in question was the missing Danish vessel *Dijmphna*, which started last year on a voyage of discovery to the North Pole.

THE *Vega*, the famous exploring vessel, returned at the end of last month to Norway from seal-hunting in the Arctic seas with 8750 seals on board.

M. LÉON POIRIER has left to the Geographical Society of Paris one-third of his fortune, the interest on which is to be devoted once every three years to granting an annuity to the Frenchman who shall have most distinguished himself by his travels in the interests of science and commerce.

THE EDISON-HOPKINSON DYNAMO-ELECTRIC MACHINE

THE following abstract of the report by Mr. Frank S. Sprague on the Edison-Hopkinson dynamo-electric machine will be found of interest to electrical engineers :—

Characteristic features of the dynamo are : General arrangements—those of a shortened and differently proportioned Edison dynamo. The pulley, however, is out-side of bearing, and with a face of $6\frac{5}{8}$ inches and diameter of $10\frac{1}{16}$ inches projects $8\frac{1}{4}$ inches outside the base plate. Field coils wound over a 9-inch core with ten layers of No. 16 copper wire (B.W.G.). Two legs in series. Armature : Diameter of core 9 inches, 74 coils, single turn, 8 strands of No. 16 wire, average length 43 inches. Wire bound. Diameter $10\frac{1}{2}$ inches, with $\frac{1}{16}$ inch clearance from pole faces. Zinc plate connecting pole faces ; ends of magnets not scraped. Resistances : Field cold, 36·5 ohms ; armature cold, ·026 ohms. Field measured ; armature calculated. Field warm, 37 ohms ; armature warm, ·0325 ohms. Power supplied from a Lawrence —Armington and Sims—engine, high-speed and non-condensing, driven by a link belt through an Alteneck tension belt dynamometer.

Engine diameter	$8\frac{1}{2}$ inches	accepted.
Stroke	$9\frac{15}{16}$ inches	measured.
Piston-rod	$1\frac{3}{8}$ inch	,,
Fly-wheel	40 inches	,,
Indicator spring	56 inches	,,

$$\text{I-H.P.} = \frac{2\,.\,\text{P. L. A. revs.}}{33{,}000}$$

$$= \frac{2\,.\,\frac{\text{P. } 9\frac{15}{16}}{12}\,.\,\frac{\pi}{2}\,(2{\cdot}425^2 - \frac{11}{16}^2)\text{. revs.}}{33{\cdot}000}$$

$$= \text{Mean pressure} \times \text{rev.} \times {\cdot}0028107.$$

The magnets were tested by the Poggendorff method.

$$\text{Total H magnetic field} = \text{Gr} \times \text{E} \times \frac{\text{position}}{\text{resis.} \times \text{dif.}}$$

$$\text{E} = 1{\cdot}457 \text{ Clarke's standard}$$
$$\text{Gr} = 6428$$

Mean force in laboratory—Westminster :—

	Earth.	No. 45.	No. 17.
May 7	·121	9·41	11·40
May 8	·122	9·46	11·45
May 12	·122	9·40	11·52

Total H field for Manchester :—

E and No. 45	9·55
E and No. 17	11·61

The results of three fairly full loads are given. No. 6 Time, about one hour ; load, 192 lamps and ground of about 5 amperes. Lamps not up to candle power.

Potential galvanometer, magnet ...	No. 45	
,, ,, position ...	2	
,, ,, strength ...	9·55	
Average deflection	20·79	
Potential at brushes	99·27	volts.
Current galvanometer, magnets ...	No. 17	
,, ,, position ...	2	
,, ,, strength ...	11·61	
Average deflection	19·39	
Current in lamp circuit	112·56	amp.
,, field	2·68	,,
,, armature	115·24	,,
Resistance, lamp circuit	·882	ohms.
and field...	·861	,,
Total resistance of circuit	·8935	,,
E.M.F.	102·97	volts.
Electrical energy in lamp circuit ...	14·97	H.P.
,, ,, field	·30	
,, ,, armature ...	·58	
Total	15·91	H.P.
Dynamometer spring at rest	112·23	lb.
,, ,, running free ...	115·00	,,
,, ,, load	180·11	,,
Total difference	67·88	,,
Above friction	65·11	,,
Total power to armature	17·34	
Power above friction	16·63	
Friction	·71	

Dynamo speed, 1081 ; engine speed, 289·3 ; efficiency of conversion, 97·7 per cent. ; commercial efficiency, 86·3 per cent.

Dynamo behaved well. Fields cold. Armature moderately warm. Wrist not uncomfortable on coils. Can also be held on commutator. Little sparking.[1] Bearings cool. No increased heating after standing.

The same remarks about the behaviour of the dynamo are pertinent to two later experiments with 192 and 230 lamps respectively. There was no appreciable increase in the heating, and the load could easily have been carried a long while. An increased load of 30 lamps could be carried some time.

[1] Some lateral play of armature and spindle.

Summary of Three Experiments

No.	Time.	Speed.		Current in amperes.			E.M.F. in volts.		Electric H.P. appearing.				H.P. delivered to pulley.		Efficiency.	
		Engine.	Dynamo.	Field.	Lamp circuit.	Total.	Brushes.	Total.	Field.	Armature.	Lamp circuit.	Total.	Absorbed.	Total.	Conversion.	Commercial.
6	1 hour.	289	1081	2·68	112·56	115·24	99·3	103·0	·36	·58	14·97	15·91	16·63	17·34	95·7	86
8	31 min.	309	1157	2·92	123·07	125·99	108·	112·1	·42	·69	17·81	18·92	20·12	20·88	94·0	85
9	1h. 1m	$315\frac{1}{2}$	1179	2·95	144·6	147·55	109·3	114·1	·43	95	21·18	22·56	23·79	24·56	94·8	86

Means : 94·8 % 86 %

INDIAN METEOROLOGY

I.

EXPERIENCE only confirms what a cursory glance would have led us to anticipate from theoretical and *a priori* considerations—that meteorology, the most modern as well as one of the most ancient of all the sciences, requires to be studied on the largest possible scale. The synoptic charts of the late General Meyer in America, of Hoffmeyer in Germany, and our own Meteorological Office, have graphically and forcibly set before us the variety and complexity of conditions that occur in a horizontal direction, while the observations of balloonists and mountain travellers have equally illustrated the important difference and often complete opposition which exists between the physics of the upper and lower aërial strata. Indeed it may be affirmed of meteorology, with even more truth than of the analogous science of geology, that it recognises neither political nor superficially physical divisions of the land. When, therefore, we confine our attention to the atmospheric conditions of one small political division of the earth's surface and attempt to educe from data collected within that region alone the laws which regulate them, we are in a far worse position—especially if we take the British Islands as our example—than if we essayed to construct the science of geology by a like process, since in the latter case, if our horizontal range is limited, these islands form an almost complete and unique epitome of geological stratigraphy. In the case of meteorology, however, it is far otherwise, since our area is not only microscopic in relation to the scale on which meteorological changes occur, but is situated in a peculiarly unfavourable position for studying those changes with success.

In the matter of vertical range we are no less badly off, our loftiest elevation being less than one-fourth of that attainable in some countries.

Fortunately for us, however, we have a dependency which offers rare facilities for the study, not merely of climate and weather, but of what is acknowledged to be the "highest branch of meteorology," viz. *atmospheric physics.*[1]

India has, in fact, been often specially alluded to by leading meteorologists as a golden field for this line of research, and Mr. Blanford has with evident pride ventured to predict that, "given a few earnest and intelligent workers, this country [India] will one day play a part second to none in the advancement of rational meteorology."[2]

The characteristics presented by India, and which have been specially noticed by Blanford, Buchan, and others, are (1) its great size—more than fifteen times that of what we are pleased to call *Great* Britain; (2) its proximity to the equator; (3) the seclusion of its area by the Himalaya on the north; and (4) the physiographical contrasts it presents. If anything further were needed to show the desirability of investigating the meteorology of India, it would be the fact noticed by Prof. Eliot in his "Report" for 1877 (p. 48), that while in Europe the changes of weather take place mainly in a horizontal direction, the homogeneity of those in India over large areas shows that they are rather the result of vertical (expansive and contractive) actions, from which it follows as a necessary corollary that if the dynamics of the atmosphere are ever to be solved, we must combine the facts obtained from regions of vertical with those from regions of horizontal motion, and, as Eliot says, "the two sets of facts must be regarded, not as opposed to, but as *supplementing each other.*" Some idea of the work that is being done and the area it represents may be gathered from the fact that according to the "Report on the Meteorology of India for 1880" there are now 121 stations in the Indian area (including Ceylon and Burmah) where meteorological observations are regularly made, together with 385 raingauge stations, representing in all an area of 1,131,000 square miles.

This work finds an official outlet in the excellent "Annual Reports" published by Blanford, as well as the valuable monographs on the "Meteorology of Bombay," by Mr. Charles Chambers, F.R.S., and those on the "Bay of Bengal Cyclones," by Prof. J. Eliot, and it will, we venture to think, be admitted by all who have carefully examined these works that they not only reflect great credit on the ability of the writers, but go some considerable way towards indorsing Mr. Blanford's prediction.

Besides these strictly official works, there are published a series of papers entitled the "Indian Meteorological Memoirs," which are intended, according to the preface by Mr. Blanford, as "a vehicle for the publication of such portions of the work of the officers of the Indian Meteorological Department as do not form part of the regular Annual Reports on the Meteorology of India."

In the present articles we purpose noticing briefly the first complete volume of these. Before doing so, however, we may observe that their quality is uniformly of a remarkably high order. We know of nothing approaching to them in this country in meteorology, except, perhaps, occasional papers in the *Transactions and Proceedings of the Royal Society*, or a few publications of the Meteorological Council; and we have to go abroad, to the *Repertorium* of Russia, or the *Zeitschrift* of Austria, before we can find papers of equal calibre. This defect is unfortunately more readily explained than remedied. In this country our best men, for reasons which are many of them obvious, and which we need not dwell upon, devote themselves to almost any other science but meteorology. The consequence is that little is done, and that little often indifferently, thus in some measure justifying the scorn which many physicists openly entertain for the science and all its disciples. Before, however, these gentlemen pledge themselves to their verdict, let them look abroad to America, Russia, and India, where more interest is taken in the science, and where the field of operations is vastly more extensive, and the conditions more favourable, and we suspect they will be inclined to modify their views somewhat, and allow that after all this useful and still growing science can not only borrow from their laboratories, but repay with interest, and that it offers a more divergent scope than is often imagined for discoveries tending to throw light on some of the most intricate problems of physics.

[1] *Vide* "Elementary Meteorology," by R. H. Scott, F.R.S., 1883, p. 4.
[2] "Vade Mecum," p. 3.

Vol. I. of these "Memoirs" comprises twelve papers, the first of which is dated December 8, 1876, and the last October, 1881, which we will now proceed to examine seriatim.

Paper I. "The Winds of Calcutta," by H. F. Blanford, F.R.S.

This paper represents an analysis of ten years' hourly observations of the wind vane, and four years' anemograms. One of the first things we notice is that while the annual resultant, calculated by Lambert's formula, which takes no account of variation of velocity but assigns an equal value to all observations, is south 14° west, that derived from the four years' anemograms, where the true resultants enter, is south 18° east, the difference being caused by the greater frequency and less velocity of west than east winds at Calcutta. Another interesting conclusion deducible from the annual figures is that the velocity of the Bay of Bengal (south-east) branch of the monsoons current is considerably less than that of the Bombay, or as it is called Arabian Sea (south-west) branch of the monsoon. This fact was previously noticed by Mr. Blanford in his paper on the "Winds of Northern India,"[1] and receives further confirmation from a comparison of wind-velocities at representative stations in the Bombay and Bengal Presidencies in recent reports. Thus in that for 1877 Prof. Eliot gives the following comparison of velocities for representative stations in August, on opposite sides of the Peninsula:—

Bombay.	Average daily wind velocity —August.	*Bengal.*	Average daily wind velocity —August.
Kurrachee ...	497·8	Sangor Island	251·0
Bombay	408·3	Calcutta	123·4
Belgaum	213·3	Chittagong ...	151·4
Bangalore ...	219·0	Dacca	147·9
Akola	189·5	Patna	80·4
Nagpur	131·9	Allahabad ...	91·6
Jubbulpore ...	127·5	Roorkee	65·5

Prof. Eliot ascribes this defect in velocity of the Bengal branch of the monsoon to the deflection it undergoes by impinging upon the Arakan and Himalayan ranges.

And doubtless this expresses a portion of the truth. It ignores, however (explicitly at least), a circumstance which we think has a good deal to do with the result, viz. the fact that this deflection acts so as to continually rob the current of its *easterly component*, due to the change of latitude, and *which, in consequence of its northerly direction, tends to be continually reproduced.* In consequence of this, the current, instead of rebounding from the Arakan hills once for all, tends to hug the mountains all round the northern borders of Assam, and consequently loses a good deal of its velocity by the friction thus engendered. The importance of this south-east branch of the monsoon current cannot be over-estimated. It depends evidently for its existence on the presence of the Bay of Bengal, so that, were the latter area land instead of water, the now moist and fertile districts of Assam and Bengal would probably be arid wastes like the deserts of Scinde and Rajputana.

In discussing the diurnal variation in the direction and velocity of the wind, Mr. Blanford alludes to Mr. Chambers' discovery of the relation between the double diurnal variation in the wind components and the critical points in the diurnal barometric tide.

M. Rykatcheff not long ago, on the basis of the diurnal variation of the east and west components of the wind, laid the foundations of a most ingenious theory of the cause of the diurnal variation of the barometer.[2] Detailed reference to it here would be out of place, but it may be observed that the fact of the easterly components prevailing at the time of diurnal rise and the westerly at the time of the fall of the barometer, both at Calcutta and Bombay, accords with the daylight conditions at all the stations cited by Rykatcheff, as well as with the view that the air near the surface flows out from the 10 a.m. wave of high pressure, both in its advance from the east and its retreat towards the west.

The explanations of the diurnal oscillation of the barometer, propounded by Rykatcheff and Chambers, while they coincide in attributing it to the proximate influence of the analogous diurnal variation in the velocity and direction of the wind, differ from each other essentially in one or two points. Thus Rykatcheff leaves the north and south components out of account

[1] *Phil. Trans.*, vol. clxiv. part ii., pp. 563-653.
[2] "La Marche Diurne du Baromètre en Russie et quelques Rémarques concernant ce Phénomene en général." *Rep. für Met.*, 1879.

altogether, and only takes the east and west components into consideration. Mr. Chambers, on the other hand, while attaching considerable importance to the meridional components, except very near the Poles, omits curiously enough all reference to the probable corresponding variations in the upper currents of the atmosphere, upon which Rykatcheff discourses most effectively. Each theory alone is defective; a combination of the two would probably form a fairly satisfactory explanation of a considerable part of the diurnal variation, though it will be safer at present to conclude that, while there is a very intimate relation between the diurnal variation of the wind and the barometric tides, we do not as yet know for certain whether either is the cause of the other or whether both are not effects of some common cause.[1]

Mr. Blanford discusses the diurnal variation of the wind at Calcutta through the medium of Bessel's interpolation formula, and by comparing similar terms in the wind and barometric equations deduces several interesting results. Thus the single (semicircular) oscillation of the north and south components, is found to represent in all probability the diurnal land and sea breeze, while the single oscillation of the east component coincides in direction and phase with the rise and fall of the day land wind from the interior of the continent.[2]

Dealing in like manner with the double (quadrantal) terms, it is shown that the variations of both the north and south components and east and west components is very similar to that of the second term in the barometric fluctuation, though the course of the north and south com onents is exactly the reverse in direction of those at Bombay.

In part ii. of this paper the thermal, baric, and hyetic windroses are discussed. The results may be briefly summed up as follows:—(1) that polar currents play no part in the wind system of Bengal, which indeed might have been anticipated, owing to the presence of the Himalayan barrier to the north isolating India in so unique a manner from extra-tropical influences; (2) that rain is most probable, not when the monsoon current is blowing steadily, but "when it is deflected from its normal direction by some local irregularity of pressure," in other words, when *small* depressions prevail, and in this respect it would seem that the law is one of general incidence; (3) that when the periodic components are eliminated there is no definite relation between the non-periodic variations of pressure and those of temperature and humidity. This result is just what we should expect from a local application of the statistical method, now we know, thanks to the recent development of the synoptic method, that the larger oscillations of pressure which accompany the passage of cyclones and anticyclones, are due to conditions which are in operation over regions widely removed from those where such oscillations prevail. This conclusion, moreover, so far as India is concerned, derives fresh support of late from the fact that certain abnormal features in the pressure over India and adjoining countries during the droughts and famines in Madras and the North-West Provinces in 1876-77 were due to some widespread influence which mainly affected the atmosphere *above* the level of the Himalayan sanitaria (7000 feet), since the variations in the density of the atmospheric stratum *below* this level were not only opposite to those at the higher levels, but to those which prevailed in the total pressures at the lower stations throughout the Peninsula.

Paper II. "The Meteorology and Climate of Yarkand and Kashgar."—This is a discussion chiefly of the registers kept by Dr. Scully, of the Bengal Medical Service, who, in the autumn of 1874, accompanied the mission sent by the Indian Government to Kashgaria under the charge of Mr. R. B. Shaw. The observations were carefully made, and as they represent a district whose meteorology had hitherto been entirely unknown, and of which we can only get samples by the aid of such rare opportunities as political embassies, their discussion is well worth the labour which Mr. Blanford has bestowed upon it.

Where it was possible, on the route, and at Yarkand and Kashgar on four days in each month, horary observations were made.

Some evidence of the laborious nature of the task to which Dr. Scully so nobly devoted himself may be gathered from the fact that in order to get a second observer he was obliged to teach an uneducated hill coolie named Bhola, first the use of English numerals, then decimal fractions, and finally the mode of reading and recording the various instruments in the newly-acquired notation.

Any one who knows the amount of intelligence evinced by the ordinary Indian coolie, will agree with Mr. Blanford in his remark that "it was an achievement which reflected the highest credit on both teacher and pupil."

From the geographical description of the country which accompanies Dr. Scully's report, we gather that the soil is very arid, the rivers are chiefly snow-fed, and that there are deposits of *loess*, similar in all respects to that of the Rhine and the Danube. By many, this loess is inferred to be of subaërial origin, and the peculiar dust-haze which prevails in these regions, is cited in favour of this idea.[1]

The chief points noticed in this paper are: (1) the excessive dryness of the climate and consequent large amplitude of the diurnal temperature oscillation; (2) the abnormally large annual range of barometric pressure; and (3) the fact that the non-periodic oscillations of barometric pressure at Yarkand, unlike those of stations situated to the south of the Kuen-Lun, Karakoram, and Himalaya ranges, appear to have some sort of connection with those of Europe. Such large oscillations are in fact probably confined to the lower and denser portion of the atmosphere, which is effectually partitioned off from the north by ranges of such lofty elevation.

The latter part of the paper deals with the diurnal periodic oscillations in the pressure, vapour tension, and winds, and it may be observed that, in regard to the first of these, Yarkand is found to exceed in range any other place under an equally high latitude. Even in winter it averages 0·07 inches between 10 a.m. and 4 p.m., while in June and July it is 0·098 inches between the same hours.

In other respects the curve is characteristically continental, the fall of the night tide being almost evanescent.

The diurnal variation of the wind at Yarkand also presents a peculiarity worthy of remark, in that, as at Zikawei, in China, and Upsala, in Sweden, it violates the general rule that easterly components prevail in the morning and westerly in the afternoon. Like the sea in these places, the desert stretches to the east of Yarkand, and there is every reason to suppose that something analogous to a sea-coast system of local convection currents exists which overrides the normal right-handed rotation of the diurnal breeze.

(*To be continued.*)

UNIVERSITY AND EDUCATIONAL INTELLIGENCE

THE Gilchrist Engineering Entrance Scholarship at University College, London, will be open to competition at the end of September. The conditions of examination are this year somewhat altered in a direction which places the scholarship better within the reach of those for whose benefit it was founded. The detailed regulations can be obtained on application to the secretary of University College; the following is a summary of them:—Candidates must be under nineteen years of age, and must send in notice to compete by September 23. The subjects of examination are (1) elementary mathematics, and (2) any two or more of the following five subjects: mechanics, mechanical drawing, essay on one of three given subjects connected with mechanics or engineering, French or German, the use of tools, either carpenters' tools, or the lathe (wood or metal), or the file. The Scholarship is of the value of 35*l.* per annum, and is tenable for two years. There is also at University College a Senior Engineering Scholarship, awarded at the close of the session, of the value of 80*l.* The regulations affecting this scholarship, as well as those of the Andrews Entrance Prizes, &c., can be obtained on application to the Secretary.

UNIVERSITY COLLEGE, Dundee, has already issued its first Calendar, necessarily a thin one, but bound to increase in size. So far as the science classes are concerned, and these are the prominent features in the College, the arrangements are fairly

[1] A full account of Mr. Chambers' theory is given in the *Philosophical Transactions*, 1873, and in the *Proc. Roy. Soc.*, xxv., p. 402.

[2] This wind is called an anti-convection current, as will subsequently be seen in our notice of a paper by Mr. Chambers on "The Winds of Kurrachee."

[1] Richthofen, in his work on China, similarly attributes the enormously thick formations of *loess* in the northern part of that country, to the action of the winds. It seems reasonable, however, to imagine that, like the analogous European deposits, it might have been originally deposited as Pleistocene glacial unstratified mud from the neighbouring Thian-Shan and Kuen-Lun ranges, and that it has since been redistributed and perhaps in part augmented, by æolian action. The fact that it occurs in North China, and not South China, and that traces of the Glacial period extend as far south as the Himalaya, favours this supposition.

complete, and a good staff of professors has been obtained. That literature will not be neglected is evident from the fact that the Principal of the College, Prof. Peterson, has for his subjects Latin and Greek. We have also received the Calendar of the Mason Science College, Birmingham, a thick volume which shows the institution to be in excellent working order. The Calendar of University College, Liverpool, is much more modest, though its staff of professors and lecturers is pretty comprehensive.

SCIENTIFIC SERIALS

Atti of the R. Accademia dei Lincei, May 6.—Report on Veri and Parona's "Geological Studies of the Fossil Shells of Terni and Rieti," by MM. Taramelli and Capellini.—Report on Dr. Lucchetti's "Crystallographic Notes," by MM. Koerner and Spezia.—A memoir (in French) on the invariants and covariants of a function transformed by a quadratic substitution, by W. Spottiswoode.—On the nature of the expansions of gas produced by the electric spark, by Sig. Villari.—Distribution of matter acting on the surface of an ellipsoid in order to procure in the interior of such a body a given action constant in force and direction, by Sig. Glaser.—On the relations existing between the refrangent power and chemical constitution of organic combinations, by MM. Bernheimer and Nasini.—On a hypergeometrical differential equation, by Sig. Besso.—Some theorems relative to the binary forms of any power, and their application to the study of the multiple roots of equations of the sixth degree, by Sig. Maisano.—On some derivatives of berberine, by M. Bernheimer.—On the distortion of perspective observed in the telescope, by Sig. Govi.—Meteorological observations at the Royal Observatory of the Campidoglio during the month of April.

May 20.—Note on Ugo Balzani's "Early Chroniclers of Europe," by S. Tommasini.—On the commentators on Martianus Capella, by Sig. Narducci.—On the theoretic value of the coefficient of tension, of the atomic specific heat of aëriform bodies, and of the dynamic equivalent of caloric, by Sig. Violi.—Account of the recent archæological discoveries in Ventimiglia, Gussola, Casalmaggiore, Norcia, Tarentum, and other parts of Italy, by Sig. Fiorelli.

SOCIETIES AND ACADEMIES

PARIS

Academy of Sciences, August 13.—M. Blanchard, president, in the chair.—On Kekulé's β-butylglycol CH_3—CH.—OH—CH_2—CH_2.OH, obtained as an accessory product of the hydrogenation of aldehyde, by M. Ad. Wurtz.—On the laws of reflection as applied to the displacements of elastic bodies of definite form acted on by external forces, by M. X. Kretz.—A comparison of the hypotheses of magnetic fluids and molecular currents, by M. P. Le Cordier.—Experimental researches on the action of a liquid introduced by a special process into the tissues of the vine for the purpose of destroying phylloxera (continued), by M. P. Lafitte.—Note on a composition employed by Mr. Hatch of San José, California, for the destruction of phylloxera, by M. J. Caire. The constituents of this compound are equal weights of sulphuret of carbon, potash, oxide of iron, and sulphur, mixed with eight times the same amount of mercury.—On copper as a preventative and curative of cholera, by M. V. Burg. After a study of thirty years the author concludes that copper absorbed in various ways into the system acts as an almost perfect prophylactic, the exceptions not being more numerous than in the case of vaccination as a preventative of small-pox. Amongst other precautions he recommends the external application of copper under the metallic form of armatures, plates, or even ordinary coins; the burning of dichloride of copper in alcoholic lamps; wine mixed with the natural mineral water of Saint-Christau; and the use of vegetables rendered green by sulphate of copper. The question of the treatment of cholera patients by copper is reserved for a future communication.—In connection with this subject, M. P. Davin recalls a memoir addressed by him to the Academy in July, 1873, on the bronze dust used in gilding as a specific against cholera.—Observations relative to a previous communication of M. A. Gaillot on the changes produced in the length of the Julian year, by Mr. E. J. Stone of the Radcliffe Observatory, Oxford.—On the determination of the right ascensions of circumpolar stars, by MM. Ch. André and Gounessiat.—On the critical point of oxygen, by M. E. Sarrau.—On the distribution of the caloric liberated or absorbed by oxygen and carbon respectively when combining to form oxide of carbon and carbonic acid, by M. A. Boillot.—On the composition of the asphalt or bitumen of the Dead Sea, by M. B. Delachanal. The presence of sulphur in considerable quantities is determined, implying a mineral origin, and distinguishing this bitumen from all others, which are of organic origin.—On the danger of contagion from the use of cracked stoneware in infectious diseases, by M. E. Peyrusson. It is shown that the germs of cholera, typhoid fever, and similar disorders may be preserved even in the slight fissures on the glazed surface to which all crockery and faïence are liable.—Memoir on wheaten flours (part ii.), by M. Ballard. The author shows that variable quantities of gluten may be obtained from the same flour, according to the different treatments to which it is subjected.—On the origin of individuality in the higher animals, by M. de Lacaze-Duthiers.—On the evaporation of marine and fresh water in the Rhone delta and at Constantine in Algeria, by M. Dieulafait.—On the means employed to determine by continuous registration the slight movements in the crust of the earth, by M. B. de Chancourtois.—On the cultivation of the date-palm in soils charged with marine salt, by M. A. Richard. The present flourishing condition of the palm-groves at Elche and Alicante, on the south-east coast of Spain, shows that this plant thrives well in land saturated with salt water.

VIENNA

Imperial Academy of Sciences, June 7.—C. von Ettingshausen, on the Tertiary flora of Borneo.—Z. von Roboz, on *Calcituba polymorpha*.—G. Tschermak, contribution to classification of meteorites.—H. von Foullon, on the mineralogical and chemical composition of the meteorite which fell at Alfianello on February 16, 1883.—J. Kachler and F. V. Spitzer, on the action of sodium on camphor.—F. W. Dafert, on periodides.—J. Schlesinger, on the causes of inertia and motion of masses.

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THURSDAY, AUGUST 30, 1883

THE BRITISH ASSOCIATION

SOUTHPORT has been selected for the meeting of this body in 1883, and the fifty-third annual session will commence on the 19th and end on the 27th of September. It is not necessary to dilate upon the various reasons which led up to this arrangement. Suffice it to say, that there was strong opposition on the part of considerable University and manufacturing cities; and the success of Southport may, no doubt, be in great part attributed to the enterprise and business zeal which, within two generations, have raised it from a seaside village to a populous, well-built, easily accessible corporate borough.

To many people, perhaps the majority, in the southern counties, Southport is a name, and nothing more. It is doubtful whether the topographical knowledge of most educated Southerners would enable them to say, offhand, in what county it is situate. No one, however, has probably once visited the town without finding many things to admire and buildings to surprise. We may be pardoned for saying that Southport is less than eighteen miles from Liverpool, thirty-six from Manchester, sixteen from Preston; Wigan, Blackburn, Bolton, Burnley, Oldham, and other large centres of manufacture being within easy distances. It should be enough to say that a circle, having its centre at the Town Hall, with a radius of forty miles, would include 4,000,000 people. The public buildings are handsome and commodious, and every accommodation is at hand to render the forthcoming gathering, even if very large in number, a perfect success.

Liverpool being so near, a well-founded hope exists that the town will be honoured by a visit from numerous American, colonial, and foreign men of science. It must not be forgotten that Edinburgh and Dublin, Glasgow and Nottingham, Newcastle and Birmingham, are all within ready access of Southport.

Southport is about five hours from London, and has through communication by two railways with every important town in the United Kingdom.

On this occasion the Reception Room will be at the Cambridge Hall; the Council Room in the same building; the evening meetings and *conversazioni* will take place in the Winter Gardens, which have been specially retained wholly for this purpose; the General Committee Room will be at the Town Hall; Sections A and B at the Atkinson Art Gallery; Section C at the Temperance Hall; Sections D_1 and D_2 respectively at the West End and at the Congregational School Room; Section E at St. George's Hall; Section F at St. Andrew's Hall; and Section G at the Town Hall. After this list, no one can doubt that there is ample accommodation, both for the members as a body, and for the several Sections.

There are several first-class hotels, capable alone, it is estimated, of boarding and lodging 1000 people or more. Apart from this, there are houses where apartments are available to any reasonable extent. Probably the various "company"-houses, as they are styled locally, could accommodate between 10,000 and 12,000 persons. In addition to the regular places of this sort, many more householders are prepared to become amateur lodging-house keepers for the time in case of need. The Secretaries and Recorders of Sections will be boarded and lodged in houses on the Promenade, facing the sea, and within three minutes' walk of the Winter Gardens, or four to six minutes from the Reception Room, which is in the centre of the town.

Dealing in detail with the arrangements for the meeting, it may be mentioned that the Local Executive Committee has for chairman Dr. James Wood, Mayor of Southport. Among other members of the Committee in question, the names are found of the Earl of Derby, the Earl of Crawford and Balcarres, the Earl of Lathom, Prof. Greenwood, and Prof. Roscoe (all of whom are also vice-presidents of the Association for this year). Committees have been formed to deal with hospitality and lodging arrangements, excursions, *conversazioni* and evening meetings, audit and finance, and the lecture to the operative classes. The local secretaries are Mr. J. H. Ellis (the Town Clerk), Dr. H. H. Vernon, and Mr. T. W. Willis (B.A. Cantab.).

The retiring president, we may remind our readers, is Sir C. William Siemens, and the president-elect is Prof. Arthur Cayley, Sadlerian Professor of Mathematics in the University of Cambridge.

The first general meeting will be held on Wednesday, September 19, at 8 p.m., when Sir William Siemens will resign the chair, and Prof. Cayley will assume the presidency and deliver an address. On Thursday evening, September 20, at 8 p.m., there will be a *soirée* in the Winter Gardens; on Friday evening, September 21, at 8.30 p.m., a discourse on recent researches on the distance of the sun, by Prof. R. S. Ball, Astronomer-Royal for Ireland; on Monday evening, September 24, at 8.30 p.m., a discourse on galvanic and animal electricity, by Prof. J. G. McKendrick, Professor of Physiology in the University of Glasgow; on Tuesday evening, September 25, at 8 p.m., a *soirée* in the Winter Gardens; and on Wednesday, September 26, the concluding general meeting will be held at 2.30 p.m.

It must not be forgotten that on Saturday, September 22, at 7 p.m., a lecture to working people will be delivered by Sir F. J. Bramwell on "Talking by Electricity: Telephones"; it is expected that the usual flock of Saturday excursionists will furnish an overflowing audience.

In connection with the *soirées*, it may as well be stated here that there is to be an exhibition of objects of scientific and artistic interest in the covered skating rink, a very prominent feature of which will be an exceptionally complete installation of electric lighting on the Siemens system. We believe that this last is intended to be one of the most complete exhibitions ever yet seen of its kind. There will also be in the large Pavilion (where the presidential address and evening discourses will be given, and concluding general meeting held) an exhibition of Lewis's improved system of incandescent gas lighting. This will also be given on the nights of the two *conversazioni*. The entries for other classes of exhibits (microscopes, &c.) are very satisfactory.

A feature of these yearly gatherings is the arrangement for excursions to places of interest in the neighbourhood of the town selected from year to year. These are very numerous this time, and include Knowsley, Lathom House, Ince Blundell Hall, the Abram Colliery, Stony-

hurst College and Whalley Abbey, the Lake District, Haigh Hall, St. Helen's and Widnes, the Wigan Coal and Iron Company's Works, Chester and Eaton Hall, Liverpool (including a visit to a White Star steamer and a run along the dock's front), Clitheroe District (Geological), and others which may be announced in these columns next week.

Rufford Park and Rufford Old Hall will also be visited, as well as the county town, Lancaster, which deserves more than passing mention. There is the old church there, the ancient castle (the residence, ages ago, of John of Gaunt), aqueducts of some importance, the Roman camp in the vicarage grounds, the assize courts, and many other objects of attraction and public buildings, including asylums and hospitals of ancient and of modern establishment, and of very various character.

There will be garden parties at Knowsley (by the kindness of the Earl and Countess of Derby), at Lathom House (on the invitation of the Countess of Lathom), and at Ince Blundell (the residence of Mr. T. Weld Blundell). In addition, the Mayor of Southport will give a garden party at Hesketh Park on Friday, September 21; and it is rumoured that he will also have two afternoon receptions, on days to be arranged hereafter, at his own residence, Woodbank. The Rev. C. Hesketh Knowlys, the rector of the mother parish of North Meols, will also give a garden party in his grounds.

The three railway companies running into the town, two of which have terminal stations at Southport, are all offering advantages and facilities in order to help making the meeting a success. For instance, the London and North Western Railway will run through carriages to Southport on September 17, 18, and 19, from London (Euston Station), Willesden Junction, Northampton, Stafford, and Crewe, by the 7.15 a.m, 11 a.m., 1.30 p.m., 3.0 p.m., and 4.0 p.m. trains, and similar arrangements will be carried out for the return journey.

Liberal arrangements have also been made by the local railway companies for the benefit of excursionists to the many attractive districts in the north and west of England.

The arrangements at the Reception Room in Cambridge Hall will be of the usual complete kind at these gatherings, including postal, telegraph, ticket, reserved seats, lodgings, inquiry, lost property, daily journal, members' lists, local programme, guide-book, and other departments. The hall has been newly decorated throughout for the occasion, and, when furnished and in full work, will doubtless bear favourable comparison with similar rooms at previous meetings of the Association. The telephone will also be brought into play, so as to connect all the Section Rooms both with the Reception Room and the Winter Gardens, as well as with the principal hotels and other large establishments in the town.

A local fund has been raised of over 2600*l.*, and strenuous efforts are being made to increase that amount to 3000*l.* This will most probably be accomplished.

Looking to all these facts—bearing in mind that Southport has a promenade of over a mile facing the sea, on which are three of the chief hotels and a string of handsome private residences and lodging-houses; a pier, which, with its extension, is within a few hundred yards of a mile in length; the boulevards (in Lord Street and its continuations east and west), bordered by handsome edifices, public buildings of no mean architectural pretension, banks, &c.—enough has been said to justify the hope that Lancashire will once more distinguish herself as the hostess of the British Association, as she undoubtedly did in 1870 (the last time that it met within her borders), when, under the presidency of Prof. Huxley at Liverpool, one of the most characteristic, as well as one of the most numerously attended and in every way brilliant and successful meetings of the British Association was held.

PROFESSOR HAECKEL ON CEYLON

A Visit to Ceylon. By Ernst Haeckel; Translated by Clara Bell. (London: Kegan Paul, Trench, and Co., 1883.)

WHEN a man of scientific genius writes a popular book, it will generally be found to be either a great success or a great failure; mediocrity, as a rule, does not attend the work of such a man in either direction. Now Prof. Haeckel is already well known to all the world as one of the few leaders in science whose literary ability is on a level with his more professional attainments, and whose genius is therefore exhibited in exposition as conspicuously as it is in research. Thus it was that when we heard he intended to publish a popular account of his six months' travel in the tropics, we expected a great treat in the way of literary performance. We had, of course, read a good deal about Ceylon before, and thus knew that it was a part of the world which in point alike of natural scenery and natural history was well calculated to arouse the enthusiasm of such an artistic-minded naturalist as Prof. Haeckel; and knowing that his pen can paint almost as vividly as his brush, we were prepared for something of unusual interest in the story of his "Visit to Ceylon." Perhaps, therefore, it is not possible to say anything in higher praise of his book than that it has even surpassed our anticipations. The man of science has retired, as it were, into the background, and left the way clear for the man of letters, the shrewd observer of men and things, the poetic lover of Nature—the frank, open-hearted, wide-minded German character which finds so admirable an expression in this great German biologist. Whether he is diving down among the coral reefs, forgetting his wounds in the keen joy of exploring the beauty and the wonder of those biological treasure-houses, or whether he is scrambling to the "World's End" through almost untrodden and untreadable jungles 8000 feet above the sea; whether he is moving in English society and deeming it needlessly formal in the matter of dressing for dinner under a tropical climate, which has turned his carefully-provided swallow-tail coat as white as a sheet with mildew; or whether he is living for six weeks at a time zoologising in a remote native village without ever seeing a white man—wherever he is and whatever he is about, we are alike charmed by the character of the man which unconsciously looks out at us in every page, and throws around him, as it were, a halo of romance.

We have said that in all this the man of science has been allowed to retire into the background. But not on this account has the man of science been idle. Prof.

Haeckel went to the tropics to work and not to play, and work he did, with a vigour and pertinacity which, under the circumstances described, can only be called astonishing. To have gone out day after day and week after week surface-fishing in an open boat beneath the almost vertical rays of a tropical sun, is in itself to have performed a feat of physical endurance which, so far as we are aware, has never been performed by any other naturalist; and to have worked steadily for half a year from daydawn to night, exploring, collecting, and investigating as Haeckel investigates—feeling all the while, as he expresses it, that each day was costing him, as a mere matter of money, somewhat over 5*l.*—to have worked thus would have been to exhaust the strength of many a younger man even in a much higher latitude than that of Ceylon. The results attained by such a naturalist in such a region, and working at such a pressure, of course constitute an immense harvest—so much so, indeed, that he thinks he has more material in his collections than the term of his natural life will admit of his sufficiently investigating. But with all this, he has wisely avoided burdening his account of "A Visit to Ceylon" with any details of his scientific labours. The book is intended for general readers, and while a sufficient number of scientific observations on the flora and fauna of the island are thrown in here and there to complete the picture which he gives of the place, these are always judiciously subordinated to the main design of speeding an honest tale by telling it plainly.

After an entertaining account of his voyage and of a week spent in Bombay, the traveller proceeds to give his first impressions of Ceylon. He is most of all struck with the magnificent luxuriance of the tropical vegetation, some of his descriptions of which are admirable specimens of word-painting. Everywhere he meets with the greatest kindness and courtesy, of which he is lavish in his acknowledgments. Having been a guest at various houses, visited and studied botanical gardens, made sundry excursions, &c., he eventually sets up a zoological laboratory upon the coast. This having constituted the main object of his journey, he had taken with him sixteen large packing cases filled with all the equipments required for zoological and morphological research. The choice of site lay between one or other of two sheltered bays—Galle and Belligam. At the former he would have the advantage of living among civilised Europeans, and of being the guest of the hospitable and cultivated English consul, Mr. Scott, of whom he speaks in terms of high esteem; at the latter he would be the only European within a radius of many miles, and require to take up his quarters in a small government house. Such being the circumstances, he says:—

"After much hesitation, and long debating the *pros* and *cons*, I finally decided for Belligam, and I had no reason to regret the choice. The six weeks I spent there were full to overflowing of wonderful experiences, and never to be forgotten as forming the crowning 'bouquet' of my Indian journey, the sweetest and brightest flowers in a garland of delightful memories. Though I might perhaps have carried on my zoological studies better and more conveniently in Galle, I gained infinitely more on the side of general knowledge of nature and humanity in the charming seclusion of Belligam."

If the naturalist had no reason to regret this choice, assuredly his readers have not, for the account which follows of his residence among the natives is the most entertaining part of his narrative. On his first arrival he is met by a general assembly of the inhabitants, his advent having been expected in consequence of the governor of the island, Sir James Longden, having written to the native officials "to be in all respects civil and serviceable." The civility in the first instance takes the form of series of ceremonious addresses presented to him by one native magnate after another, emphasis being given to the close of each by "a grand rattle of drums performed by a row of tom-tom beaters squatting in the background." These high functionaries presented in their dress a sort of hybrid between the European and the Cinghalese. "Beginning at the top, a tall English chimney-pot charmed the eye—of all head coverings beyond a doubt the most hideous and inefficient. However, as the Cinghalese see Europeans wear this cylindrical headpiece on all solemn occasions as the indispensable symbol of birth and culture, never abandoning it even in the greatest heat, they would regard it as a serious breach of etiquette to appear without the singular decoration." Below the hat there came "an enormously high and pointed white shirt-collar, and a coloured silk scarf tied in a bewitching bow." Then a swallow-tailed dress coat, white waistcoat with jewelled buttons and gold chains. But instead of trousers wherewith to complete this grotesque imitation, each of the dignitaries ended off in a red or party-coloured petticoat and bare feet.

Having suitably acknowledged this unexpected ceremony, Prof. Haeckel sets to work unpacking and setting up his laboratory in one of the rooms of the government house. From that moment throughout his stay of six weeks he is pestered by the insatiable curiosity of the entire neighbourhood, and even by that of native visitors from a distance, which on one occasion presented themselves in the form of four old maiden ladies of distinction, "each more wrinkled and hideous than the last," who desired to be instructed in science and to have their photographs taken. The Professor is here ungallant enough to remark, "If they had been but three, I could have mistaken them for the three Phorcydes, the witches of the classical Sabbat, and might have made myself agreeable to them after the fashion of Mephistopheles." Hoping to satisfy the universal curiosity in a collective manner, he tried the experiment of giving lectures through an interpreter; but he found that there was no spark of real scientific interest underlying the childish desire to see something new. However, he managed to get on admirably with all around him, gave away multitudes of presents in the shape of coloured prints, &c., presided one day over the grand Buddhist festival for the 19th of December, and on the 20th filled the same office of president at the annual festival of the Wesleyan mission. "I had done honour to the sublime Buddha yesterday, and to-day I must pay tribute to worthy Mr. Wesley. . . . My friends in Galle and Colombo, who heard through the papers of my extraordinary proceedings, laughed at me 'consumedly.'"

But we have no space to give any sketch of the strange experience of these six weeks' sojourn among the primitive natives, so curiously composed of the instructive, the æsthetic, the ludicrous, and the pathetic. We have said

enough to show that the book* ought to be read by every one, and therefore we shall now conclude by drawing more prominent attention to sundry opinions and suggestions, which, as Englishmen, we should desire to see our Government consider and act upon.

First, as regards the promotion of science:—

"The extraordinarily favourable climate and position of Peradenia especially fit it for more extensive use from a scientific point of view as a botanical station. In the same way as our young zoologists find the recently established zoological stations on the sea coast (at Naples, Roscoff, Brighton, Trieste, &c.) of inestimable value for their deeper scientific studies and experiments, a year's residence in such a botanical station as Peradenia would give a young botanist more experience and work than he could obtain in ten years under the various unfavourable conditions at home. Hitherto, less has been done in the tropical zones than elsewhere for such establishments for study and experiment, though they would be exceptionally beneficial. If the English Government would establish and maintain such a station for botany at Peradenia, and one for zoology at Galle—in the charming bungalow, for instance, belonging to Capt. Bayley, which is admirably suited to such a purpose [and would be sold by the owner to effect it]—they would be doing signal service to science, as they have already done by the *Challenger* Expedition and other great undertakings—and once more put to shame the great Continental States of Europe, who spend their money chiefly on breechloaders and big guns."

In reading this passage all true Englishmen should feel regret that their Government is not deserving of the meed of praise which the courtesy of the writer bestows. Seeing that we are the great maritime and colonising power, it is nothing short of a public disgrace that we are without a zoological station upon any of our thousands of miles of coast, and that hitherto there is no prospect of our escaping from the sarcasm (whether conscious or unconscious) wherewith the national seat of "deeper scientific studies and experiments" in marine biology is here specified as *Brighton*. Is it too much to hope that the Fisheries Exhibition may at length help to open the eyes of a Liberal Ministry to the importance of doing something in this direction?

Only in one particular does the English rule in India fall under censure, and this has reference to the atrocious treatment of the stage-coach horses. The scenes described are certainly monstrous beyond imagination—flogging by the whole village, dragging by the nostrils, wringing by the ears, and burning with torches. Truly, as Haeckel observes: "It is difficult to conceive how the English Government, which is generally so strict in its arrangements and discipline, has not long ago put an end to this brutality to animals, and more particularly extended its protection to the wretched horses that serve the 'Royal Mail Coach.'" Here is surely something for the anti-vivisectionists to memorialise upon with benefit.

We cannot take leave of this delightful book without congratulating the translator on the beautiful English into which she has rendered it.

GEORGE J. ROMANES

OUR BOOK SHELF

Elements of Histology. By E. Klein, M.D., F.R.S., &c. (London: Cassell and Co., 1883.)

THIS, which is the first of Cassell's "Manuals for Students of Medicine," contains 342 closely-printed pages, with 168 well-executed woodcuts, mainly reproduced from Klein and Noble Smith's "Atlas of Histology," or the "Handbook for the Physiological Laboratory," intercalated in the text. It is not too much praise to say that the information in this little volume is generally very complete, quite up to date, and written in a concise, though, at the same time, thoroughly clear style.

Dr. Klein wisely omits all reference to the titles of works and papers, introducing where necessary simply the name of the discoverer of, or observer most intimately associated with, the structure referred to. Where different opinions exist, this is obviously convenient, and the right thing to do; but why on page 7 the names of fifteen histologists, followed by the words "*and many others*," should be given, it is difficult to understand, especially as they are quoted with reference to the indirect division of nuclei or Karyokinesis, of which every worker at histology must have seen many examples.

In a work like the present, where all usually received ideas are given, it is curious to find that no reference is made to Schäfer's with regard to striated muscle. Surely this cannot be an accidental omission, especially as Haycraft is twice quoted.

That the action of tannic acid on human red corpuscles is not described in the text, although figured (p. 9, fig. 9*a*), is clearly an oversight, as that of boracic acid on newt's red corpuscles is both figured and described. In future editions it will be convenient if the same numbers be used in the text as in the diagram when describing the different parts of the kidney tubules, constant reference to the description of fig. 133 being now necessary.

With the exception of the above minor details, unqualified praise must be given, and the "Elements of Histology," which is really a very complete manual, should be used and re-used by every student and practitioner of medicine who wishes to acquire a sound knowledge of the normal histology of man.

J. W. G.

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications.*

[*The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to insure the appearance even of communications containing interesting and novel facts.*]

"Elevation and Subsidence" again

A LETTER appears yesterday, again criticising Mr. Starkie Gardner's general views about pressure, in the same sense as was done by myself a fortnight ago. But, referring to that gentleman's opinion that pressure can render rocks molten or fluid, Mr. Young goes on to remark: "Is not the supposition the exact reverse of what is really the case, viz. that not only does pressure *not* liquefy rocks, but actually *prevents* their melting at a temperature at which they *would* melt were the pressure removed?" Your correspondent, offering this remark with a query, seems as if his mind was not quite made up on the subject; and with reason; for it must, I think, be considered at present an open question whether the temperature of rocky matter is, or is not, raised by pressure.

Sir W. Thomson stated, in an address to the Geological Society of Glasgow in 1878, that certain experiments by Dr. Henry Muirhead and Mr. Joseph Whitley seemed to show that iron, copper, brass, whinstone, and granite are less dense in the solid than in the liquid state at the melting temperature. If so, pressure would assist in liquefying these substances. On the other hand, some observations of Mr. Johnston-Lavis, made on lava at Vesuvius, point in the opposite direction. Granted that the earth is, as a whole, extremely rigid, we cannot gather from that fact any certain information about the effect of pressure on "rocky matter," when near the melting temperature. We do not know whether the nucleus of the earth consists of matter which could, under any conditions, be directly converted into surface rock; nor yet do we know anything certain about its

temperature at depths bearing considerable ratios to the radius. Indeed the state of our knowledge is best expressed by the words of the old song, "Oh dear! what can the matter be?" It is even conceivable that, whatever it be, it may be above its own critical temperature; in which case the laws affecting incompressible liquids become inapplicable.

An interesting paper upon this latter hypothesis was published by Prof. Zöppritz in the *Transactions* of the first Geographical Congress of Berlin, 1881. It is entitled "Ueber die Mittel und Wege zu besserer Kenntniss vom inneren Zustand der Erde zu gelangen," and published by D. Reimer, Berlin.

Harlton, Cambridge, August 24 O. FISHER

I OBTAIN NATURE in monthly parts, and am indebted to a friend for calling attention to the article on "Elevation and Subsidence" by Mr. J. Starkie Gardner in vol. xxviii. p. 323, in which he considers that, "wherever considerable weight is added to any part of the earth's surface, a corresponding subsidence of its crust almost invariably follows." As it is evident from the last paragraph in Mr. Gardner's paper that he esteems this opinion to be novel to the readers of NATURE, and being the first time it can be considered as having been discussed in your pages, it might have been more satisfactory perhaps had he passed in review the conclusions arrived at by others who have preceded him.

Sir John Herschel (see "Physical Geography," § 132, 1862, and "Familiar Lectures," Lecture I.), assumed in a general manner that "if continents are lightened they will rise; if the bed of the sea receives additional weight it will sink." It is to be regretted that the facts advanced as evidence by so great an authority did not prove sufficiently conclusive to claim general acceptance. Mr. T. F. Jamieson, F.G.S., in 1865 (*Quarterly Journal of the Geological Society*, vol. xxi. page 178), considered that the enormous weight of snow accumulated during the glacial period "may have had something to do with the depression of the land which then occurred, and that the melting of the ice at its termination would account for the rising of the land."

Under the advocacy of Prof. James Hall ("Palæontology of New York," vol. iii., 1859), the subject has received much consideration in America; this has been so great that Capt. C. E. Dutton, of the United States Geological Survey, was enabled to say that "few geologists now question that great masses of sedimentary matter displace the earth beneath them and subside" (NATURE, vol. xix. p. 251).

The principle that accumulation of material causes subsidence and that denudation results in elevation of the crust of the earth has been advocated by myself on numerous occasions during the last eighteen years, being considered equally applicable to rocks of every age during the whole series; in England from the Cambrian rocks of Shropshire to those now in process of deposition in the seas which surround our coasts. The idea originated to me from observations in the Longmynd and of the Upper Silurians of Shropshire and North Wales during 1864. Its universal application and the physical effects dependent on the phenomena formed the special subjects of two addresses as president of the Liverpool Geological Society in 1871 and 1872. The conclusions were deemed by NATURE (vol. vi. p. 379) of such importance that you considered my "interpretation of the facts deserved further consideration." Abstracts of these essays also appeared in the *Geological Magazine*, vol. ix. p. 119, and vol. x. p. 202. The views entertained have been subsequently advocated by me in the *Proceedings of the Liverpool Geological Society*, the *Geological Magazine*, and the *Reports of the British Association*, the last time being during the meeting of the British Association at Southampton (*Report*, 1882, p. 540), which paper has appeared in full in the *Geological Magazine* for July and August, 1883.

The only author who has considered this subject and to whom Mr. Gardner refers, is the Rev. O. Fisher, F.G.S., whom he deservedly praises for his masterly work, "The Physics of the Earth's Crust," 1881.

In spite of much adverse criticism I have been content to wait all these years, feeling convinced that after commendation similar to that accorded by you (by no means a singular occurrence), the subject of oscillation, as the result of changes in the distribution of sediments, would eventually be taken into consideration; for a frequent remark has been that "there appears to be something in it"; and no geological fact is more persistently referred to than that the formation of sedimentary strata of every age "*has occurred during a period of subsidence*."

Birkenhead, August 22 CHARLES RICKETTS

"Decentralisation in Science"

I FULLY agree with the remarks on this important subject made in your leading article of last week; and the necessity for local scientific societies being in some way placed in direct communication with each other and with the central metropolitan societies has long been present in my mind. It is perhaps as yet premature to broach any definite scheme for effecting this object, which, as the writer of the article points out, would be surrounded by very great practical difficulties. The whole subject might very well be discussed by the Conference of Delegates about to attend the meeting of the British Association at Southport.

There are numerous scientific societies and field-clubs throughout the country whose work is being frittered away in useless directions solely from the want of proper scientific guidance. As a preliminary step towards this most desirable economising of individual energy it appears to me that centralisation in the various counties is the first essential. This has been well enforced in the Preliminary Report of the "Local Scientific Societies" Committee of the British Association, published in NATURE a short time ago by Mr. Francis Galton, the Chairman of the Committee.

It is most satisfactory to know that the British Association has taken the matter in hand, as this body is of all others the most competent to deal with the subject, if for no other reason because the Association is the only scientific society that holds its meetings in various provincial centres. Among the difficulties that would have to be met in any scheme of county affiliation not the least formidable is the purely local feeling existing in many small societies, which leads their officers and members to reject all overtures from larger and more influential bodies in the mistaken belief that cooperation would entail a loss of individuality. A good illustration of this kind of difficulty has quite recently come under my notice in attempting to bring about some kind of amalgamation between the local societies of the county of Essex.

Till such narrow views of the functions of a local society are successfully combated no great advance towards centralisation can be made. R. MELDOLA

21, John Street, Bedford Row, W.C., August 27

The Earthquake in Ischia

IN 1878, when touring in the Himalayas, we spent the last two Sundays in August at Kyelang, in the Lahoul Valley. On each of these days I felt a sharp shock of earthquake about 4 p.m.

On both occasions I was sitting in a room on the upper floor of the German missionaries' house. A broad wooden verandah runs round the front and sides of the lower floor of this building.

I was about to rise and leave the room, when I heard a loud rumbling noise; my first idea was that the children of the house were amusing themselves with dragging each other in a small wooden waggon up and down this verandah as they were in the habit of doing, but the sound was much louder, as loud as that of a railway train when near the spectator. A second later I felt a violent oscillation, and a padlocked door, opposite the door of exit, shook violently backwards and forwards several times. A week later another earthquake occurred almost at the same hour, and under the same conditions.

Three years later, in 1881, we again passed through the same part of the Himalayas on our return from the Spiti Valley, which we had reached by way of Kunówar.

This time no earthquake took place during our stay in Lahoul; we crossed the Rotang Pass, and went to stay in the Kulu Valley with our friend Col. S——, Deputy Conservator of Forests at Mañali, about sixteen miles on the southern side of that mountain. Col. S——'s house is raised high above the river on the right bank of the Beas; it is placed in the midst of a Deodar forest, and built of wooden logs placed horizontally, and alternating with courses of large stones laid one upon the other, but not mortared together. A wooden verandah runs all round the building, and forms a balcony to the rooms on the upper floor. I imagine it is its mode of construction which enabled this house to resist the severe test to which it was subjected on this occasion.

On October 1, about 1 p.m., we were sitting, a party of three persons, in a temporary verandah resting on the bare earth, and floored with matting, which our host had erected to supplement the permanent one where our native tailor was seated at work.

I had just risen to speak to him, but before I could do so, a loud rumbling sound seemed to come on my right hand (or from the direction of the Kulu Valley).

One of the party called out *thun*—we had had a thunderstorm the day before—but changed the word to *earthquake*. For a second or two I held my breath—I felt rooted to the spot; then the permanent wooden balcony over my head began to creak and groan most violently, and I distinctly saw the front wall of the house advance towards me, and recede from me, three or four times.

After the motion had ceased, the rumbling sound, which at its greatest intensity seemed beneath our feet, died away in the opposite direction (or towards Simla). I made many inquiries afterwards, but was unable to ascertain whether any shocks of earthquake had been experienced on these dates either in Kulu in 1878 or in Lahoul or at Simla in 1881.

The recent catastrophe in the Island of Ischia has called the attention of those who make a study of such disturbances of the earth's surface to the simultaneous occurrence of earthquakes in various parts of the world, which induces me to send you these facts, in the hope that they may interest some of your readers and lead them to form some conjecture as to the possible centre of the earthquakes in the Himalayas.

I am not aware to what extent the geological formation of the Himalayas has been investigated, but (speaking as a non-professional) during three long tours in various parts of these mountains I have never observed any traces of extinct volcanoes. I ought, however, to mention, perhaps, that there are hot springs at Beshist on the left bank of the Beas River, about four miles from Mañali, and also at Manikern, in the Parbuti valley, which debouches from the Kulu valley, about thirty miles lower down, also on the left bank of the river. Manikern is a great place of resort for Hindu pilgrims, who consider these hot springs miraculous; it is also occasionally visited by Europeans who have found these waters efficacious in rheumatic affections. Earthquakes do not seem to be uncommon in these valleys, but it has been remarked that they generally, if not always, occur in the autumn, just when the rainy season is at an end.

COSMOPOLITAN

Lime and Bones

THE observation of your correspondent in NATURE, vol. xxviii. p. 329, regarding the effect of lime in strengthening the bones of children, induces me to communicate certain facts which I observed during a recent tour of two months in Norway.

We travelled by land from Christiania to Throndhjem, thence by sea to the North Cape and back, and made expeditions into the interior at different points on our downward journey.

I noticed everywhere an extraordinary number of weak-boned, crippled, and bandy legged children, also a great number of men and women with weak bones and distorted limbs.

Almost the whole of Norway is a network of mountains composed of various forms of primitive and metamorphic rock, and though marble exists in this country I saw none in the districts through which we passed.

COSMOPOLITAN

Christiania, August 11

Copper and Cholera

REFERRING to the paper read before the French Academy (as reported in your last issue) on copper as a preservative against cholera, it may be worth while to state that when visiting the great copper mines at Fahlun in Sweden (probably the oldest and largest in the world) I was informed that cholera had never appeared there, and that so well was the fact known that on the last visitation of cholera in Sweden some members of the Royal family took up their abode in Fahlun to escape the disease. The atmosphere was there loaded with copper fumes to such an extent that not a trace of vegetation was visible on the hills surrounding the town; so that this really seems to confirm by experience on a large scale the theory alluded to.

WALTER R. BROWNE

Sulphur in Bitumen

FROM the abstract of the meeting of the Paris Academy of Sciences in your last number (vol. xxviii. p. 408), M. B. Delachanal appears to consider that the presence of sulphur is peculiar to the bitumen of the Dead Sea, and from this he deduces a theory as to its inorganic origin.

In some experiments which I had occasion to make this summer on the bitumen of the Great Pitch Lake of Trinidad I found that this substance contained a very considerable quantity of sulphur. Several per cents. of the volume of the gas obtained by its destructive distillation consisted of hydrogen sulphide. The origin of this asphalt is generally considered to be organic, but I am not aware whether the entire absence of calcium salts from its ash, a fact which was proved nearly a century ago, and has since been confirmed, has been explained on this theory.

HUGH ROBERT MILL

Edinburgh, August 27

Thunderstorms and Auroræ

A CONNECTION between these phenomena has been repeatedly suggested. J. W. Ritter has articles on the subject in Gilbert's *Annalen* (1803 and 1804), and Kupffer has a long one in 1827. Other writers who have dealt with it or with the connection between auroræ and atmospheric electricity generally are Schübler (1817), R. Phillips (1854), F. Dellmann (1860), E. Loomis (1860, 1861, and 1862), A. Poey (1861), A. De la Rive, F. Abbott (1863), E. Edlund; and in NATURE, vol. xii. p. 127, there is a summary of the observations by Herr von Bezold. This may serve as a partial answer at the end of Mr. Chadbourn's letter.

A. RAMSAY

4, Cowper Road, Acton, W., August 27

The Meteor of August 19

THE details of this meteor in the letter of your correspondent Mr. Mott and my own are for the most part in such close accordance that one might suppose we had been comparing notes. There is, however, one particular in which our respective accounts differ so widely that one feels inclined to ask whether there were two meteors or whether one of your correspondents has made a mistake as to the direction of the course of the meteor.

First let me correct an error of my own. I find now I was wrong in giving the point of starting as a *few degrees* eastward of the north star. I am somewhat of a stranger at the place where I saw it, and I now find that the point from which it started was as nearly as possible north-east, and about 65° or 70° above the horizon.

I am quite clear as to the path being downwards in an almost perpendicular direction inclining a little to the left. Mr. Mott, on the other hand, describes it as "nearly horizontal, inclined a little downwards about 10° or 12° above the horizon, apparently much foreshortened." It appears to me—perhaps owing to a want of scientific knowledge—quite impossible that a meteor visible a few miles south-west of London, falling as I have described, could be identical with one seen two hundred miles north-west of London travelling in the direction described by Mr. Mott. I of course lay the stress on the direction of the meteor and not the distances of the observers from London.

A. TREVOR CRISPIN

Lansdowne Road, Wimbledon, S.W., August 27

IT may be of interest to some of your readers to know that the meteor mentioned in NATURE as seen on Sunday evening, August 19, was also seen here, timed by me at 10.1 p.m. The compass bearings were from south-east past east to east-east-north, about 35° from horizon; colour, yellow orange; first seen coming from behind a cloud; divided due east, one part falling considerably.

W. M. POOLEY

Bath Road, Cheltenham, August 26

Stachys palustris as Food

I SHOULD be much obliged if any of your readers could give me any information as to whether the rhizomes of *Stachys palustris*, L., are used by the country people either in Great Britain or elsewhere for food. I believe the English name of the plant is Base Horehound, and that in the last century it was so used.

A. WENTZ'L

Krásnicza Wola, Grodzisk, near Warsaw, August 18

OYSTERS, OYSTER FISHING, AND OYSTER CULTURE AT THE FISHERIES EXHIBITION

As long as the English "native" keeps its prominent place in the market all questions concerning oysters and oyster culture will have a special interest for the British public at large. For the man of science oysters are none the less interesting, although from a different point of view. For him it is a great puzzle that up to now we are in so profound a state of ignorance concerning certain of the most important phases of life of a mollusk so exceedingly numerous, which may indeed be called very common, if not always plentiful, all along a large extent of the European coast. Questions such as the following:—Are oysters functional hermaphrodites or not? At what age can oysters reproduce their species? How long do the oyster larvæ (the so-called "spat") swim about in the ocean as free and independent, although minute, living specks? What is the effect of currents and temperature, both upon the growth and upon the fertility of the oyster? are or were up to very lately wholly unsolved, and no really scientific inquiry had thrown any definite light upon them. Even the anatomy of the oyster was very imperfectly known, and it was only last year that the researches of Dr. Hoek, exhibited in the Netherlands department of the Exhibition, threw a flood of light upon this point. These are the first of a more extensive series of investigations which are still in preparation, and which will treat of the embryology and the food of the oyster, the fixation of the spat, and the physical conditions under which the apparently very fertile oyster beds of the Eastern Scheldt are placed. These investigations have been undertaken and pursued for three summers consecutively by the Netherlands Zoological Society.

The exhibit in the Netherlands department is the only one in which the scientific side of the oyster question comes into the foreground. Highly interesting from the point of view of practical oyster culture are, however, two exhibits—one in the Norwegian, one in the British department, which we propose to describe somewhat more at length.

The Norwegian one is a bottle containing oysters from the small lake of Ostrawigtjen, near Soggendahl, on the south coast of Sweden. This lake, which is only about 800 feet long and 500 feet wide, with a depth of about six fathoms, may be regarded as a real "hothouse" for oyster culture, the temperature of the water being at the end of last April no less than 22° C., whereas in winter the water at a depth of three fathoms never registers any lower temperature than 7° C., the average bottom temperature in summer being 27° C. Considering the latitude in which the lake is situated, these temperatures are indeed very remarkable, and have not yet been fully explained. Some would ascribe it to a most luxurious vegetation of Confervæ which is found in the lake, the partial decomposition and fermentation of which might increase the temperature. It is, however, open to question whether this confervoid vegetation must not perhaps be rather looked upon as an effect than as a cause of the high temperature.

This lake incloses a natural bed of oysters, and already *at the end of March some of these oysters contain ripe black spat*. In the summer months the productivity of course greatly increases, and up to November (*i.e.* nearly nine months consecutively) ripe oysters with larvæ in their gills are met with.

It need not be said that this is a splendid collecting ground. The spat is collected on birch twigs which are suspended in the water on wires stretched over it.

In 1882 one thousand collectors were brought out, each having a surface of about sixteen square feet, and on these 730,000 young oysters were obtained, which were than transferred to natural beds in the fjord close to Stavanger.

If the oysters were left where they are they would certainly grow very quickly; oysters of one year sometimes attaining a size of six to seven centimetres. For several reasons, however, the transfer is regarded as more suitable for them.

A most curious fact concerning this lake remains yet to be told, viz. that it is situated 2½ feet above high-water mark; that it is separated from the sea by a dry tract of land with large boulders; and that only between September and March, when the weather is very rough, fresh sea water can gain access to the lake by the sea being thrown across this tract of land. At the opposite side of the lake a small rivulet of fresh water enters the lake.

A second "hothouse" for oyster culture appears to have been discovered only very lately; it is in most respects similar to the first; the depth is about 3½ fathoms; the temperature in October was 18° C., in April 21° C. It is situated at Vesetvig near Stavanger, on the Hardanger fjord.

The British exhibit, to which we would wish to call attention more especially, bears the name of Wootton, Isle of Wight. It consists of models of ponds devoted to the "basin culture" of oysters, essentially different in principle from "foreshore culture," which is at present more successful and more generally recommended.

"Basin culture" is nevertheless perhaps a future stage, when once the acute practical intelligence of the oyster-culturist and the investigations of zoologists shall have succeeded in reproducing the natural circumstances under which the oyster spat lives and thrives. At the conference on oyster culture held by Prof. Hubrecht, we were told that investigations more especially relating to basin culture were at the present moment being carried on in Holland. The result obtained in the Isle of Wight was this, that in the first year (1880) the number of the spat obtained was about 25,000, in the second year (1881) 250,000, and in the third (1882) 1,500,000. Notwithstanding this successful commencement, the final results have as yet remained far below the expectation, since only a comparatively small number could be brought to be marketable oysters. A difference between these experiments and those carried on in the Netherlands, which may perhaps prove to be one of fundamental importance, is this, that no contrivances have been made use of in the Isle of Wight to sufficiently aërate the sea water in the basins. Lack of oxygen may have been the principal cause of the great mortality of the spat. Moreover, a certain amount of sea water was let in and let out at favourable tides, and this must to a certain extent have interfered with the reliability of the results.

These experiments were carried on with oysters that were imported from the Arcachon beds.

The exhibits in the ostreicultural department by Mr. Fell Woods, the well-known director of the South of England Oyster Company (Hayling Island), and by the Whitstable Oyster Company, likewise deserve attention. In both, specimens of shells of one-year-old oysters are shown, the occupants of which are said to have produced "black spat" at that early age. Even if these observations are well authenticated, it is nevertheless recognised that such facts are very rare exceptions, and that generally at three years, and more profusely still at from four to six years, the maximum quantity of ripe spat is produced by the oysters, whose generative organs are most active at that age (cf. Hoek).

France has only a comparatively small exhibit; the implements used in oyster culture at Arcachon are there shown. The Netherlands are represented by a more complete collection, showing both dredging and collecting apparatus, so-called "hospitals," tiles, knives, &c. Models of several oyster parks, partly constructed in old fortifications, and consequently having a very "defensive" aspect, are, moreover, exhibited, as well

as maps and charts showing the way in which the foreshores, &c., are leased by Government to individuals and companies interested in oyster culture. For further details concerning oyster culture in the Netherlands, we may refer the reader to the conference paper on this subject. On the whole oyster culture appears to be very successful in this country.

In the American department there is a large collection of the most various oyster-shells, as well as the model of a vessel occupied in dredging an oyster bed. "Culture" of oysters appears to be very little practised in that country up to the present day, the natural beds being as yet of a nearly inexhaustible richness, especially in the Southern States, where they are principally situated in the lagoons along the coast-line, and the oysters very often used as manure. Nor has the trade in these regions been developed to any extent. More northward Chesapeke Bay is the richest ground, and from thence oysters are transplanted along the coasts of the different Northern States, and at the same time brought into the market in enormous numbers. Together with the scientific investigations in the Netherlands, those in the United States, conducted by Brooks and Ryder, and those of Bouchon Beaudely in France, stand foremost as commendable efforts to bring pure science to bear upon fishery problems of great practical importance.

UNITED STATES COAST AND GEODETIC SURVEY[1]

THE author of this very important treatise states, in his preface, that he has attempted to give a sufficiently comprehensive account of the theory of projections to answer the requirements of the ordinary student of this subject. The literature of projections being extensive—the work of the most eminent mathematicians—the author has contented himself with making such extracts from the great mass of papers, memoirs, &c., which he deemed requisite for his purpose, giving, for further information, references to such original sources as are comparatively easy of access.

As the different conditions which projections for particular purposes have to satisfy are so wholly unlike, no general theory underlying the whole subject of projections can be given; it is therefore conveniently divided into several sections: and here the author mentions his obligations to M. Germain's most important "Traité des Projections" (Paris, 1865), which contains an account of almost every projection that has been invented. At the request of the Superintendent, Carlile P. Patterson, the treatise has been divided into two parts. The first part contains the mathematical theory of projections, while the second part contains merely such a sufficient account of the various projections as will enable the draughtsman to construct them.

The surface of the sphere being non-developable, the exact representation of even a portion of it upon a plane is impossible. Certain conditions can, however, be fulfilled which will render it sufficiently exact for any particular purpose. The areas may be proportionately preserved, in which case we have an equivalent projection; or the angles of small portions may be preserved, in which case we have an orthomorphic projection. The exigencies of any particular use for which a projection is designed give rise to a great number of other conditions corresponding to which projections have from time to time been invented: so that the history of projection has been peculiarly that of the solution of more or less independent problems: for a complete account of which the reader is referred to M. D'Avezac's "Coup d'Œil historique sur la Projection des Cartes de Géographie" (Paris, 1863).

[1] "United States Coast and Geodetic Survey" (Carlile P. Patterson, Superintendent) A Treatise on Projections. By Thomas Craig (Washington: Government Printing Office, 1882.)

The author has treated his subject under the following heads:—

I. Orthomorphic Projection.
II. Equivalent Projection.
III. Zenithal Projection.
IV. Projection by Development.

The first part of the volume treats of the mathematical theory, and is subdivided into nine sections. The first section contains a brief introductory account of the principal properties of conic sections and perspective projection—the most natural and simple method of representation. Sections II. and III. treat of methods of orthomorphic projection. Section IV. treats of projections by development; Section V. gives an account of zenithal, and Section VI. of equivalent projections. Students of these sections are presumed to have a fair acquaintance with the methods of ordinary analytic geometry and the elements of the differential and integral calculus. The next three sections are extremely general, and will require rather more extensive mathematical knowledge. These sections were designed to connect the particular problem of the plane representation of a sphere with the much more comprehensive methods of representation of one surface upon another, and to induce in the student, having a real interest in the general theory, a desire to consult the original memoirs for fuller information.

The second part of the volume, which treats of the construction of projections, does not appear to require any detailed description; but as much of it is merely reprinted from the first part, the propriety of thus separating the "construction" from "theory" seems rather doubtful. The book ends with thirty-one tables, nearly all extracted from the original memoirs of the writers on different parts of the subject of projections. In some cases, however, improved tables by other authors are given. Where the ellipticity of the earth has been taken into account the tables are given unchanged, as the effect of small changes of ellipticity would be almost inappreciable; and, moreover, we have in p. xiii. of the introduction the important statement that "The United States Coast and Geodetic Survey will undoubtedly soon be able to produce a much better value of the ellipticity than has yet been given."

Such are the contents of this valuable book we have endeavoured to describe. It presents, however, some signs of hasty arrangement and want of strict attention to the correction of the press, which will doubtless be removed from the next edition. Indeed the copy under notice would scarcely seem intended for publication in its present form. For instance, "The accompanying plates . . ." mentioned in p. 230 are wanting; and we notice the following typographical errors, &c.:—

Preface, p. x. *Philosophical Magazine*, 1865, should be 1862.

Preface, p. x., and Introduction, p. xiv. There are obvious errors in the title of Gauss's Memoirs.

Introduction, p. xiv. *Phil. Trans.* vol. l. should be vol. L.

P. 80, line 12 from bottom, for pl*a*tting read pl*o*tting.

Pp. 80 and 210. The descriptions of Cassini's projection do not seem to be correct.

Pp. 81, 82, and 210. The woodcuts defective.

P. 83. Curious error in the numerator of the general expression for ρ.

Pp. 67 and 197. Woodcuts of Fig. 13 not good.

Pp. 71 and 201. Fig. 15, woodcuts require correction.

Pp. 76 and 206. Fig. 18, woodcuts not very good.

P. 149. In the denominator of the value of m the power 2 of $(1 \mp \epsilon \cos \omega)$ should be ϵ. In the first term of the denominator of the value of k, $\sin^2 \frac{\omega_1}{2}$ should be $\sin^2 \frac{\omega_2}{2}$, and in the second term ω_2 should be ω_1.

P. 150, line 2. For $1 \epsilon^2 \cos^2 \omega$ in the denominator of the last term of the value of $\frac{dm}{n}$, read $1 - \epsilon^2 \cos^2 \omega$.

P. 214. Fig. 44, the letter P out of place; compare with Germain's Fig. 98; in the letterpress "angle $APC = \omega$" should be $= \pi$.

Also the numbering of the sections seems to require some revision. Section VII. referred to in p. xiii. of the introduction, as containing Mr. Schott's account of the polyconic projection, is not of course the Section VII. of the text, and though Part II. is not divided into sections, yet in p. 230 "The Tables" appear under § xii.

PROMISE AND PERFORMANCE IN CHINESE SCIENCE

UNDER the title of "Science à la Chinoise," a writer in a recent number of the excellent *North China Herald* dwells on what may be called the disparity between the promise and the performance of Chinese science. The ancient classics contain beautiful maxims on the necessity for research into nature. The "Great Learning" tells us that knowledge is perfected by the investigation of nature; Confucius urged his pupils to study the "Book of Poetry," because, among other things, they could become acquainted with the names of plants and animals; Mencius tells us that the careful study of phenomena is the road to knowledge, and in illustration says: "Though heaven is high and the stars distant, yet, having investigated their phenomena, we can sit down and calculate their revolutions for a thousand years." It has long been a proverb among the learned that to be ignorant of a single thing is a disgrace to the true scholar, and to be ignorant of nature is as if nature did not exist. When the revered ancient sages of China, whose words are in the mouths of all, thus encourage scientific research, we should be led to anticipate great results from the patience, intelligence, and ingenuity of the Chinese. But, as in so many other respects in that anomalous country, we have excellent maxims and little more. There is, says this writer, neither research nor knowledge; science has no existence. There is indeed a considerable natural literature. From ancient times the Chinese have taken note of natural phenomena. Their record of solar eclipses is perhaps the most ancient and accurate in the world. They have more or less elaborate works on astronomy, mathematics, botany, zoology, mineralogy, physiology, and many other sciences. Yet there is scarcely any true science in them. Classification, even in regard to plants and animals, there is none. Mineralogy is mainly a description of curious stones. Nor is there any progress, for the more ancient works are generally the best, and as a consequence the Chinese to-day are as their fathers were thousands of years ago. The superstitions respecting natural phenomena, which are as living active truths to-day for all classes in China, remind us rather of man in his state of barbarism than of the ancient culture and civilisation of the Middle Kingdom. The sun and moon are to the Chinese as they were to primitive man, living things, gods to be worshipped. The stars in their courses powerfully influence, if they do not absolutely determine all human events. In them the wise may read as in a book the destiny of man and the fate of empires. Their combinations make lucky and unlucky days, and we shall do well to note carefully their signs and silent warnings. Comets are the precursors of famine, pestilence, and war—prognosticators of the wreck of empires and the fall of kings. Eclipses are the periodic efforts of the dragon fiend to destroy the lights of heaven, and every notice of an approaching eclipse sent by the Imperial astronomer to the provinces is accompanied by a Government order to employ the usual methods of gong-beating and so forth in order to rescue the threatened luminary. Again, thunder is the roar of the anger of heaven, and to be smitten by a thunderbolt is to be marked as a thing accursed. Wind is born in the heart of great mountains, whence it issues at the command of the wind-god. Most districts have their wind-mountains. That at Lung-Shan in the northern province of Chihli is the most remarkable. It has a cave at each of its four sides. The spring wind issues from the cave on the eastern side, the summer wind from the southern, and so for the others. Wind eddies or whirlwinds are raised by the hedgehog in his rapid passage from one place to another, the dust serving to screen him from the vulgar gaze. Rain is produced by the dragon god, who carries up vast quantities of water from the lakes and rivers in his capacious jaws, and pours it down in showers over the earth. Every mountain has its spirit or genius, every valley its nymph, every spring its naiad. Hence mountains and rivers, old trees and curious rocks, become objects of worship.

These and the like superstitions which enter every domain of nature are not confined to the poor and illiterate; they are shared by the rich and learned, nay, they are repeated and acknowledged by the Imperial Government itself in its decrees in the *Peking Gazette*. The highest scholar in the empire knows no more of nature than the humblest peasant. The years have come and gone, repeating the same old story, but there has been no ear to hear it, no mind to understand it. Nature has found no interpreter among the Chinese; during their long national life they have contributed nothing to science. How are we to account for this? In other fields of national effort, and especially as inventors, they must be allowed a high place. It cannot be indifference, for they have written largely on the beauties, marvels, and mysteries of nature, and many have shown keen interest in the discoveries of science. It may partly, perhaps, be due to the fact that the intellect of the nation is employed in the struggle for place and power along grooves in which science has no part. The writer we quote thinks it is mainly owing to the narrow and perverted system of education; and while the present system continues the study of science will be impossible to the youth of China. The cleverest young men find it as much as they can do to take their first degree at twenty. The higher degrees, which are also the avenues to office, can scarcely be won for years later, and thus they cannot afford a thought for anything beyond the common curriculum.

ON THE PROPERTIES OF WATER AND ICE[1]

DR. PETTERSON'S memoir is a most valuable contribution to our knowledge of the natural history of the waters of the globe. Every reader of Arctic voyages must be familiar with the variety of names attached to the different kinds of ice met with in these regions, such as "pack-ice," "bay-ice," "brash-ice," and the like. To one who has never seen them, the names convey very little information either of their appearance or of their mode of formation. Dr. Petterson's paper explains in a satisfactory and very remarkable manner the nature of the difference between the different kinds of ice.

In the first part of the work the subject is treated physically, and in the second chemically. In both parts there is much that is new and valuable.

In the Arctic Ocean, and especially in that part of it visited by the *Vega*, the saltness of the water varies much from place to place. The large rivers of Siberia constantly pour forth fresh water which lies on the surface of the ocean and spreads round the coast like a fringe. This layer often extends a considerable distance out to sea, where it gradually thins out. Nearer the shore it is thicker, but wherever the depth exceeds 20 or 30 metres the dense ocean water is found below and the two layers

[1] "On the Properties of Water and Ice." By Otto Petterson. Publication of the *Vega* Expedition. (Stockholm, 1883.)

persist without sensible mixture. As an example may be cited some observations made on board the *Willem Barents* in the Kara Sea on August 3, 1881:—

Depth. fathoms.	Temp. °C.	Density.
0	+8·2	1·006
1	+6·2	1·009
2	+1·7	1·020
3	−1·0	1·0236
5	−1·5	1·0247

Here, while we have what is practically fresh warm water at the surface, and to a depth of a fathom from it, at two fathoms we have cold Arctic Ocean water. Looking therefore to the great variety in the composition of the waters exposed to the winter cold and therefore likely to produce the ice met with in Arctic regions, Dr. Petterson has studied separately the change of heat and volume by the freezing of (1) pure water, (2) brackish water of little saltness, and (3) ocean water of ordinary saltness. With regard to the freezing of brackish or salt water no previous investigations of a quantitative character exist, and the author's results are all new. With regard to the freezing of pure water the most important investigations were those of Plücker and Geissler. While verifying their result as to the *average* coefficient of dilatation of ice, the author made the important discovery that the volume of ice *decreases* as the temperature rises, in the vicinity of the melting point. In extending his researches to brackish and salt waters he found this anomaly more and more accentuated the more salt was contained in the ice formed. Rightly seizing the importance of this very remarkable observation the author makes the behaviour of pure ice in the vicinity of its melting point one of the main objects of the investigation. The "dilatometer" used was a glass vessel of peculiar construction and of a capacity of 41 cubic centimetres. The water to be experimented on was frozen in it, so that it formed a cylinder of ice surrounded by mercury, which extended also into a capillary tube and indicated changes of volume. As the accuracy of the results depends, amongst other things, on the correctness of the determinations of the absolute dilatability of mercury; and as this is somewhat uncertain, and indeed variable, at low temperatures, the author adopted the device of Plücker and Geissler for producing a practically undilatable envelope for his experimental substance. The principle of it is very simple. The envelope is of glass with a coefficient of expansion 0·000028; that of mercury is 0·000181. If the volume of the glass envelope is to that of the mercury contained in it in the inverse proportion of their coefficients of expansion the residual volume will be constant even though the temperature vary. If the volume at 0° C. of the glass be 18·1 cc. and that of the mercury 2·8 cc the residual volume is $18·1 - 2·8 = 15·3$ cc. If the temperature is $t°$ the volume of the glass is $18·1 (1 + 0·000028t)$ and that of the mercury $2·8 (1 + 0·000181t)$, and the residual volume is $v = 18·1 - 2·8$ as before. The effect of variation in the coefficient of expansion of the mercury is thus reduced to a minimum

When a cylinder of ice had been frozen in the instrument, it was immersed in a mercury bath, and subjected to variations of temperature, either with freezing mixtures, or, in winter, by exposure to the atmosphere.

These series of experiments were made with distilled water. The first series was made with water taken from the store jar in the laboratory. It gave a slight opalescence with nitrate of silver, and cannot therefore claim to have been pure. The ice formed by its congelation expanded with rise of temperature from −20° C. to −0°·3 C. Here it began to contract until it melted at 0° C. Two other series of experiments were made with water repeatedly distilled. The ice from it did not begin to contract till the temperature had risen to −0·03° C.

There can be no doubt, especially in view of later experiments with brackish waters, that the not chemically pure distilled water did contract at a measurable distance from its melting point. With regard to the other two samples, the temperature at which the ice began to expand with heat is so close to its actual melting point, that it is impossible to have implicit reliance in the result claimed. The author's own view will be best judged from the following paragraph (p. 282):—

"It is impossible to decide if absolutely pure water would be entirely free from this weakness or not, since we cannot assume that water which has boiled for a quarter of an hour or more in a glass vessel is absolutely free from minimal quantities of foreign substances as, for example, sodium salts, silica, &c. For my own part I am rather inclined to think that absolutely pure water, if it could be tested, would show an absolutely fixed melting point, but I think that this problem very much resembles another question still undecided, viz. is absolutely pure water a conducting or non-conducting substance for electricity?"

It would be well to repeat the experiment with pure freshly distilled water, freeing it from air by boiling *in vacuo*, which Dr. Petterson's apparatus would easily admit of. There would then be very much less risk of the glass being attacked.

Experiments made with sea-water ice proved that the property of contracting with heat, as the melting point is reached, becomes more and more marked the greater the quantity of salt in the ice. Three series of experiments were made. In the first, the ice when melted had a specific gravity of 1·0003, and contained 0·014 per cent. chlorine. It began to contract at −4° C. The second had a specific gravity of 1·00534, and contained 0·273 per cent. chlorine. It began to contract at −14° C. The third had a specific gravity of 1·0094, and contained 0·649 per cent. chlorine. It was contracting at the lowest observed temperature, −19° C.

In connection with these remarkable results it must be mentioned that at the same temperature, as, for instance, −15° C., the volume of the ice which on being melted furnishes 1 cc. water at 0° C. is less the greater the amount of salt contained in it. Sea water being an exceedingly complex body, it is to be hoped that Dr. Petterson will extend his research so as to examine in the same direction the ice formed by simple solutions of each of the more important ingredients of sea water. How different ice produced by the freezing of sea water must be from what we are accustomed to see on our lakes and ponds in winter, will be evident when we read (p. 286):—". . . The new ice which arises by sudden freezing of the calm surface of the Arctic sea is a *tough* substance, which can be wrinkled and folded by external pressure without breaking. Although it may be thick enough to bear the weight of a man, it is so plastic that a footstep makes a deep impression as in mouldable clay."

The physical part of the work closes with the investigation of the latent heat of fusion of fresh and salt ice. The result is that "the latent heat developed by the freezing of sea water is *extraordinarily inferior to that of pure water*."

Hardly less interesting than his physical experiments, are the investigations into the chemical composition of sea water ice.

It has been very generally believed that sea-water ice owes its salinity to mechanically entangled brine, and that all that is really solid in it is pure ice. Scoresby, probably the most acute observer amongst Arctic voyagers, referring to this subject, says:—[1]

"Although I have never been able to obtain from the water of the ocean, by experiment, an ice either compact, transparent, or fresh, yet it is very probable that the retention of salt in ice may arise from sea water contained in its pores; and, in confirmation of this opinion, it may be stated that if the newest and most porous ice be removed

[1] "An Account of the Arctic Regions," Edinburgh, 1820, vol. i. p. 230.

into the air, allowed to drain for some time in a temperature of 32° or upwards, and then be washed in fresh water, it will be found to be nearly quite free from salt, and the water produced from it may be drunk."

During the Antarctic cruise of the *Challenger* the writer of this notice made some experiments to decide the question whether or not sea-water ice is a mixture of pure ice and sea water or brine. The *melting point* of salt-water ice of various sources was carefully observed, with the following results. Ice formed in a bucket of sea water over night melted at −1·3° C. The bulk of ice formed was insignificant compared to the volume of water in which it was formed, so that this was a specimen of *bonâ fide* sea-water ice, without admixture of snow or spray. In the same way the melting point of pack-ice was determined. The freshly collected ice began to melt at −1° C.; after twenty minutes the thermometer had risen to −0·9°, and two hours and a half afterwards it stood at −0·3°, having remained constant for about an hour at −0·4°. Another portion of the ice rose more rapidly in temperature, and when three-fourths of the ice was melted, the thermometer stood at 0° C. In the case of the ice frozen in the bucket, the melting point remained constant for twenty minutes at −1·3°, after which no observations were made, so that we do not know if this ice, formed under the most favourable circumstances, showed the same irregularities as the pack-ice, picked up out of the sea; but as the bulk of ice experimented on did not much exceed 10 cubic centimetres, the greater part of it must have melted in the twenty minutes. Indeed as the amount of ice formed in the bucket did not sensibly alter the composition of the water left liquid, there seems to be no reason why the ice should not be a homogeneous substance.

Adhering brine can have no influence on the melting point of ice, consequently, if sea-water ice consists of pure ice with entangled brine, it must melt at 0° C. If its melting point is different from 0° C. then the solid matter of the ice is not pure ice. We have seen that frozen sea water has a melting point of −1°·3, which is fairly constant, and that pack-ice, which must necessarily be formed by the freezing of salt water, the congealing of spray, and the accumulation of snow, begins to melt about −1°, the temperature gradually rising as the constituents of lower melting point are liquefied. It is thus readily apparent how it is that Scoresby found that such ice "allowed to drain for some time in a temperature of 32° or upwards," produced in the end potable water. The salt-water ice of low melting point effectually prevents the intermingled snow from melting, which finally remains practically intact, and of course can be drunk on being melted.

Dr. Petterson on purely chemical grounds comes to the same conclusion. He says (p. 303): "Those who support the common theory that sea ice is in itself wholly destitute of salt, and only mechanically incloses a certain quantity of unfrozen and concentrated sea water, must confess that we in this case ought to find by chemical analysis exactly the same proportion between Cl, MgO, CaO, SO_3, &c., in the ice and in the brine as in the sea water itself." That this is not the case is shown by a number of analyses of sea-water ices in which the proportion of Cl : SO_3 varied from 100 : 12·8 to 100 : 76·6, the average proportion in sea water being 100 : 11·88. The results of his investigations may be summarised as follows:—

Ocean water is divided by freezing into two saliniferous parts, one liquid and one solid, which are of different chemical compositions. Taking the relation Cl : SO_3 as standard of comparison, the most striking feature of the freezing process is that the ice is richer in sulphates, and the brine in chlorides. The extraordinary variation, both in saltness and in chemical composition of every individual specimen of sea ice and sea brine, shown by the tables, depends on a secondary process, by which the ice seems to give up its chlorides more and more, but to retain its sulphates. Hence the percentage of chlorine is no indication of the saltness of the ice, though it may to a certain extent be taken as an index of its age.

In connection with this part of the subject, the author cites Prof. Guthrie's work on Cryohydrates, and gives the following table:—

Cryohydrate of	Contains	Solidifies at
NaCl ...	76·39 per cent. water ...	−22° C.
KCl ...	80·00 ,, ,, ...	−11°·4 C.
$CaCl_2$ ...	72·00 ,, ,, ...	−37° C.
$MgSO_4$ ...	78·14 ,, ,, ...	− 5°·0 C.
Na_2SO_4 ...	95·45 ,, ,, ...	− 0°·7 C.

Supposing that these cryohydrates are formed in the freezing of sea water, it is easy to see how, as the temperature rises, the chlorides melt out first and leave the ice richer and richer in sulphates.

Before concluding this notice, attention must be called to a statement in a note at the foot of p. 318: "As a thermometer immersed in a mixture of snow and sea water, *which is constantly stirred*, indicates − 1°·8 C., we may regard," &c. This can be true only if the temperature of the atmosphere is − 1°·8 C.; if it is 0° C. or higher, the temperature of the sea water will assuredly rise to the melting temperature of snow, or 0° C.

Even though it should turn out that chemically pure ice does, as the author suspects, melt suddenly without previous contraction *as ice*, the discovery of the existence of a *minimum density point* of ice, not chemically pure, which includes all the ice on the globe, is one of the very highest importance.

It is to be hoped that we shall soon have a further instalment of work on a subject so large and so important, and with which the author has shown himself so well qualified to deal. J. Y. BUCHANAN

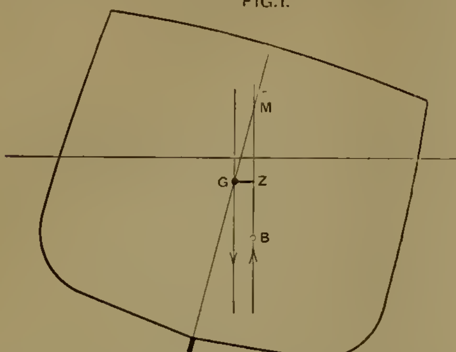
THE STABILITY OF MERCHANT STEAMSHIPS

I PROPOSE to state, and in part to restate, the more important scientific considerations concerning the stability of merchant steamships which the investigation of the *Daphne* disaster has brought to light, following the main lines of the second part of my Report, which has been published *in extenso* in several newspapers. In this case, as in all cases touching the complicated question of ship stability, it is very necessary to be careful not to draw hasty inferences or any inferences at all which are not strictly deducible from the facts or principles established.

It is desirable to guard the reader in the first place against considering the cases of the ships *Daphne* and *Hammonia*—which I have had occasion to associate somewhat closely in my Report—as identical in more than a certain number of features, there being other features in respect of which there is little or no resemblance. I will presently point out both the resemblances and the differences, but first let me remind the reader unfamiliar with naval science what is meant by a curve of stability, quoting the Report as far as may be necessary for the purpose. Fig. 1 may be taken as the transverse section of a vessel inclined at an angle of 15 degrees to the upright. The total weight or gravity of the vessel will act downwards through the centre of gravity G, and the total buoyancy will act upwards through the centre of buoyancy B, as the arrows indicate. It will be obvious that the vessel cannot rest in the inclined position with these forces and no other operating upon her; she must revolve until gravity and buoyancy act in the same vertical line, but in opposite directions. The further she is inclined the more will the ship be immersed on one side and emersed on the other, and therefore the further out will the centre of buoyancy move. Now as neither the gravity nor the buoyancy need be altered in amount by mere inclination,

and as they are equal and opposite in direction, it follows that, whatever the inclination, the force acting will always be the same, but the leverage, marked G Z, will vary as the centre of buoyancy moves. At 30 degrees inclination, for example, G Z is much greater than it is in Fig. 1 at 15 degrees. In Fig. 2 these lengths are set up as

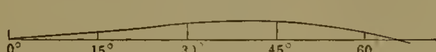
FIG. 1.



ordinates of a curve, and similar lengths for inclinations of 45 and of 60 degrees are similarly set up; the curve drawn through their upper extremities is this vessel's "curve of stability," observing that the base line is divided into equal lengths for equal angle intervals on any convenient scale.

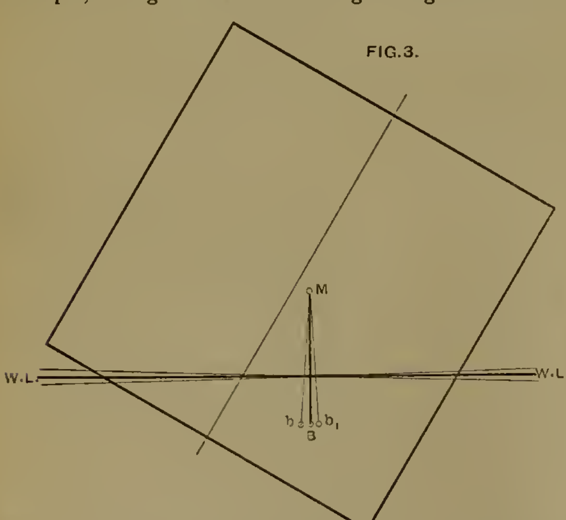
As regards the "metacentre," I must explain here, as

FIG. 2.



I did in my Report, that in former times, when "initial stability" alone was calculated, the word "metacentre" had a much more limited meaning than it possesses now. It formerly had relation to the upright position of the vessel, in which case the buoyancy acts upwards through the centre line of the ship's section—along G M, for example, in Fig. 1. After receiving a slight inclination,

FIG. 3.



the vessel has, as we have said, a new centre of buoyancy, and the buoyancy itself will act upwards along a fresh line slightly inclined to what was previously the upright line, and will intersect it at some point, M. This point was called the "metacentre," and if we suppose the angle in Fig. 1 to be very small (very much less than 15 degrees), then the M shown there approximately marks the "metacentre." When a ship is much more inclined, the point at which two consecutive lines of the buoyancy's upward action will intersect may not be, and often will not be, in the middle line of the ship at all, but this point is nevertheless called the "metacentre," and the use of the word in t is extended sense has recently become general. In Fig. 3 is shown a floating body of square section, inclined in the water at an angle of about 30 degrees. W L is the water line or line of flotation; B is its centre of buoyancy. By giving it a "slight" inclination from the position, it will of course have a new centre of buoyancy given to it. If we incline it one way *b* will show this, if we incline it the other way *b′* will show it, and for each of these positions there will be a new line of action or buoyancy. But these lines of action, together with that through B, will all meet or intersect in one point, and this point (M) will be the metacentre at 30 degrees of inclination. In Fig. 4 I have shown curves of stability for a prismatic body, with the centre of gravity in the centre of form, and also with that centre in some cases raised and in others placed below the centre of form. In this figure the draught of water is taken at 3/25ths of the total depth of the prism. In Fig. 5 I have given curves of stability for the prismatic body with the centre of gravity and the centre of form taken as coincident, but with different draughts of water. In Fig. 6 I have given the curve of stability of a similar prismatic body, immersed 2/5ths of its depth, and having its centre of gravity situated 6 inches below its metacentre. These figures serve to illustrate very clearly the error involved in the assumption that with stability at the upright position and stability at 90 degrees—or but little instability at the latter, which is what some authors have instructed the profession to be content with—there need be no apprehension of any deficiency of stability at intermediate angles of inclination. They show that with square sections and prismatic forms there may be various dispositions of centre of gravity and draughts of water, with which stability in the upright position and again at 90 degrees are not proofs of safety, but indications of the gravest danger.

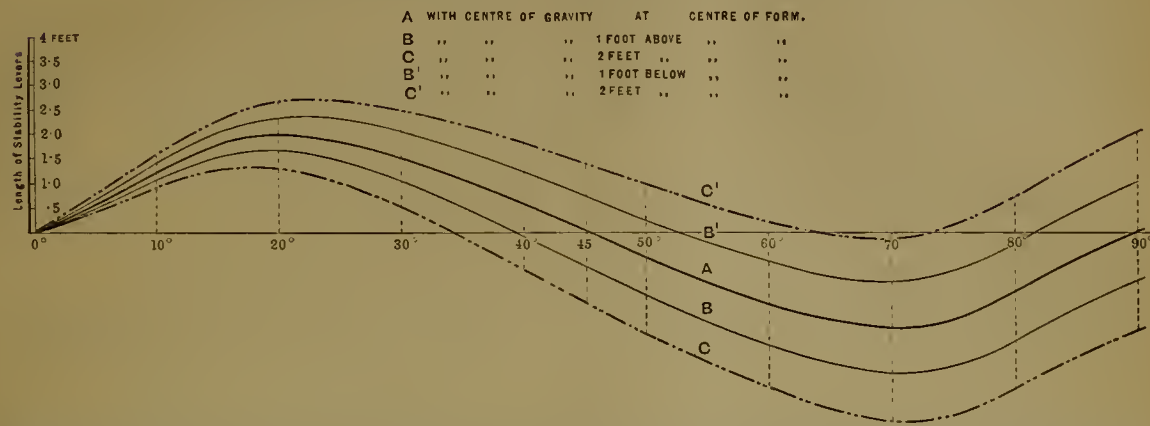
With these figures before us, we now have both the *Hammonia* case and the *Daphne* case amply illustrated, and can carefully distinguish between the two. The *Hammonia* case—as put forward by Mr. Biles, who conducted her calculations—is that of a high-sided vessel with her stability reaching a maximum soon after she had inclined 30 degrees; and she therefore finds her analogy in one or other of the cases shown in Fig. 5. In the latter figure it will be seen that with the centre of gravity in the centre of form all positive stability vanishes at an inclination of 45 degrees in the two cases A and B; but the *growth and decline* of the stability are very different indeed at the different immersions. When the immersion is smallest the stability rises in a steep curve (A), attaining a comparatively large maximum something under 20 degrees, and then declines, more gradually than it rose, as the inclination goes on. By increasing the immersion from 3/25ths to 5/25ths the curve B is produced, and here we see a vast change of stability, the curve, which rises very slowly from the base line, never reaching one-fourth the maximum ordinate of curve A; only attaining its maximum beyond 30 degrees of inclination, and then declining less slowly than it rose, until it vanishes. Immerse the body to double the last immersion, and we find in curve C that now, instead of vanishing at 45 degrees, the stability only there begins, rising to a small maximum a little beyond 60 degrees and vanishing at 90 degrees. It is in curve B that we find a state of things very closely analogous to that disclosed by the *Hammonia* curve, which I now give in Fig. 7. In both cases the stability increases but slowly; in both it reaches early a maximum; and in both it disappears altogether before the vessel is more, or much more, than

inclined through half a right angle. The case of the *Daphne* resembles this in the slowness with which the stability increases as the vessel is inclined, this slowness being due to the same causes in both cases doubtless; but an examination of the triple-branched curve of her stability given in Fig. 8 shows that the analogy between the two cases ends at quite a moderate angle of inclination, say 30 to 31 degrees. In this figure (8) the curve A

FIG. 4.

CURVES of STABILITY of PRISMATIC BODY of SQUARE SECTION

DRAUGHT of WATER $\frac{3}{25}$ths of DEPTH.

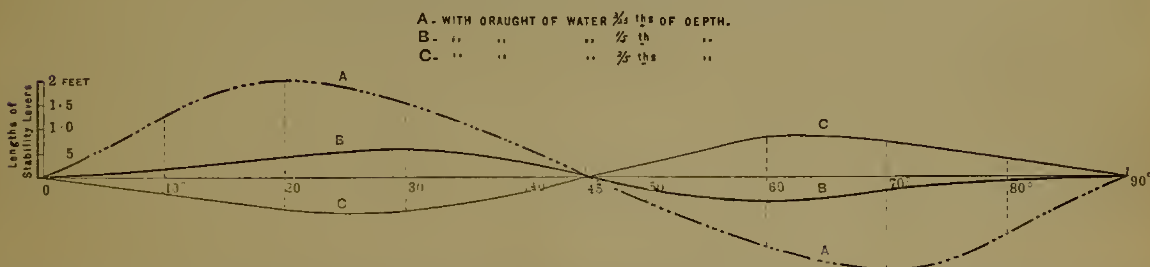


is constructed on the assumption that the ship was free to take water on board as the main deck became immersed; the branch B presumes the poop to have been watertight; and the branch C is calculated to show how the stability curve would have increased until the bulwarks came under water, provided these bulwarks had been watertight. It will at once be seen that the *Daphne* cannot be regarded as analogous to the *Hammonia* or to the curve

FIG. 5.

CURVES of STABILITY of PRISMATIC BODY of SQUARE SECTION

WITH CENTRE OF GRAVITY AT CENTRE OF FORM.



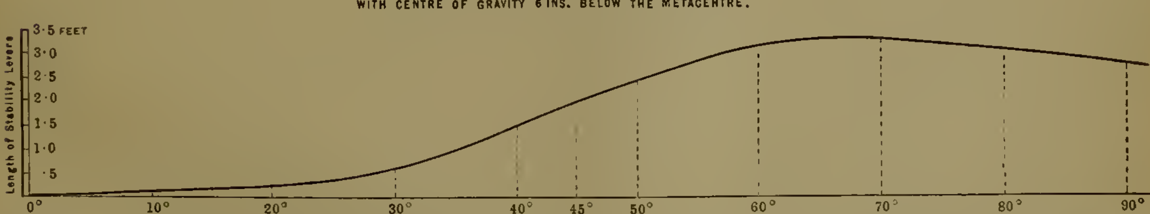
B in Fig. 5, in so far as the stability at very large angles is concerned. On the contrary she would have more resembled the case of Fig. 6, provided her sides had gone as high as her topsides and been there decked over. The *Daphne's* curve A ceases to rise soon after the main deck becomes immersed, and then falls rapidly away in the same manner and for the same reason as all ships lose stability when, or soon after, the freeboard has become

FIG. 6.

CURVE of STABILITY of PRISMATIC BODY of SQUARE SECTION

DRAUGHT of WATER $\frac{2}{5}$ths of DEPTH.

WITH CENTRE OF GRAVITY 6 INS. BELOW THE METACENTRE.



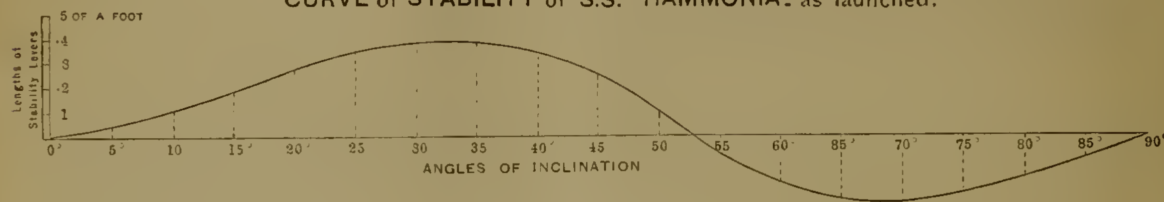
exhausted. It must therefore be clearly understood that it is in the early stages of the two curves that the cases which I have had to make public find their resemblance; at the later stages the *Daphne* illustrates the consequences of the immersion of the deck, while the *Hammonia*, by losing all stability before the deck became immersed, opened up a state of things which startled her builders, surprised the profession, and confounded the text-books, and

must force extended calculations upon all those who hereafter undertake to launch ships upon the stability of which any doubt can by possibility exist.

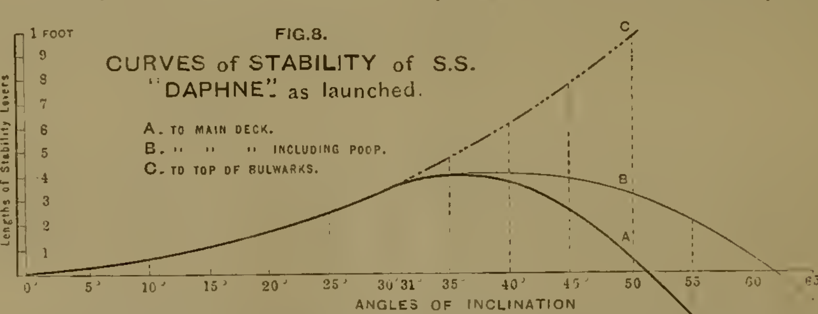
It is pretty widely regarded as a remarkable fact that there should have been any deficiency in the knowledge of shipbuilders concerning the conditions or possible conditions of the stability of ships at their launching draughts. But to me this deficiency seems the most natural thing possible. It needs no explanation to those who remember what immense transformations and extensions have come upon the shipbuilding trade during even my own professional experience. I well remember looking with wondering interest in Sheerness dockyard at the first iron ship ever seen there; and yet the construction of iron ships had become so universal fifteen years ago that I wrote my work on "Shipbuilding in Iron and Steel" to meet a widespread necessity, the idea of writing descriptions of wood ships having already passed away. I equally well remember the building at Sheerness of the first screw steamship ever constructed there; but where now are any but screw steamships built for ordinary ocean work? Some sailing ships and some paddle steamers doubtless are built even now; but the screw steamer has almost undisputed possession of the world's ocean trade. With these changes have come in wholly new developments of shipbuilding science, and the present is not by any means the first instance in which it has fallen to my lot to point out errors of doctrine—false deductions from former practice—which were misleading the shipbuilder. In the case of the strains to which ships

FIG. 7.

CURVE of STABILITY of S.S. "HAMMONIA" as launched.



are subjected, the deductions made by the most eminent men who discussed the subject scientifically at the end of the last and the beginning of the present century seemed to me to be irreconcilable with the conditions of modern ships, and after lengthened investigations I found that they were not only wrong, but in some cases the reverse of the truth, and I contributed to the Royal Society a paper on the subject which has brought modern theory and modern practice into better relationship. In the matter of stability it was most natural that as we abandoned the employment of wind as our propelling power—which of course imposed upon ships the necessity for large stability to withstand the wind-pressure—shipbuilders were able to resort to greater proportionate length and to enlarged proportionate area of midship section; and thus to bring about conditions in which large initial or early stability, so to speak, fell out of demand. Nor is it easy to say when deficient stability would have come under close investigation, had it not been for the accident of certain ships of very *low freeboard* coming under consideration at the Admiralty, as explained in my Report. These led to the calculation of stability at successive angles of inclination, and to the method of recording the results in the form of the "curve of stability" previously described. But besides the *change* of the seagoing ship, there has been the enormous extension of its employment, our carrying trade on the sea having increased by leaps and bounds. Every one knows that when the demands of commerce are very urgent, science and scientific research are apt to be neglected. The necessity for great carrying power and speed at sea has been attended by an equal necessity for



quickening the loading and discharging of ships in port, and consequently steam windlasses and cranes, and many other modern appliances involving upper weight have come into vogue, and their effect upon stability has not been always considered. From these and other causes there has been brought about that somewhat extensive employment of ships of small stability, or of no stability at all in themselves, to which it lately became my duty to direct attention. It is no doubt the general belief that a high-sided ship having some initial stability, will, as she inclines, gather large additional stability, and will retain some even at very large angles; that, as my Report states, has greatly encouraged people to be satisfied with very small initial stability, in some cases with none at all, and even less than none. Many steamships of large tonnage have been built of late years for influential steam companies and other owners, which ships are totally incapable of floating upright without the aid of ballast or of cargo, and which cannot be unloaded in dock without being held upright with hawsers attached to the shore. Such ships, even when capable of floating unballasted without capsizing, can only do so by lolling over at large angles of inclination, and there finding a position of stable equilibrium. When carefully watched over and stowed with suitable cargoes, these ships can usually be made safe at sea, and sometimes even safer than ships with larger initial stability but less range—a circumstance to which undue prominence has perhaps been given, and which has diverted many from the grave elements of danger which more often are associated with small initial stability. "There is not the least doubt, however, that a very small initial stability given to many modern mercan-

tile steamships—given in the belief that much more is sure to be gained as the ship inclines (within large limits)—has resulted in the capsizing of many ships at sea, and in grave danger to many that are still afloat, not in the same manner, because not in the same condition as to lightness as the *Hammonia* and *Daphne*, but from other not less real deficiencies." Sad and serious as this statement is, I repeat it here with perfect confidence in its accuracy.

Sometimes such vessels are brought into a condition of apparent safety by the stowage of their own coal, but as the coal is consumed their stability diminishes, they capsize, disappear, and the word "missing" is recorded against them in an official return. No means exists, notwithstanding all our shipping legislation, for insuring that the facts will be brought to light—indeed, at the official inquiry which follows under the present conditions, the question of stability may not even be mentioned. As the stability of a ship is often an intricate matter which can be effectually controlled only by close and careful calculation, and as no Government department is at present charged with the duties even of collecting, recording, and making known those dimensions and particulars of ships which determine their stability, the matter must be left to right itself. Maritime ships of small stability incur dangers from, and are doubtless lost by, the operation of causes which are but very imperfectly appreciated.

It is under the urgent pressure of a very rapidly growing mercantile steam marine that the shipbuilding trade has somewhat, I fear much, outrun the companionship and regulation of science. It is only quite recently that the necessity for developing their scientific staff and appliances has been borne in upon the minds of shipbuilders. There never, even yet, has been so much as a training school or college established by them for the education of young naval architects and draughtsmen throughout the country. But the Admiralty have had their dockyard schools at work for nearly forty years; school after school of Government naval architecture has been established; the Institution of Naval Architects has been formed, and done invaluable work, for more than twenty years; and some private shipbuilders have at length entered with spirit and enterprise upon the labour of developing the practice of scientific naval architecture. No part of my duty in connection with the *Daphne* inquiry has been so agreeable to me as that of bearing witness to the admirable efforts of several Clyde firms in this respect; and there is no result that can follow from the inquiry which I should esteem so highly as the emulation of their efforts throughout our shipbuilding establishments generally—unless, indeed, it were that of a general awakening of *shipowners* to their great and enduring responsibilities in this matter.

EDWARD J. REED

INTERNATIONAL POLAR RESEARCHES

AT the present moment, when every student of modern science is anxiously awaiting the result of the labours of the international observation parties which have for nearly a year been self-imprisoned around the Pole, I venture to make the following suggestions relating to international Polar researches.

The state of the ice in the Arctic seas is, as is generally known, very changeable during various seasons. It is thus impossible beforehand to draw conclusions as to the probable state of the ice one summer by its state the year before, and this circumstance has greatly impeded active researches in the Arctic regions. From time to time valuable and expensive expeditions have been despatched, but these have in most instances been unfortunate enough to encounter the adverse seasons, and the purely geographical gain has in consequence not been in proportion to the cost. At other seasons, on the other hand, when the ice seems to have promised a far advance northwards there has not been any expedition ready to take advantage of the circumstance. Had there at certain times and seasons been expeditions prepared to use the opportunities which have presented themselves, and in the right locality, I have not the least doubt that a very far advance into unknown Polar regions might have been made at a very small cost. In spite of the, in many respects, exceedingly valuable discoveries which have resulted from these expeditions to geology, meteorology, and other modern sciences, they seem certainly on the whole as if they had been started under an unlucky star, which is, in my opinion, caused by the circumstance *that the period and season selected have not been the proper ones*. What we have thus gained has generally been obtained with great loss of time, money, and valuable lives. A most remarkable contrast to this is, however, the voyage of the *Vega*, which from beginning to end seemed to have been attended with success only, as the forced wintering, when having practically accomplished its object, only tends to heighten the charm of this venture.

From the experience we have gained of the changes in the ice, it is however evident that Polar researches have hitherto, in one respect at all events, been effected in an erroneous manner, and great loss of money and life caused thereby. The *geographical* researches around the Pole should in my opinion be conducted in a different manner. Instead of, as has hitherto been the case, that finely equipped expeditions are despatched at random and at unconsidered periods, an arrangement should be made between the various European nations to equip a certain number of expeditions, which should be despatched *every* summer to the *same* locality *during a period of ten to eleven years*. During a period of this length it is probable that the conditions of the ice, which we may assume undergo periodical changes, have run their cycle, and during certain years of such a period opportunities would undoubtedly occur which would enable a very far penetration into the Polar basin.

The expenses attending such expeditions would, if skilfully arranged, not exceed those of one of the costly ones which have hitherto been despatched, while they would not result in the great loss of life which seems to attend the larger one or two years expeditions under which ambition naturally leads the members to venture on any undertaking which may give returns equivalent to the expectations of the equippers.

Hitherto the Dutch alone have arranged their expeditions to the Polar regions in a systematic manner. They have, as is generally known, for some years regularly despatched an expedition every summer to the regions around Spitzbergen and Novaya Zemlya; but that they have not, geographically, obtained any great results may be ascribed to the circumstance that they have employed sailing vessels instead of steamers. Neither have they in all probability laid special stress on geographical achievements in these parts; the expeditions hitherto despatched may thus be considered as mere pioneering ones. From next year it is, however, the intention of the Dutch to employ a steamer instead of a sailing vessel, and then their researches will, no doubt, be more fruitful.

It is now admitted by every student that Polar researches are of great importance in several respects, and the establishment of the international circumpolar stations is a proof of this, while the manner in which these have been arranged seems to promise to be the first step towards a series of researches in the Arctic regions, which would, as the meteorological ones, be best carried out through an international cooperation. In order to advance in the unknown Polar basin, it appears to me to be essential to abandon the random expeditionary attempts hitherto persevered with, and organise instead systematic researches. And if these are carried out by international

cooperation, the levy on the individual participators would be very small indeed.

There are in my opinion three points in the Arctic seas which offer, I believe, special advantages as bases for penetrating towards the Pole, and on which particular attention should be concentrated, viz. the north of Spitzbergen, the north-east of Novaya Zemlya, and the Behring Straits.

To the north of Spitzbergen, *i.e.* to the north of the Seven Islands, Norwegian hunters have, in the autumn of certain years, found the sea to the north and north-east so free from ice that they have deemed it a very easy matter to have penetrated with a steamer considerably northwards. Such was, for instance, the state of the ice in the autumn of 1881. And similarly the sea to the north-east of Novaya Zemlya has in certain years been easy of navigation, and finally, judging by researches, it may be assumed that the same is the case with the sea north of the Behring Straits.

Now, in order to carry out the programme which I have here suggested for a more systematic research of the Polar regions, I advocate that four small but excellent steamers should be provided, of which one should every year be despatched to a station on the north coast of Spitzbergen, another to one at the northern point of Novaya Zemlya, and the remaining two to respective stations north of the Behring Straits. This should be carried out during eleven consecutive years. Then when the state of the ice in certain seasons was very favourable, the vessels should take advantage of the opportunity and proceed northwards.

The advantage of this plan is that it would be attended with very little risk, while the object should be not to attempt to force an advance, but rather to wait patiently until the favourable opportunity presents itself, and then to act with boldness and decision. There is on the other hand every reason to assume that the time of the members of these expeditions would be employed throughout in a way beneficial to science. As a matter of safety it would also be advisable to establish fixed stations or depots in suitable places, to which the expeditions could resort in case of need.

From the experience we have gained of late it may be safely assumed that the Polar basin is not during any whole summer or autumn covered with continuous ice; it is in fact evident that the sea shows large tracts of open water during these seasons. The ocean ice north of Spitzbergen is thus always in a constant—at times even exceedingly violent—state of drifting in the most varied directions, according to the currents and winds prevailing. At times, too, the ice has been found to drift in a direction contrary to those of currents and winds. North of Spitzbergen there must, therefore, during certain periods of the season be large tracts of open water which are capable of receiving the enormous ice masses in drift.

As is generally known, Petermann advanced the hypothesis that Greenland extended in a more or less broad belt of land towards the Pole, from whence it diverged downwards to Behring Straits. If this is so, the great Polar basin should be divided into two parts with a common outlet into Behring Straits, although distinctly separated from each other by the land belt in question. They would at the other end discharge themselves into two different channels, viz. one in Baffin's Bay and the other in the Greenland and East Spitzbergen ocean. This hypothesis has been supported by many eminent *savants*, as for instance Parpart, Jäger, and Chavanne.

Without, however, disputing the correctness of the reasons for this assumption, it would not be difficult to point out circumstances which would refute the hypothesis. And although several things seem to corroborate the assumption that the real Polar basin contains a belt of smaller and larger islands, it is perfectly obvious that the climatological and consequently the glacial conditions of these regions would have been quite different from those now prevailing had a large continent of the kind described by Petermann occupied the greater portion of the central Polar basin. I myself believe, judging by the strong motions of the ice north of Spitzbergen and Novaya Zemlya, and certain circumstances attending the same, that the climate of the Polar regions is a sea or insular climate rather than a continental one. In making this assertion, however, I do not say that a continent such as that referred to has not existed there in the Tertiary or early part of the Quaternary period.

However this may be, the question to be solved is one of preeminent importance to men of science, and I feel certain that a mode of research effected in the manner I have here advocated would certainly result in its solution.

KARL PETTERSEN

Tromsö Museum, July

NOTES

A MEETING of the General Committee of the International Fisheries Exhibition was held at South Kensington on Tuesday. Mr. Birkbeck presided, and read the Report of the Executive Committee, which stated that the number of visitors to the Exhibition has, up to the present, been very large. The numbers up to Saturday, the 25th inst., were 1,444,515, showing a daily average of 16,050. The juries have, with few exceptions, now completed their labours, and their reports will be laid before the Special Commissioners, appointed by Her Majesty's Government, for consideration and approval. The Report closes as follows:—"With regard to the future, it is indispensable that the Executive Committee should obtain the necessary powers from the General Committee to announce the closing of the Exhibition on some day to be fixed hereafter, and that they should further be invested with authority to carry out any negotiations and make any agreements they may deem necessary for the subsequent utilisation of the buildings, which have been erected at so great a cost, in order that a fair proportion of the money that has been expended upon them may be recovered. In furtherance of the latter object, the Executive Committee have much pleasure in stating that they have received from Her Majesty's Commissioners of 1851 an intimation that, provided the grounds are used solely for the purposes of holding exhibitions, they would be willing to extend the existing agreement (which expires on December 31 next) for a further period of three years. The Executive Committee have every reason to believe that, with the approval of the Prince of Wales, exhibitions of great importance will be held in each of these years. Under these arrangements the authorities, which His Royal Highness may be pleased to constitute for carrying out each of these exhibitions, will become tenants of the Fisheries Exhibition, and would accordingly pay a proportion of the original cost as rent for the use of the buildings. The Chairman said it was a matter of congratulation that the numbers admitted had proved to exceed the most sanguine expectations of the general public, and the Committee had every reason to believe that for the future, especially during the month of September, large numbers of visitors would attend. The most important portion of the Report referred to the future use of the buildings. Next year it was proposed to hold a great international exhibition of horticulture, floriculture, and forestry, and they had every reason to believe it would be successful. There had been some question of the conferences being continued later on. The discussion on the paper by the Duke of Edinburgh was adjourned *sine die*, and probably, if His Royal Highness was in London at the end of September or the beginning of October, he might be disposed to attend. There was also another promise given that there should be a fishermen's

congress, which it was proposed should be held at the end of September. The only other matter was with reference to the juries. The reports had nearly all come in, and they had only now to wait for the meeting of the Government and Special Commissioners to confirm the various awards.

ON the occasion of the unveiling of the statue of Daguerre at Cormeilles on Sunday, it was stated that the family is not extinct, The present representative being M. Behon-Daguerre, a contributor to French scientific journals. It was on August 19, 1839, that the Daguerreotype was publicly exhibited by Arago at a sitting of the Academy of Sciences. This communication was made in accordance with the provisions of a law granting to Daguerre and Niepce a joint annuity of 400*l*. for the purchase of their invention on behalf of the French nation. Of the members of the Academy sitting on August 19, 1839, only two are now alive—M. Dumas, the Perpetual Secretary, and M. Chevreul, who was then in the chair. It was M. Chevreul who congratulated M. Daguerre in the name of the Academy of Sciences.

AN excellent paper taken from an address delivered to secondary school teachers in Switzerland has been circulated by the U.S. Education Bureau to answer the question, How to teach natural science. It urges that knowing facts is not the object of such education; in that case a supply of works of reference would be a royal road. "One gets on faster with a child by carrying it, but it is for the child's interest to teach it to run and swim by itself." A teacher, therefore (who must be laboriously grounded himself), must patiently bring *all* his scholars, not the most promising only, to discover and observe facts for themselves—teach them to *see*. Cram is most dangerous in scientific teaching, because most easy to both of them. Books, therefore, should be little used, and nothing about an object should be taught without such object before them. After *seeing*, the next lesson is *describing*, with the help of drawing if possible, both leading to accuracy in the use of language. Plants first, which are plentiful for experiments, then animals of different classes: later on minerals should be chosen, mechanical effects on these latter first, later on chemical. The district museum of natural history and such classes would mutually assist each other greatly; in fact neither, to be successful, would long go on without the other. And, indeed, the lecturer wisely cautioned his hearers that making collections must not become a rage with the pupils.

M. PASTEUR has addressed a telegram to M. Dumas, Perpetual Secretary of the Academy of Sciences, to inform him that he has received telegraphic news from the French Mission which has gone to Egypt to study the cholera. M. Pasteur says that it contains very curious observations of a highly novel character.

THE Royal Commissioners on Technical Instruction are now engaged in preparing their Report, which promises to be a work of considerable magnitude. The completion of it will probably occupy more time than was originally contemplated.

PROF. W. M. HICKS, M.A., has been appointed Principal of Firth College, Sheffield, in the room of Prof. Jones, the newly-appointed Principal of the South Wales College. Mr. Hicks is a Fellow of St. John's College, Cambridge, and was seventh Wrangler in 1873. He worked in the Cavendish Library under the late Prof. Maxwell.

IN addition to the observations carried on around the Pole the physical institution at Upsala has also carried out others in that place during the winter, which were brought to a close on August 15 last.

WE have received from the President of the University of Tokio the Calendar of the Departments of Law, Science, and Literature for the session 1881–82. Like the Report of the Japanese Department of Education, this volume comes somewhat late. As is the case with all the Japanese educational establishments, we notice here the rapid reduction in the number of foreign teachers, and the increase in the number of qualified natives who take their places. Thus in the department of science we find thirty-six instructors of various grades, of whom only seven are foreigners, and there have recently been still further reductions. Many of the native professors appear to have excellent academical degrees from European and American universities, and one is a Cambridge Wrangler. Among the changes in the curriculum during the session we observe that the permission to students to choose between the study of French and German is taken away, and the latter language made compulsory. "This change has been made in order to enable students to pursue their studies or professions in future to the best advantage, since it is believed that Germany is the country where the sciences here pursued have reached the highest comparative development." Several graduates were despatched during the year to Europe to continue the study of zoology, mechanical engineering, medicine, and political science. The total number of students was 170, 91 of whom had entered for the scientific course; while the total number of graduates was 133.

THE new biological laboratory of the Johns Hopkins University, which will be opened next September, has, *Science* states, been especially constructed with reference to providing opportunity for advanced work in experimental physiology. It contains two large rooms for general advanced work in animal physiology, in addition to others specially designed for work with the spectroscope, with the myograph, for electro physiological researches, and for physiological chemistry. It also contains a special room constructed for advanced histological work, and well supplied with apparatus and reagents, a room for micro-photography, and rooms for advanced work in animal morphology.

A TELEGRAM from Batavia, dated August 27, states that terrific detonations from the volcanic island of Krakatoa were heard on the previous night, and were audible as far as Soerakarta, showers of ashes falling as far as Cheribon. The flashes from the volcano were plainly visible from Batavia. Serang is now in total darkness. Stones have fallen at that place. Batavia was also nearly in darkness. All the gaslights were extinguished during the night. It was impossible to communicate with Anjer, and it is feared that some calamity has happened there. Several bridges between Anjer and Serang have been destroyed, and a village near the former place has been washed away, the rivers having overflowed through the rush of the sea inland. This rush is spoken of in the telegrams as a "tidal wave," but it is evidently more of the nature of an earthquake wave, a phenomenon so well known on the west coast of South America. Java is the centre of one of the most active volcanic regions on the globe; it has about sixteen active volcanoes, and many more which are mostly quiescent, not extinct.

A TERRIBLE tornado broke over the south-eastern part of Minnesota on August 22. At Rochester forty persons are reported to have been killed and fifty injured. A third of Rochester is stated to be wrecked, and it is feared that the whole country around that town is in ruins. The number of killed is estimated at some hundreds. A passenger train on the Rochester and Northern Railway was blown off the line, and it is reported that twenty-five passengers were killed and thirty-five injured. The storm also visited Utica, St. Charles, and neighbouring counties.

ON August 5 at about 9 p.m. a very fine meteor was observed from several places around Lake Vettern, in Sweden. It passed across the sky from west to east, and possessed a magnificent lustrous head and tail somewhat resembling a large rocket. Its speed was as slow as that of the latter, its passage

occupying exactly two minutes, while it left a shining track for several seconds in the sky. On August 12 at about 9 p.m. a meteor was seen at Sarpsborg, in Norway. It represented a fireball with a long shining tail, passing in a straight line across the sky in an easterly direction. It was in view for about one minute.

A SHOCK of earthquake of a rather severe nature, but of short duration, was felt at Agram at 3.40 p.m. on the 28th inst. It was accompanied by subterranean rumblings.

M. JACQUELAIN has endeavoured to prepare a pure carbon for electric purposes that should be as hard and as conductive as gas carbon. He first takes gas carbon, which he submits to four processes: (1) treatment with dry chlorine at a red heat for thirty hours; (2) treatment with hot alkali for about three hours; (3) immersion in hydrofluoric acid (1 to 2 of water) at a temperature of 15° to 25°; (4) carbonised by heating strongly in the vapour of a high-boiling hydrocarbon, for commercial purposes gas tar will do well. All these operations may be performed after the carbon has been cut into sticks. By these processes the impurities have been reduced to a minimum and a good, pure carbon obtained.

THE director of the Jardin d'Acclimatation of Paris has just received an entire tribe of Kalmucks from the desert lands in the neighbourhood of the Caspian Sea. It consists of 9 men, 8 women, 4 girls and children, 18 camels, 15 mares and young horses, 10 Kirghiz sheep, with tents, instruments, arms, &c. They will probably visit London after having made in Paris a stay proportionate to their success.

ON Sunday week an extraordinary ascent was made at Nogent-sur-Marne. The aëronaut ascended at 4.30 p.m., and landed near St. Cloud on the following day at 7 a.m. He remained 14½ hours in the air, and travelled no more than 30 kilometres.

M. FRIEDEL has found that at certain temperatures blende, chloride of sodium, and boracite exhibit pyroelectric phenomena. Boracite he found to be so most markedly at the point when it lost its cubical form whilst cooling after being heated to 265°.

MESSRS. LONGMANS AND CO. have issued the eleventh edition of Prof. Atkinson's translation of Ganot's "Elementary Treatise on Physics." About thirty-two pages have been added to the new edition, while the chapter on the steam-engine has been entirely recast.

MR. FISHER UNWIN has added to his useful series of Half-Holiday Handbooks a Guide to Wimbledon, Putney, and Barnes. The same publisher also sends us a little Handbook to the Fernery and Aquarium.

M. DE FONVIELLE asks us to say that by mistake he stated in his note on the Montgolfier statue that it was cast in bronze; it is in plaster, and the cast is being executed.

THE additions to the Zoological Society's Gardens during the past week include a Maholi Galago (*Galago maholi*), purchased; a Vervet Monkey (*Cercopithecus lalandii*), presented by Mr. J. H. Sheppard; two Golden Eagles (*Aquila chrysaetos*) from Scotland, presented by Mr. A. H. Browne; two Short-toed Eagles (*Circaëtus gallicus*), purchased; a Yellow-headed Conure (*Conurus jendaya*), presented by Her Grace the Duchess of Wellington; a Slender-billed Cockatoo (*Licmetis tenuirostris*), presented by Mr. R. Keele; a Land Rail (*Crex pratensis*), presented by Mr. M. Bryant; a Partridge Bronze-winged Pigeon (*Geophaps scripta*), and a Modest Grass Finch (*Amadina modesta*), presented by Mrs. J. Abrahams; a Martinique Waterhen (*Porphyrio martinicus*), a Mississippi Alligator (*Alligator mississippiensis*), presented by Mr. Cuthbert Johnson; six Chameleons (*Chamæleon vulgaris*), purchased; a Hog-nosed Snake (*Heterodon platyrhinos*), presented by Mr. F. J. Thompson.

OUR ASTRONOMICAL COLUMN

THE DIVISION OF BIELA'S COMET.—Those who have made themselves acquainted with Hubbard's masterly researches on the motion of Biela's comet will be aware that he arrived at the conclusion that the disruption of the comet, by whatever cause effected, took place in heliocentric longitude 318°·6, and latitude + 12°·0, distance 4·36, which position he states the comet occupied in November, 1844. In fact, if we adopt Hubbard's final elements for perihelion passage in February, 1846, we find for 1844, November 16·0 G.M.T., longitude 318° 36′, latitude + 12° 2′, radius-vector 4·3665, and the true anomaly 209° 57′. At the time when Hubbard's investigation was made, no one of the known minor planets attained this distance from the sun. We are now acquainted with several which recede further, towards aphelion passage, and an encounter between the comet and a small planet might explain the phenomenon which occasioned so much astonishment in 1845-46. The orbits of some 230 of these bodies have been calculated, but on submitting them to examination with a view to discover whether any one of the planets could pass through the point indicated by Hubbard as that of the separation of Biela's comet, we arrive at a negative result. *Andromache* recedes to a distance of 4·723 from the sun, *Ismene* to 4·590, and *Hilda* to 4·632, but at such distances all three are much nearer to the plane of the ecliptic than Hubbard's position. We may therefore say that if the Biela catastrophe was occasioned by collision with a small planet, it was not one of the large number already calculated.

VARIABLE STARS.—Mr. Knott has succeeded this year in following the variable S Virginis almost if not quite to a minimum, but unfortunately the long twilight, moonlight, and hazy and cloudy skies in July preventing him from fixing the exact date. On April 4 the star was 9·7m., and ruddy; April 25, 10·15; May 4, gauged 11·5; May 31, 12·1; June 25 and 28, 12·2 and 12·3; June 30, 12·7; and on July 4, by a doubtful observation, 12·75. The observations made by Mr. Hind, soon after the discovery of the star's variability in 1852, compared with those of Prof. Schönfeld to 1875, give the following elements:—

		Days.
Minimum	1875, April 27·4 + 373·77 E.	
Maximum	1866, June 7·15 ,,	

This formula assigns July 4, 1883, for minimum, a date closely borne out by Mr. Knott's observations, and for next maximum, 1883, October 30, not observable.

The star varies from about 5·7m. to 12·7. It is XIII. 420 of Weisse's Bessel, and its position for 1884·0 is in R.A. 13h. 26m. 56·9s., N.P.D. 96° 35′ 46″.

Mr. Knott has also found a maximum of R Scorpii on 1883, July 9, magnitude 10·1. S Scorpii had already passed maximum when the observations commenced in the middle of May.

THE GREAT COMET OF 1882.—It may be hoped that one or more of the larger instruments in our observatories will be employed in a further attempt to fix positions of this remarkable body during the absence of moonlight in September. Positions were given in NATURE, vol. xxviii. p. 334, and will also be found in the *Astronomische Nachrichten*.

Now that the period of revolution resulting from the most reliable calculations approximates to eight centuries, it would be interesting to bring together in their original form the numerous descriptions of the great comet of 1106, the substance of which is given by Pingré, more especially the references to the direction of the tail (between the east and north) in the latter part of the comet's appearance. Like the comet of 1882 it was seen close to the sun: one historian says it was so observed from the third to the ninth hour of the day on February 4.

GEOGRAPHICAL NOTES

WITH reference to the Austrian Meteorological Expedition which on Tuesday last arrived in Vienna from Jan Mayen, we are now able to give the following particulars of the wintering at the island. Leaving Iceland on August 1 the *Pola* sighted the southern point of Jan Mayen on the 3rd, but a thick fog prevented landing until the following day. Lieut. von Wolgemuth, with some officers, at once came on board, and great were the rejoicings on both sides at the meeting. The chief of the expedition states that at the end of August, 1882, the northern storms began with a heavy fall of snow. September was, how-

ever, fine and warm, but with October the storms from the north again came back with cold weather, accompanied by magnificent auroræ of yellow, blue, and sometimes even red colour. The aurora borealis was always seen, and in constant motion, at times covering the whole firmament. On November 12 the Polar night commenced, but the darkness was not appalling, as the ever-recurring auroræ lit up the night. Terrible snowstorms often compelled the members to keep indoors, and not until December came the ice began to form along the coast, but it was often after broken up under terrific storms. During these the spray from the surf on the coast was often thrown several hundred feet inland and covered every object with salt crystals, so that fresh water had to be fetched long distances. In January the greatest cold occurred, $-35°$ C., but even during that month southerly winds often brought the glass up to $+20°$ C. On January 30 the Polar night came to an end. March was on an average the coldest month, and during the same the station was for a short time snowed up. During April and May fresh weather reigned. Early in June a whaler passed the island, but did not observe the station, and by the end of the month no ice was found on the island. Throughout the winter but little stove firing was necessary, and both houses and the provisions fully answered their purpose. During an exploration of the island a grave was discovered which is believed to be that of one of the shipwrecked Dutch sailors who wintered here in 1633, and died through scurvy. The scientific observations of the expedition are, the members state, very valuable, and have been carried out in accordance with the international programme. There was no case of scurvy among the members or the crew, against which every precaution had been taken by the munificent patron of the expedition, Count Wilczek.

In September it will be exactly twelve months since the *Dijmphna* and the *Varna* were last seen in the Kara Sea, since when no news whatever as to the fate of the two vessels has been obtained. It was hoped that the *Willem Barents*, which has been cruising in the Kara Sea during the summer, would have brought some tidings of the missing vessels, but this expectation has failed, as recently stated in NATURE, the Dutch exploring vessel neither finding any trace of the same, nor learning anything from the Samoyedes on the coast of Siberia. If the rumour, which was current early in the year, that the Samoyedes had seen the wreck of a large vessel on the *east* coast of Waigatz Island, is remembered, and which was proved to be incorrect, we may assume that there is as little truth in the recent one to the effect that a vessel had wintered off the *east* coast of that island, a spot which it is hardly likely that either vessel could have reached. On the other hand there is little probability of the hope expressed by Hovgaard in his last despatch and by Nordenskjöld having been realised, viz. that the equinoctial gales of October would set the vessels free and enable them to winter at Port Dickson or adjoining port, as Hovgaard had instructions from Herr Gamél, in such an eventuality, to despatch a message thereof to Yakutsk, and if this was done it would have reached us ere now. There is now only the hope left, if no mishap has occurred to the vessels, that they got free early in the spring, as Norwegian smacks found open water in the Waigatz Strait as early as in May last, and have proceeded to Port Dickson, from whence news may now be looked for. This port may have been reached in safety, although it seems remarkable that the Dutch Meteorological Expedition should not have returned to Europe instead during the summer in accordance with the instructions of the Circumpolar Congress, by which all parties were to return in August of the present year. If to this are coupled the circumstances that the *Varna* is merely a third class Norwegian coasting steamer of inferior qualities and the *Dijmphna*, although strengthened for Polar exploration, an old vessel of no great strength, and that the vessels were last seen in a place which is notorious for the terrible pressure and drift of the pack-ice, with sudden hurricanes, there certainly seems ground for the anxiety for the ships which is now becoming manifest in Copenhagen and Amsterdam. The Dutch have, we learn, taken decided steps to ascertain the fate of their countrymen, it having been decided at a meeting in Amsterdam last week immediately to equip and despatch a steamer in search of the *Varna* from Hammerfest, in Norway.

The *Ellida* left Hammerfest last week to try to discover the missing Dutch Expedition. The Meteorological Institution has now contracted with the owners of a second ship, the *George*, which started on Saturday from Archangel with orders to land on the west coast of Waigatz, and to send help overland to the Kara Sea if the entrances by water are closed. A telegram from Utrecht announces that a Dutch gentleman offers a reward of 50,000 Norwegian krone to the ship which shall find the Dutch Polar Expedition, last seen in the autumn of last year on board the *Varna* in the Kara Sea. This expedition had assigned to it the carrying out of the magnetic and meteorological observations at the mouth of the Yenisei, under the command of M. Lamie, a lieutenant in the Royal Netherlands Navy.

In the *Bolletino* of the Italian Geographical Society for August, Sig. C. de Amezaga gives a detailed account of the Galapagos Islands, based on the recent reports of MM. Icaza and Wolf. The archipelago, which belongs politically to the Republic of Ecuador, is a sort of No Man's Land, at present almost uninhabited except by cattle, goats, swine, horses, asses, and dogs, placed there early in the present century, which have multiplied exceedingly, and partly reverted to the wild state. The dogs especially are very numerous and savage—like their wolfish ancestors preying on the goats and cattle. Similar propensities have been developed by the "bimana" from the mainland, who, at various dates between 1831 and 1878, have attempted to establish settlements on Floriana (Charles Island) and one or two other members of the group, but who generally ended by mutually exterminating each other. The archipelago comprises thirteen volcanic islands, and numerous rocks scattered over a space of about 6000 square miles, but collectively forming scarcely more than 720 square miles of dry land. Within this limited area are represented two remarkably distinct physical and climatic zones, one low, hot, barren, and rainless, extending from the sea-level to an elevation of about 650 feet, the other thence to an extreme altitude of 1435 feet (cone at north extremity of Albemarle Island), subject to tropical rains from February to May, followed by heavy dews for the rest of the year. Here the igneous rocks have been completely disintegrated, forming a thick layer of argillaceous clay and humus, on which flourishes a varied and vigorous vegetation. But this upland wooded region is of such limited extent compared with the arid lowlands, that probably not more than 60 square miles altogether are suitable for cultivation. The native flora, while in general of the American type, presents many peculiarities, especially in the phanerogamous plants, all of distinct species, which have not been satisfactorily explained by the special climatic and physical conditions. There is also a total absence of lianas, creepers, palms, musaceæ, aroideæ, and the other monocotyledonous families which form the glory of the Amazonian forests. The indigenous fauna is represented chiefly by reptiles, including four or five species of snakes, none of which are venomous. The huge turtles and land tortoises, the strange marine iguanas, and other survivals from remote geological epochs, impart a certain antediluvian aspect to the landscape, especially of the low-lying coastlands, which are little frequented by the animals recently imported from the mainland. The climate, everywhere healthy, with an average temperature not exceeding 73° or 74° F., even in the hot zone, is favourable both for stockbreeding and the cultivation of sugar, bananas, and all kinds of vegetables and fruits of tropical and temperate regions. There, are, however, no mineral resources, and a complete absence of guano, phosphates of lime and carbon, all of which were formerly supposed to abound in the archipelago. On the other hand, there are a few good and easily-accessible havens, such as those of Post Office Bay and Black Beach Roads on the east side of Floriana. Hence the Galapagos Group, lying at about 900 miles from Panama on the direct route to Australasia, cannot fail to acquire great economic importance as a provision and coaling station as soon as the projected interoceanic ship canal is constructed. The notice is accompanied by a good chart of the archipelago on a scale of 1 : 889,000, showing elevations, cones (of which two are still active), marine and prevailing atmospheric currents.

It may be remembered that Baron Nordenskjöld, at the moment of leaving Iceland on the present expedition, discovered in the possession of an Icelander an old map of North Europe, which he, judging from his announcement of the discovery through the medium of the Royal Geographical Society, believed to be very old, perhaps as old as the famous Zeno map, to which reference has previously been made in NATURE, and which he appeared to consider further supported his views concerning the Norse settlements in Greenland. We have, with reference to the map in question, received a communication from an eminent Swedish geographer, informing us that, having had an opportunity

of examining the map now in Stockholm, he is convinced that it is simply a Dutch sea chart from the beginning of the seventeenth century, and of no value whatever, which he believes Baron Nordenskjöld did not, under a mere cursory examination, discover. In consequence of the opinion pronounced by our correspondent, the Swedish Geographical Society has decided not to have facsimiles of the map taken.

INDIAN METEOROLOGY[1]

II.

THE title of Paper IV.—"Storms in Bengal, accompanied by increased Atmospheric Pressure and the Apparent Reversal of the Normal Diurnal Oscillation of the Barometer," by Prof. J. Eliot,—must necessarily appear somewhat strange to those accustomed in our latitudes to the frequent masking, if not actual reversal, of the normal diurnal oscillation, by the large and rapid non-periodic oscillations to which the atmospheric pressure is subject.

There are two reasons why this reversal should be rare in the tropics, and of such frequent occurrence, as to be more often the rule than the exception, in higher latitudes. One is, that the range of the diurnal barometric oscillation is greatest near the equator, and diminishes as we approach the poles, and the other, that the range of the non-periodic oscillations varies in precisely the opposite way, increasing very nearly in the ratio (as Ferrel has shown) of the square of the sine of the latitude to unity.[2]

Instances of such inversion, are said by Prof. Eliot to be extremely rare in Bengal, but a perusal of the paper leads us to conclude that it is rather a case of *de non apparentibus* than *de non existentibus*, and that a tendency towards reversal takes place to some extent in all north-westers and analogous storms of a sudden and violent character.

Humboldt, Col. Sykes, and Allan Broun, have all graphically described the regular march of the diurnal barometric oscillation, but their observations were mostly made a good deal nearer the equator than Bengal, and thus in regions where reversal of the diurnal oscillation would be a still rarer phenomenon.

In the cases cited by Eliot, including one observed by Hill at Allahabad, the following characteristic changes were observed about the time of reversal:—

1. A marked rise of the barometer.
2. An equally marked and simultaneous fall of the thermometer.
3. A sudden decrease of the tension of aqueous vapour.
4. An instantaneous change of wind direction.

A consideration of all these features, leads the author to conclude that in these cases a downrush of cold air belonging to an upper current (which is known to travel seawards in Bengal, and is therefore dry) takes place in the centre of the area of low pressure belonging to the storm. On no other hypothesis, does it seem possible to explain all the facts, especially the rise in the barometer, and the fall in vapour tension.

A close study of these north-westers, whether accompanied by reversals or not, and their analogues in other parts of the world, is certain to unravel much of the complexity surrounding such and all aërial disturbances, and as all facts bearing on them are valuable, and the present writer was for some time resident in a locality (Dacca) where they occur with marked intensity, he may perhaps be allowed to remark that one of the most peculiar features he has noticed in connection with them, is the almost instantaneous return of the wind to its original direction after the rear of the storm-cloud has passed the zenith. Both before and after the storm, the wind in Bengal blows from the direction of the sea (south-east). The storm-cloud appears to form in the north-west by an aggregation of vapour that is carried thither in cloudlets from the sea. After a time, the threatening mass advances towards the sea, the sea-wind meanwhile blowing towards the advancing cloud with increased force, until the latter has arrived pretty close, when a lull takes place, after which the wind instantaneously changes to the opposite quarter (generally through the north), from which it blows with great violence. Then come the characteristics already noticed, together with continuous lightning and hail, the latter often very large.

When the storm-cloud, the rear of which presents a very definite outline, passes the zenith, the wind invariably returns to its former direction, and gradually dies down as night approaches. The whole phenomenon appears to favour the notion which has always been entertained by the writer, and is merely a slight extension of the explanation given by Prof. Eliot, that a sudden *oblique* uprise of moist hot air takes place, deflecting the upper current into a corresponding *oblique* downward course, which determines the direction of the storm and continues as long as the uprising air interferes with its regular motion parallel to the surface. This explains why the storm always follows the course of the upper current, as well as the immediate readjustment of the original conditions, as soon as the region of ascending air which causes the deflection has passed the spot.

Paper V. "On the Rainfall of Benares in Relation to the Prevailing Winds," by S. A. Hill.—The observations utilised in this paper, as far as the velocity of the wind is concerned, raise a question of general importance, and one which we think ought to engage the attention of all thoughtful, and certainly all practical meteorologists. Up to 1872, the anemometer was only 15 feet above the ground, but in that year it was raised to a height of nearly 80 feet. Now what are we to think of the effect of such a change of position on the observations?

According to Mr. Stevenson, 15 feet is the lowest elevation at which an anemometer should be placed, since below this height the velocity is found to be enormously affected by the nature of the surface. On the other hand, 80 feet is an elevation which would not only cause the instrument to register a considerably higher velocity,[1] but also secure for it nearly complete immunity from the disturbing influences which would be sure to affect it in the lower position.

It is indeed very much to be regretted that in setting up anemometers in India no sort of uniformity seems to have been attempted. Thus from a list of their elevations above the ground given in the "Meteorological Report for India" in 1876, every variety of height imaginable occurs, ranging from 5 feet 7 inches at Khandwa to 76 feet 11 inches at Benares! At no two stations are the anemometers at the same level, and though it is somewhat complacently admitted by Mr. Blanford "that it can hardly be affirmed that in the majority of cases the anemometric records are strictly comparable," one is naturally inclined to ask why the instruments could not have been placed, if not exactly, at least more nearly, at the same level. Such an arrangement would seem to be a cardinal requisite where such a sensitive element as air motion is involved, and indeed Mr. Stevenson's experiments have shown that while observations below 15 feet are almost useless, the velocity increases rapidly with the height through the first 100 feet; so that until a correction is made for the height, it would be vain to attempt a comparison of observations made at lower, with those at higher levels.

Fortunately in this case the discussion mainly relates to the direction of the wind, so that the conclusions are not seriously affected by the change in the position of the instrument.

The chief conclusion arrived at by the author is similar to that obtained by Mr. Blanford in the first paper of this series for Calcutta, except that Benares affords no corroboration of the conclusion drawn from the former register, that "rain is the more probable in proportion as the deflection of the monsoon current is greater."

It appears, nevertheless, that there is a well-marked connection between the amount of rain that falls *in a day* and the deflection of the rain-bearing current, the maximum amount being from the opposite quarter (north-west) to that from which the monsoon blows.

Paper VII. "Variations of Rainfall in Northern India," by S. A. Hill.—This is one of the most interesting and important papers of the whole series, its ostensible object being partly to test the soundness of the idea which was propounded by Dr. W. W. Hunter and others in 1877, that sunspots, rainfall, and the occurrence of droughts and famines were closely associated in India. Regarding this *vexata quæstio* it may be said at the outset that while the general results of an investigation embracing an area which covers eleven degrees of latitude and twenty of longitude (equal to that of the British Isles, France, Germany, Austria, Holland, and Belgium combined), like those of Mr. Blanford for Southern India, bear out Meldrum's theory of an eleven-year cycle of rainfall, they exhibit certain irregularities, or, more properly speaking, double oscillations, which, as Prof.

[1] Concluded from p. 407.

[2] "Relation between the Barometric Gradient and the Velocity of the Wind," by W. Ferrel. *American Journal of Science*, vol. viii., November, 1874.

[1] According to Mr. Stevenson's formula, which holds near the surface, the velocity would be increased by this change of position in the ratio 1·32 : 1. Thus for 1872 the observed mean value was 67·8. To make this comparable with the years that follow, it should be 89·4!

Hill shows, are probably due to local reacting circumstances, and which afford but little hope of our ever being able to forecast droughts and famines in North-West India solely from a knowledge of the state of solar maculation.

The fact that the terrestrial effects of solar changes are conspicuous in some localities and almost totally absent in others seems to many persons incompatible with the cosmical nature of the influences at work, but to those who study the subject it appears, on the contrary, the only result to be reasonably expected both from experience and analogy.

Thus ordinary weather is the integral of all the differentiations effected during the regular seasonal changes in solar declination, and it need scarcely be remarked what an endless variety of conditions we have in this case, due primarily to the operation of a gradual and periodic cause. Owing to diversities of superficial character, elevation, contour, latitude, &c., we have meteorological oscillations set up, differing from each other in phase and amplitude, which, like the tides of the ocean, in some places tend to exaggerate and in others to annihilate each other. So also must it be, where we have solar changes which gradually perform their cycle in a period of years. The forced oscillations they originate, though small, may in some localities, by a unison of oscillations of the same phase, or an absence of opposing oscillations, be exaggerated above the mean amplitude, just as in others they may, owing to an inequality of phase or the clashing together of opposite variations, be rendered inappreciable.

Prof. Eliot, in his "Report on the Meteorology of India for 1877" (p. 3), evidently recognises this fact, when he admits the probability that "at one part of the sunspot period one effect of the variation of solar radiation may be to exaggerate local irregularity." While therefore it is probable that we shall find only a few places, where the terrestrial effects of solar spot variation are of sufficient magnitude and regularity to render secular forecasting possible (assuming that our foreknowledge of solar changes is reliable), such a fact ought by no means to be used as an argument against the utility of studying the relations between solar physics and terrestrial meteorology.

Mr. Blanford, who from the first attacked the somewhat crude hypothesis propounded by Dr. Hunter regarding sunspots and famines in Southern India, has in his own person furnished a practical protest against any such idea, since his researches on the connection between barometric pressure and sunspot variation have tended, not only to confirm the belief in the bond existing between solar and terrestrial changes, but have also opened out new collateral facts, which, if followed up, are certain to yield results of the highest importance to the science of meteorology. His own views on this question, which have frequently been misunderstood in certain quarters, and referred to as adverse to the general question, are concisely expressed in the following sentence, which we quote from an official report recently made by him to the Indian Government :—

"While, however, I am unable to concede to the conclusions hitherto placed on record, that degree of importance which has sometimes been claimed for them, as affording rules of guidance for the prognostication of scarcity and famine, I am fully in accord with the Famine Commissioners as to the importance of following up such clues as they afford, and of pursuing with all the means at our command the investigation of the class of phenomena to which they belong. It has happened again and again in the past history of science, that hypotheses, which in their original form were more or less erroneous, have nevertheless been most fruitful in their results. In giving system and definite purpose to research they have served a most useful office; and although the course of their verification may have resulted in demonstrating their error, the same process has brought to light the germs of new and unsuspected truths which might have long remained hidden but for the stimulus to investigation afforded by rejected theories."

The nature of the entire question indeed, seems to have been a good deal misunderstood in this country, at least to judge from the extraordinary amount of obloquy and opposition which it has encountered in various quarters.

On the one hand, it must be obvious to any one who casts even a merely superficial glance at the vast changes in the physical condition of the sun, indicated by the spots, prominences, &c., and the dependence of all terrestrial meteorology on the quantity (and perhaps quality) of the heat radiated from our great luminary, that such changes in the former, must be reflected to some extent in the latter, as indeed they are universally allowed to be in the case of terrestrial magnetism. On the other hand, it is equally obvious to the merest tyro in meteorology, that such meteorological fluctuations, though in many cases distinctly recognisable, are not only of small average amplitude (especially in high latitudes) when compared with those which occur, as we say at present, non-periodically, but take a period of years to accomplish their cycle. To imagine therefore, that such changes, even if thoroughly determined, will *alone* enable us to forecast the general weather of a season or a year, is manifestly irrational as far as these latitudes are concerned, while even in India and the tropics generally, we have grounds for believing that there are only a few places, where the extreme range of the oscillation bears a ratio to the non-periodic changes large enough to constitute it the dominant factor of the weather.

That when the conditions which regulate the larger and more irregular changes are better understood, a knowledge of the underlying secular meteorological changes coincident (or nearly so) with the varying phases of solar activity, will be of great *assistance* in framing seasonal forecasts, it is impossible to doubt. At the same time it seems strangely to have been overlooked by the majority of those who have interested themselves in this fascinating question, that though the sunspot variation in meteorological elements may alone be insufficient to form the basis of a practical system of weather prophecy, it is very likely to prove *the key by which the entire weather problem may be solved*, since, when once we know the precise qualitative as well as quantitative meteorological effects of a gradual *secular* change in the solar radiation, coincident with the sunspot cycle, we shall gain an immense insight into the way in which the larger and more rapid oscillations are effected by the *ordinary* changes in solar radiation, brought about by season, latitude, geographical locality, &c., these latter oscillations only differing from the former, in being more frequent and of greater amplitude.

Prof. Hill in his investigation, adopts a plan which is obviously necessary to any one who knows the peculiarities of Indian meteorology, viz. the separate treatment of the summer and winter rainfalls.

The former season embraces the period from May to October, and the latter the remaining months. An eleven-year period, in favour of which there is a good deal of preliminary evidence, is assumed, in order to see if there is any correspondence with the analogous mean period of solar spot variation. The oscillation of the summer rainfall for the whole area about its mean, is then estimated for the cycle, and is found to accord generally with the results for Southern India, and with Meldrum's supposed universal law, in showing a direct variation with the sunspots, the range being twice as great as in Southern India. At the same time considerable irregularity is visible, some of the stations at the border of the monsoon region giving results contrary to the average variation. The winter rainfall on the other hand shows a much closer relation to the sunspots, the remarkable thing about it being, that instead of varying *directly*, as the summer fall, it varies *inversely* with the spotted area.

The variation of this winter fall shown in the text is very regular, and confirms a conjecture hazarded by the present writer in 1877, that the similar variation which he had previously shown to exist in the winter rainfall of Calcutta would be found to be more decidedly marked in the sub-Himalayan zone to the north of it (NATURE, vol. xvi. p. 267, Meteorological Notes).

In a postscript to the paper Prof. Hill has worked out the variation from a longer series of observations, which were discovered, apparently by accident, at the Board of Revenue in Allahabad, and which, by means of registers kept in the Himalayan province of Kumaon, "to which the civil disturbances following the mutiny of 1857 did not extend," allows the cycle to be worked out for the period 1844 to 1878. The final result given in the form of percentage variations from the mean, is as follows :—

Winter Rainfall of North India

Years of Cycle.	1	2	3	4	5	6	7	8	9	10	11
Mean percentage variation.	−6·8	−0·6	−3·6	−15·5	−17·3	+0·8	+27·3	+24·7	+2·1	−5·6	−5·4

For the summer rainfall the variation given in the text is as follows :—

Summer Rainfall of North India

Years of Cycle.	1	2	3	4	5	6	7	8	9	10	11
Mean percentage variation.	+3·6	+7·4	+9·8	+12·6	+7·8	−5·6	−10·8	−8·1	−10·0	−7·0	−0·6

In the cycle as arranged above, the first year is that which contains the year of maximum sunspot, and the eighth that of minimum sunspot.

With the figures in the text, the maximum winter rainfall occurs on an average rather more than a year before the minimum of sunspots, and the minimum of rainfall appears either to coincide with, or to follow the maximum of the sunspots, at about an equal interval.

While, therefore, the facts are so far favourable to a close connection between sunspots and rainfall in Upper India, they do not lead so much to the conclusion that the former directly affect the latter, as to their both being effects of some common and as yet undetermined cause.

It should be further noticed, both as a result of this investigation, and an example of one of the "new and unsuspected truths" which Mr. Blanford says are often incidentally brought to light, that the variations of the summer and winter falls are almost exactly contrary to each other, and as this has been found to occur not only in the years of the mean cycle, but also in individual years, it has been concluded by Prof. Hill that *in Northern India the winter rains are excessive when the summer rains are defective and vice versâ.*

This contrary variation, which is of itself a valuable discovery, is moreover shown to be due in some measure to a reaction of the winter on the summer rainfall. Thus, in years of heavy winter rainfall in Northern India, and therefore of heavy snowfall in the Himalayas, an excess of barometric pressure attended by diminished temperature, is found to occur during the earlier months of the year, which causes the air to move outwards from the centre of relatively highest pressure, and so bar the approach of the Arabian Sea current from the south-west, as well as the Bay of Bengal current from the south-east, and by thus compelling them to part with their moisture in other districts, such as the hills of Central India, or East Bengal and Burmah respectively, causes deficiency and drought over the Punjab and North-West Provinces, or Western Bengal.

On the other hand, in years of defective winter rainfall, the temperature is generally high, and the pressure low, in the early months of the year; while the currents from the south-east up the Ganges valley appear in full strength, and are accompanied by early and abundant summer rains.[1]

Mr. Blanford has partly attributed the high atmospheric pressure which occurs in the years of heavy snowfall, to the cooling thereby produced, but as this abnormally high pressure sometimes extends right down the Bombay coast, it is plain that the snowfall is not the only determining cause, and that we must look to some more general cause to explain the matter. Prof. Hill speculates very intelligently on this cause, but as the speculation requires confirmatory evidence, it will be as well perhaps not to dwell on it at present.

It may, however, be observed that this speculation accounts satisfactorily for the double oscillation of the Bengal summer rainfall with its *maxima* at both sunspot epochs, as well as the double oscillation of the annual rainfall of Southern India, noticed by the late Mr. J. A. Broun, F.R.S., in NATURE, vol. xvi. p. 334 (which, unlike that of Northern India, is solely due to the *summer monsoon current*) with its *minima* at both epochs, two remarkable facts, which might at first sight appear to be almost irreconcilable, if not unaccountable.

Before leaving this interesting and suggestive paper, it should be remarked that the variation in the winter rainfall of Northern India is shown to be closely connected with the curve of air-temperature in the tropics calculated up to 1862 by Dr. Köppen, and continued up to 1877 by Prof. Hill from Indian observations alone.

The following table gives the epochs of maxima and minima of both elements, and the conclusion can, we think, scarcely be resisted that there is a causal connection between them, since in every case but one, the rainfall epochs slightly *follow* those of the temperature:—

Maximum and Minimum Epochs of Tropical Temperature and Winter Rain

Minima.		Maxima.	
Temperature.	Rain.	Temperature.	Rain.
1836·9 ...	1837·8 ...	1842·7 ...	1842·7
1847·7 ...	1848·1 ...	1854·7 ...	1855·0
1858·4 ...	1860·6 ...	1865·1 ...	1865 5
1874·8 ...	1874·7 ...	(1876·3) ...	(1876·9)

[1] These opposite conditions are now so universally recognised, as almost to form a canon of Indian meteorology.

Similar variations are shown to exist in the winter rainfall of other parts of the world, as well as in the humidity of Russia and Siberia, which favour the hypothesis long entertained both by Prof. Hill and the writer, that "the winter rains in Northern India occur simultaneously with an increase in the quantity of aqueous vapour in the atmosphere over Eastern Europe and Western Asia, and that the cause of both may *possibly* be found in an unusually high temperature in the tropics, whereby the evaporation of the waters of the ocean is accelerated and the upper current of moist air known as the anti-trade has its velocity increased."

SCIENTIFIC SERIALS

American Journal of Science, August.—Principal characters of American Jurassic Dinosaurs, part vi.: Restoration of Brontosaurus, with plate, by Prof. O. C. Marsh. The restoration is effected by bones belonging almost exclusively to a single individual, which when alive was about fifty feet long; chief characteristics: long flexible neck, very short body, massive legs and feet, the latter plantigrade, and leaving footprints about a square yard in extent, very large tail with solid bones, remarkably small head, smaller in proportion to the body than that of any other known vertebrate, skull being less in diameter or weight than the fourth or fifth cervical vertebra. The living animal must have weighed over twenty tons, and appears to have been a stupid reptile of slow motion, without offensive weapons or dermal armature, amphibious in habits, feeding on aquatic and other succulent plants.—The evolution of the American trotting horse, by Francis E. Nipher. The minimum time of trotting a mile, in a previous paper determined at 93, is here reduced to 91 seconds, and it is suggested that the trotter will very probably finally surpass the race-horse in speed.—On concave gratings for optical purposes, by Henry A. Rowland, Professor of Physics, Johns Hopkins University, Baltimore.—Glacial markings of unusual forms in the Laurentian Hills, by Dr. Edmund Andrews. Several illustrations are given of the peculiar marks here described, which are chiefly curved striæ, serrated striæ, and curious scoop-marks, both striated and unstriated, very difficult to explain on any theory of glacial action.—Response to the remarks of Messrs. Wachsmuth and Springer on the genera Glyptocrinus and Reteocrinus, by S. A. Miller.—On the present status of the eccentricity theory of glacial climate, by W. J. McGee. In reply to recent critics the author urges several arguments in defence of Croll's theory of secular variations in terrestrial climate.—On the commingling of ancient faunal and modern floral types in the Laramie group, by Charles A. White.—Notes on some fossil plants from Northern China, by J. S. Newberry. From the general character of these plants, which were collected by Mr. Arnold Hague, the author considers that Pumpelly and Richthofen's estimates of the great area and value of the North China coal and iron deposits are by no means unwarranted. The plants, all of the Carboniferous age, seem to prove that the Chinese coal basins belong to two great geological systems, one answering to that of the European and American coal-measures, the other probably referable to the Rhœtic and Lias.—Review of De Candolle's "Origin of Cultivated Plants," with annotations on certain American species, by Asa Gray and J. Hammond Trumbull.—On the supposed human footprints recently found in Nevada, by O. C. Marsh.

THE *Journal of the Franklin Institute*, August.—Cranes; a study of types and details, by Henry R. Towne.—A remarkable error in the common theory of the turbine water-wheel, by J. P. Frizell.—Béton in combination with iron as a building material, by W. E. Ward.—The grindstone, by J. E. Mitchell.—The Glover tower and the working of sulphuric acid chambers, by Moses A. Walsh.—On radiant matter spectroscopy, a new method of spectrum analysis, by William Crookes, F.R.S.—The cause of evident magnetism in iron, steel, and other magnetic metals, by D. E. Hughes, F.R.S.—National Exhibition of Railway Appliances, Chicago, Ill.—Obituary, Benjamin Howard Rand, Franklin Institute.—Notes.—Induced currents in reciprocal movements.—Twinkling of stars during auroras.—Spanish copper tubes.—Photozincography.—Orange peel.—Constitution of the sun.—Colour of distilled water.—Deep-sea explorations.—Generation of inflammable gases in the diffusion of beets.—Amber.

Journal of the Russian Chemical and Physical Society, vol. xv. fasc. 5.—On the formation and properties of oxide of

sodium, by N. Beketoff. The amount of heat disengaged during the complete hydration of sodium has been found equal to 55,000 calories, which figure, combined with that of Thomsen, gives 100,260 calories for the heat of oxidation of one molecule of sodium (50,130 for each atom).—On the naphtha lamp for burning heavy oils, examined at the Chemical Society's competition, by M. Andreéff.—On the naphtha of Caucasus, by MM. Markovnikoff and Ogloblin; second part.—The chief constituent parts of this naphtha, about 80 per cent. of it, would be hydrocarbons of the C_nH_{2n} series—C_8H_{16}, C_9H_{18}, and so on to $C_{15}H_{30}$. The authors propose to call them naphthenes, and describe their properties at length. The aromatic hydrocarbons constitute about 10 per cent. of the naphtha, partly known before, and partly seeming to belong to new series isomeric with the styrol series and its isologues. The oxygenated products, partly acid and partly neutral, play also an important part in the naphtha, which contains also a few phenols and lower hydrocarbons.—On naphtha; an answer to MM. Markovnikoff and Ogloblin, by Prof. Mendeléeff.—On the continuous graphic determination of the depth of shallow waters, by Prof. Petrushevsky. The author proposes to adjust to a boat a pole whose longer end would be dragged at the bottom of the river, whilst its shorter end would draw on a board the configuration of the bottom.—On the determination of the average coloration of a surface painted with different colours, by the same.—On the influence of light on the electrical conductibility of selenium, by N. Hesehus.

Archives des Sciences Physiques et Naturelles, July 15.—Verification of some atomic weights, by M. C. Marignac; first memoir, bismuth and manganese.—American ants, by Henry MacCook.—Ripple marks studied in Lake Leman, by Dr. F. A. Forel (one sheet of illustrations).—New researches on the Saturnian system, by W. Meyer.—Hypoxanthine in potatoes, by A. Weber.—Chloride of calcium, by V. Meyer.—Remarks on methods of determining vapour densities, by Alois Janny.—Acetoximes, by J. Petraczek.—On the aldoximes, by V. Meyer.

Rendiconti of the Royal Lombard Institute of Sciences and Letters, July 12, 1883.—Descriptive catalogue of a new series of rare or unpublished Greek coins and medals preserved in the Royal Numismatic Cabinet of Milan, by the curator, E. B. Biondelli. Amongst the 128 extremely rare and in some cases even unique specimens here described are medals of Julius Cæsar with Augustus from Achulla in Zeugitania, and of the two African Gordians from Cilicia, besides several coins from Sabrata, Thæna, Clypea, and other North African towns, including one of the Mauritanian king Ptolemy, son of Juba II., absolutely unique. The general catalogue of all the oriental and mediæval series, together with the historic and commemorative medals, is making rapid progress, and its publication is promised in a short time. The complete legends as far as legible are given in all cases, together with a brief description of the subjects.—The structure of the seeds in the family of the Oleaceæ fully described, by Prof. R. Pirotta.—On the functions of a single variant with more than two periods, π, π', π'' . . ., by Prof. F. Casorati.—Meteorological observations at the Observatory of Milan, with tables of barometrical and thermometrical changes, and records of relative humidity, direction of the winds, and cloudiness during the month of June.

SOCIETIES AND ACADEMIES

Paris

Academy of Sciences, August 20.—M. Blanchard, president, in the chair.—Observations on the smaller planets made at the great meridian of the Paris Observatory during the second quarter of the year 1883, by M. Mouchez.—On a letter of General Stebnitski concerning the figure of the earth, by M. Faye. The Russian *savant* holds that the actual form of the globe, as expressed by the ideal continuation of the sea-level beneath the continents, differs from the theoretic ellipsoid not only in the undulations produced by the attraction of mountain ranges, and of the denser parts occurring here and there in the crust of the earth, but also in the deformations due to the attraction of the continents. In reply M. Faye contends that the mathematical surface of the globe is not modified by these causes, and that the level of the oceans is not sensibly affected by the influence of the mainland.—A study of the deformations and development of heat produced by the use of round-faced hammers in forging, by M. Tresca.—Observations touching a passage in M. V. Burg's recent communication on the use of copper as a preservative against cholera, by M. Vulpian. The author explains that a statement attributed to him by M. Tresca, regarding the use of copper as a prophylactic by English and French officers in Egypt, India, and Cochin-China, is groundless. He adds that he regards the advantage of the use of copper as a preservative as extremely doubtful.—On the separation of gallium (continued). Separation from tungsten and phosphoric acid, by M. Lecoq.—Experimental researches on explosive gas motors, by M. A. Witz.—Researches on the iodide of nitrogen; on chemical radiometers or iodide of nitrogen photometers; on the preparation in a low temperature of nitrogen, iodide of ammonium, and iodate of ammonia under the influence of light, and on the double iodide of copper and nitrogen, by M. Antony Guyard.—A contribution to the history of the formation of coal, by M. B. Renault. The author concludes that in many cases fossil coal is produced by the transformation *in situ* of the constituent elements of the plants whose forms it has preserved; that both the wood and bark have entered into the formation of coal, and that in the process of transformation the organic elements have diminished in size in a determinable proportion depending on the primitiue density of the constituent organic matter.—Remarks on the *Phylloglossum Drummondii*, by M. C. Eg. Bertrand.

Berlin

Physiological Society, July 27.—Prof. H. Munk spoke regarding the doctrine of the functional restoration of the cerebrum first deduced by Flourens from experiments he made on the cerebrum of doves. Flourens had observed that, on the excision of but a small part of the greater brain, the disorders which resulted in the sensuous perceptions and intelligence of the animal operated on ceased after some time, and the animal then acted as before in its normal state. On the excision, however, of a larger part from the cerebrum, the subsequent restoration was only incomplete. Were, again, a very large part cut off from the greater brain, the resulting disorders continued to the end of the animal's life. Flourens had further concluded that the functions of the whole of the cerebrum were strictly equivalent to one another, and that every part of it was capable of vicariously taking the place of every other part. This doctrine propounded by Flourens regarding cerebral functions having, however, been overthrown in consequence of investigations by Fritsch and Hitzig and replaced by that of the localisation of particular functions in particular parts of the cortex cerebri, the phenomenon which to all observers, on the removing of less than a quarter from the hemisphere of the cerebrum, had suggested the idea of functional restoration of the brain, now received a different interpretation. By some investigators it was maintained that the restoration was to be explained by the function of the excided part of the brain being taken over either by the corresponding part of the other side or by some other part of the brain situated on the same side, in the cortex, or in the interior, in either case in addition to its own special function. Others, again, deemed the restoration only an apparent one; in reality no function was suspended by the removing of a part of the cerebrum, it was only a check that was imposed through the irritation of the act of separation, and when that was relieved, the normal functions came again into play. Prof. Munk has for several years carried on investigations into the functions of the cortex cerebri, leading, as is known, to the conclusion that a limited part of the cortex situated on the flap of the occiput was the seat of the central visual perceptions (the sphere of vision) and that another exactly defined part of the cortex, situated on the flap of the temples, marked the site of the acoustic perceptions (the sphere of hearing), while a third region was appropriated to the sphere of feeling. He has further prosecuted his inquiries into the question of the restoration of cerebral functions, and by experiment has endeavoured to determine whether the assumed restoration of functions previously discharged by parts of the cerebral cortex now removed were a true statement of the fact, and if so how this was accomplished. He first affirms the universally recognised fact that the restoration of matter lost to the brain by the excision of a part or parts of it in no case ever happened, but in every case after the excision the remaining mass only cicatrised. As regards functional restoration, then, his experiments in the spheres of sight and hearing led him to the following conclusions:—Were the spheres of sight or the spheres of hearing removed from an animal, it remained blind or deaf for the rest of its life; no restoration of the faculty in question ever took place in either case, though only limited por-

tions of the brain were removed and the whole of its remaining mass were left intact; this latter could nevertheless in no case ever take the place of the excided parts. Were, again, only one sphere of sight or one sphere of hearing removed, the animal became blind or deaf on the opposite side, and this one-sided blindness or deafness likewise continued throughout the whole of the rest of the animal's life. Even should only small parts of one sphere of sight or one sphere of hearing be removed, restoration of the functions of these parts never followed. Were, for example, the outer half on the left side of the sphere of sight taken away, the median half of the right retina would then continue blind so long as the animal survived this operation. Were the inner half of the sphere of sight taken away, the lateral half of the opposite retina would be rendered blind throughout the rest of the animal's life. Were the hinder part of the sphere of hearing destroyed, the animal would for the rest of its life continue deaf to deep tones. Were the anterior half of the sphere of hearing taken away, the animal would be rendered for ever insensible of high tones by the corresponding ear. Even though ever so small portions of the sections in question of the cerebral cortex were removed, the corresponding part of the retina would be rendered blind, and the animal become deaf to the tones appropriate to the part where the excision was made. It is true that in time the animal learns to make up for the defects caused by the operations and with the remaining unaffected parts of the retina (supposing the operation has reference to the sight) will contrive to see so well, and act in general in such a way as to superficial observations to convey the impression of an animal endowed with normal powers of sight. On close examination, however, of the particular parts of the retina it will in every case be found that the parts corresponding with the excided part in the central cortex is blind. Functional restoration of an excided part of the cerebral cortex never therefore occurs, however small be the part excided. Otherwise, notwithstanding, it would seem to be the case with another function of the sphere of sight which is concerned not with the first visual perceptions but with visual representations or conscious images consequent on perceptions. Has an animal, for example, taken from it the central sphere of sight, it then loses all conscious images; the mere seeing of objects with the intact peripheral parts of the retina is still possible for it, but not the recognition of them. After some time, however, the animal will regain the power of forming conscious images, and will then recognise the objects it sees. Here, then, we have the restoration of a lost function on the part of the cerebrum. In this case, however, the functional restoration is, according to Prof. Munk, only an apparent one. The actual state of the case is as follows:—Conscious images are formed in this way. Visual perceptions becoming an object of attention produce visual representations which give rise in one place of the central organ to a change which, existing as latent conscious image, is aroused by an equivalent or similar visual representation, which in its turn is begotten of perception and attention. These conscious images have their seat in the central part of the sphere of sight corresponding with the central part of the retina, the place of clearest vision. If this central part of the sphere of sight be removed, the animal loses its conscious images, it is soul-blind. According to Prof. Munk's conception, however, the seat of conscious images lies in the centre of the sphere of sight only for this reason, that usually the visual perceptions coming from the central part of the retina, and therefore the most distinct, alone become the subject of attention, and are transformed into visual representations. The images of perception, on the other hand, reaching from the peripheral part of the retina to the peripheral part of the sphere of sight, being less distinct, do not become the subject of attention, and are therefore not transformed into visual representations. If, however, with the central part of the sphere of sight conscious images are taken away—if the animal is soul-blind—attention can now fasten only on the images which are seen by the periphery of the retina, the central part being quite vacant in consequence of the operation. In this case, then, visual perceptions in the peripheral parts of the sphere of sight are by attention transformed into representations, whence now conscious images are drawn. If you render an animal soul-blind on one side, it will never of itself draw conscious images from that side, it will see only with the central parts of the second eye. If now, however, you blindfold the animal on its visual side, and so compel it to look with the peripheral parts of the side operated on, the soul-blindness on this side will vanish. Restoration is consequently a word totally inapplicable here. On the contrary, all that we here find is that cerebral parts are utilised as a repository of conscious images, which by the normal animal are not turned to account simply because it has other parts with more distinct powers of perception to answer its purpose. The circumstance that former observers have always been impressed with the idea of restoration of sensuous activity is to be explained by the fact that the sphere of sight and the sphere of hearing lie only to a small extent on the surface which is more exposed to injury, and therefore, in the case of a simple excision from the cerebrum, they are always only partially affected.—Prof. Zuntz related briefly that last year he had inoculated guinea-pigs with bacillæ of septicæmic rabbits and mice, and that they had all escaped harm. When, however, he repeated these experiments this year, the inoculated guinea-pigs all fell sick, but not from septicæmia, but from peritonitis. When, again, rabbits and mice were inoculated with bacillæ of guinea-pigs who had died of peritonitis, they bred septicæmia and *vice versâ*. Under the microscope both kinds of bacillæ were seen to behave quite alike.

VIENNA

Academy of Sciences, June 14.—F. Steindachner and L. Doederlein, contributions to knowledge of Japanese fishes (second paper).—E. Hann, on the climate of Bosnia and Herzegovina.—C. Etti, on the history of the tannic acid of oak bark.—M. Neumayr, on the morphology of the valve of bivalves.—L. Teisseyra, contribution to a knowledge of the Cephalopoda fauna of the Jura in the Risan Government (Russia).—Zd. H. Skraup and G. Vortmann, on the derivatives of dipyridine.

June 21.—H. Hammerl, a study on the copper voltameter.—R. Benedikt and M. von Schmidt, notes on halogen derivatives.—K. Hazura, on nitro-sulphoresoreinic acid.—H. Bittner, on *Micropsis veronensis*, a new Echinus of the Upper Italian Eocene.—Contributions to a knowledge of Tertiary Brachiura fauna.—A. Lieben and S. Zeisel, on the constitution of butyrochloral.—K. Natterer, on dichlorocrotonaldehyde.—J. Kachler and F. V. Spitzer, on the action of the isomeric camphor bromides on nitric acid.—Zd. H. Skraup, a sealed paper on the constitution of quinine.—S. Exner, on the defective excitability of the retina by light of abnormal incidence.—J. Woldrich, on the diluvial fauna of Zuzlawitz in Bohemia.

CONTENTS

THURSDAY, SEPTEMBER 6, 1883

NEOCOMIAN FOSSILS

The Fossils and Palæontological Affinities of the Neocomian Deposits of Upware and Brickhill. Being the Sedgwick Prize Essay for 1879. By Walter Keeping, M.A., F.G.S. Large 8vo, pp. 167, with eight plates of fossils. (Cambridge, 1883.)

PHOSPHATIC deposits may be said to occur, in this country, on all horizons from the Bala limestone to the crag, yet do they most abound in the "strata below the chalk," and particularly in those portions of the Cretaceous system which underlie the chalk of the south-east Midlands. Thus Cambridge is almost as famous for its *coprolites* as Newcastle for its *coals*, and the economic inferiority of the Mesozoic rocks has of late years been partially redeemed, in consequence of the numerous workings in these valuable beds.

The immense collections of fossils from the several "phosphate diggings," now in the Woodwardian Museum, afford those who have watched the growth of these accumulations a splendid opportunity for studying an unusually rich vein of palæontology, and at the same time of forming more correct views as to the physical history of these much debated deposits.

The coprolite beds *beneath the gault* at Upware were vigorously worked some sixteen years ago, but as the phosphates were inferior in quality, the work presently slackened, though not before large quantities of fossils had been secured by gentlemen from Cambridge, who have ever been foremost in studying these and the allied deposits. The workings at Little Brickhill were described by the author himself in the *Geological Magazine* (volume for 1875, p. 372), but since then the knowledge of its fauna has been largely increased, "so that the Brickhill bed is now only second to Upware and Farringdon in its organic richness." Recently (1879) the Upware sections have again been exposed.

There can be no doubt that Mr. Walter Keeping is very well qualified to perform the task he has undertaken, and that the Sedgwick Prize Essay for 1879 must rank as one of the most useful contributions to Neocomian geology and palæontology that has appeared in this country. It is, in fact, the outcome of a long experience judiciously applied. The author summarises his own work in the preface, so that readers may know what they have to expect: his conclusions as to the age of the ironsand and phosphatic series are stated to be in near accordance with the opinions of Messrs. Walker, Teall, Meyer, and Barrois, "all of whom have placed the Upware bed in the Upper Neocomian or Aptian series (Lower Greensand)." The *mammillaris* zone he regards as the basement bed of the gault, and to this horizon refers the Downham phosphate bed.

The first part of the work deals with the deposits generally, the indigenous fauna, the "derived" fossils, the British and foreign relations of the beds. The second part is devoted to special palæontology.

In discussing the question of phosphatisation he observes that the nodules of Upware and Brickhill have been derived, for the most part, from the Upper Jurassic rocks, where as a rule the majority of the Jurassic fossils are *not phosphatised at all*: and he concludes from the similarity in the general character of the phosphate of lime nodules, whether from Oxford Clay, Kimmeridge Clay, or Portlandian Rock, that the phosphatic matter was derivative, and all, or nearly all of one age. At page 30 he speaks of a coprolite heap near Ampthill, as looking like one mass of *Ammonites biplex*, mostly worn and fragmentary, whilst the Ammonites of the Oxford Clay are composed of limonite, and some of the fragments of fossil wood are silicified. Strangest fact of all—the Coral Rag fossils from the neighbouring rock have not been phosphatised in the least. The author (p. 15) suggests that this purer form of carbonate of lime was "uncongenial to the phosphatic matter," which would have more affinity for argillaceous substances, and yet he quotes the case of a stalagmitic deposit having become phosphatised by percolation.

It is not a little suggestive that whilst *Ammonites biplex* from the Upper Kimmeridge (Middle Portlandian of the French) is phosphatised in heaps, the Oxford Clay Ammonites are in the condition of limonite. This seems to show that *original conditions* have had something to do with the case. Both Oxford Clay and Kimmeridge Clay Ammonites and casts are often pyritised in their own beds; on the other hand, the Kimmeridge Clay as a rule, especially in the Valley of Aylesbury, has all the appearance of being rich in phosphatic matter. The process of replacement, therefore, whereby the fossil cast became the phosphatic nodule, may have been inaugurated during the progress of denudation, assisted possibly by accumulations of contemporary animal matter due to abundant aquatic and semi-aquatic life. In this way the phosphatisation was probably completed shortly after emergence, and the future coprolites were collected in banks and shallows, to be distributed subsequently, along with lydites and anything that could stand knocking about by the action of waves and currents, throughout the shore deposits of a slightly later period. Hence we venture to suggest that the phosphatisation of the Upware coprolites was effected at some distance from their present billet, and thus that the fragments of Coral Rag were never exposed to the temptation of having their carbonic acid replaced by phosphoric.

The principal object of the essay is of course to describe the indigenous fauna, and to correlate the deposits generally with others of the period, whether British or foreign; the similarity of the Upware and Brickhill fossils to those of the Neocomian beds of the Brunswick area at Shöppenstedt and Berklingen being especially mentioned (p. 73). This, together with the special palæontology, has been very satisfactorily worked out. We have already alluded to the general conclusion based on these investigations, and it only remains to notice some of the more detailed matters.

For instance (p. vi.), the author notes the close palæontological relationship of the ironsand and phosphatic series as found at Upware, Potton, Brickhill, and Farringdon, the great difference in the fauna at Potton being due, he conceives, to the influence of physical conditions. He further alludes to the *special* character of the native forms of life, and to the marked preponderance of Brachiopods, Polyzoa, and Sponges; to the profusion of Brachiopod

shells, both individually and specifically, and the graduation of the various types (species) into one another (p. 22).

In dealing with this latter subject the author has possessed unusual facilities, since himself and his father have availed themselves of the 15,000 Brachiopods collected from Brickhill to arrange sets of connecting forms between recognised species of *Terebratula*, *Waldheimia*, and *Terebratella*. It must not be supposed that between all the species enumerated the connecting morphological varieties are equally evident or of equally frequent occurrence: between some species the passage is simple and clear, both as to the main line and the offshoots, whilst between others much more searching is required to establish the connecting series.

Brickhill indeed seems to have been a centre, as regards the Brachiopoda, of inordinate fecundity accompanied by considerable inosculation of form, just one of those places, in fact, where the oft-demanded missing link was manufactured on a large scale, whilst at Upware and other places on the same horizon the form-groups known as "species" had somewhat contracted their circle of variation. Doubtless almost every zoological group has had its Brickhills in the course of ages, though the chances of preservation and subsequent discovery must limit the number accessible to research. Mr. Keeping, having given us valuable and cogent proofs of the mutability of the forms of Brachiopoda, and apparently somewhat uneasy as to the results of his own conclusions, proceeds to assure us that the value of "species" has been considerably enhanced by these investigations both to the naturalist and the stratigraphist.

Glancing briefly at the part devoted to special palæontology, we learn that the vertebrate remains of Upware are in a great part truly Neocomian species native to the deposit. The probable identity of form of some of the palatal teeth of Jurassic and Neocomian species is insisted on especially in the case of *Sphærodus*.

Coming to the Invertebrata, we find that Cephalopoda are by no means individually numerous; they are for the most part well-known Aptian species. Neither are the Gasteropoda at all plentiful, though some new species are described, including two of *Nerinæa*, both very rare. This is the more remarkable as the uppermost Jurassic rocks of England are, as far as we know, devoid of this genus. The oysters form an important feature, and, excluding the plaited species, greatly resemble those of the Jurassic rocks. Mr. Keeping is convinced that the shell he refers to *Gryphæa dilatata*, Sow., is a genuine native. It is somewhat singular that the Oxfordian *G. dilatata* should have been resuscitated rather than the Portland-Kimmeridge *Ostrea expansa*, Sow., which swarms in the Upper Kimmeridge (Middle Portlandian) of Bucks and in the Portland stone of several localities. On the whole there is a fair list of Monomyaria, including some new species.

Of the Dimyaria one species of *Trigonia* occurs, and is restricted, it would seem, to Upware. The Arcadæ are well represented, three species of *Pectunculus* being given. Of the remaining genera *Cypricardia* and *Cyprina* have the most species, but none are quoted as abundant, though some new species are described. The Brachiopoda, Polyzoa, and Sponges, as every one knows, make up the bulk of the fossils, many of the latter being identical with those of Farringdon.

The table tells us that 151 species are listed from Upware and 88 from Brickhill. Of these 45 occur at Farringdon, 39 at Godalming, 24 at Speeton, 21 at Potton, 19 at Tealby, Shanklin, and Atherfield respectively, 16 in the Hythe beds, 6 in the Folkestone beds, 1 in the Hunstanton Red Chalk, and a doubtful case in the Folkestone Gault.

The book is conveniently got up, not being too large, is well illustrated by Foord, and altogether forms a most desirable volume for the Mesozoic geologist.

W. H. H.

OUR BOOK SHELF

Sound and Music. By Sedley Taylor. Second Edition. (London: Macmillan and Co., 1883.)

THAT this excellent elementary work has at last reached a second edition is certainly in one respect satisfactory. But that nine years should have been occupied in the process, while the "popular" rubbish of the paper scientists has in many cases (or at least is proclaimed as having) annual or biennial reproduction, is matter for profound regret and meditation.

We noticed so fully (NATURE, vol. x. p. 496) the first edition of Mr. Taylor's work, that it is not necessary to say much now. Some of the parts to which we formerly took exception have been considerably modified; in all cases but one, we think, for the better. The one exception is that about the use of the word *force* (or opposite systems of forces) in the explanation of the mutual destruction of sounds by interference.

The word "*timbre*" has been excised, and its place supplied by "quality"; but the hideous misuse of the English word "clang" in the sense of a harmonious combination of sounds still disfigures the later pages. It is time that a definite and suitable nomenclature should be once for all introduced into this part of the subject. There are many words, such as "sound," "note," "tone," &c., which every one seems to think himself entitled to employ as it pleases him, even to the extent of using one of them occasionally in two perfectly incompatible senses. But almost anything would be preferable to a literal transcription of Helmholtz's words into an English book, without regard to the inevitable incongruities.

Southern and Swiss Health Resorts. By William Marcet, M.D., F.R.S. 12mo, pp. 408. (London: Churchill, 1883.)

Nice and its Climate. By Dr. A. Baréty, translated, with additions, by Charles West, M.D. 12mo, pp. 162. (London: Edward Stanford, 1882.)

THIS work of Dr. Marcet is written in an easy, popular style, and gives people very much the sort of information they want. It begins with advice to invalids about to visit the Riviera regarding dress and food, next has something to say regarding hotels, boarding-houses, apartments, and villas; gives some general ideas of social life in the health resorts of the Mediterranean coast, and then proceeds to a more purely climatological description of the Riviera in general and of the particular characteristics of the different towns upon it. Dr. Marcet's residence for some years on the Riviera gives his description of the health resorts there all the accuracy and fulness, without unnecessary detail, which personal acquaintance alone can secure. The same may be said of his description of the health resorts of Switzerland, and his account of the Swiss resorts at low or moderate elevations are particularly interesting and useful. As a guide to invalids the

book is rendered more complete by a short account of Italian health resorts, Algiers, Egypt, Madeira, and Teneriffe. The book will be useful both to invalids who are meditating a winter abroad and to medical men by aiding them in the selection of the proper places to which to send patients.

Dr. Barety's description of Nice and its climate is naturally very much fuller than Dr. Marcet's, and while the latter is a useful guide to the selection of a health resort, the former will be of great service to those who have already fixed upon Nice. The climate of this town varies very much in its different parts, and the proper selection of an hotel or residence is of considerable importance. As a guide in this, and also as a general handbook for reference when residing in the town, Dr. Barety's book is to be recommended.

Vichy and its Therapeutical Resources. By Prosser James, M.D., M.R.C.P., &c. Fifth Edition. Pp. 84. (London: Baillière, Tyndall, and Cox, 1883.)

THIS is a small guide-book to Vichy, pleasantly written. It contains, as usual in such books, a general account of the place, its springs bathing establishments and environs, analyses of the waters, and an enumeration of the diseases in which they are said to be useful. We doubt whether the latter part will be of very much service either to medical men or to invalids; it might, we think, have been omitted with advantage.

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts, No notice is taken of anonymous communications.*

The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to insure the appearance even of communications containing interesting and novel facts.]

Arithmetic Notation of Kinship

MANY writers have endeavoured to devise a simple method of describing the various forms of kinship, which, when expressed verbally, are cumbrous and puzzling in the highest degree. I suspect, however, that if we had always been as familiar with the binary system of arithmetic as we are with the decimal, that the facilities afforded by a numerical system of notation of kinship would have been so obvious that it would have been adopted as a matter of course. The notation I am about to propose is numerical, but it is not binary. It however contains implicitly, as we shall see, owing to the laws that govern numbers, the most important advantages of the binary notation, and it seems better to begin to explain it from the latter point of view.

The number of direct ancestors that a person has in successive generations is . . . 2^4, 2^3, 2^2, 2^1, followed by 2^0 for himself, the corresponding binary notations being 10,000, 1000, 100, 10, 1 respectively. We also see on a little reflection that, this being the case, every direct ancestor in the nth degree admits of being specified by a particular number, consisting of $n + 1$ places of figures. Thus the two parents may be represented by 10 and 11, the four grandparents by 100, 101, 110, 111, and so on. Let us draw up two schemes of ancestral roots, identical in arrangement, but using in the one the symbols of f for "the father of," and m for "the mother of," and employing binary notation in the other:—

child

f *m*

ff *mf* *fm* *mm*

fff *mff* *fmf* *mmf* *ffm* *mfm* *fmm* *mmm*

&c. &c.

1

10 11

100 101 110 111

1000 1001 1010 1011 1100 1101 1110 1111

&c. &c.

We see (*a*) that if we disregard the child, and speak only of his or her ancestry, all the even numbers apply to one sex and all the odd ones to the other; (*b*) that each term is derived from its ancestral terms in so simple a way that it carries on its face every step in the line of descent, however long it may be, through which each ancestor is related to the child. Therefore, as I began by saying, if we were familiar with decimal notation, we should long since have described each form of ancestry by it. Instead of saying that "B was a grandmother, namely, a father's mother of A," we should have said "B was 101 of A." Or again, instead of saying that "C was first cousin once removed to D, the father's father's parents of C being the mother's parents of D," we should have said "the 1000-1 of C are the 110-1 of D." The case might have been one of half-blood, say by the father's side, then "the 1000 of C would be the 110 of D," a notation which grows in simplicity as the verbal equivalent grows in complexity.

Being, however, unfamiliar with binary notation, we fall back on the decimal, and translate the above numbers into their equivalents, which are those I propose for the arithmetic notation of kinship, as entered in the table below.

Table of Ancestral Roots

Grade of kinship.	Father's side.				Mother's side.			
Child				1				
Parents		2				3		
Grandparents ...	4		5		6		7	
Great-grandparents &c.	8	9	10	11	12	13	14	15
		&c.				&c.		

The sex of 1 is unspecified, it is equivalent to the word "child," but all other odd numbers refer to females, and all even numbers refer to males. If n is the register number of any ancestor, the register numbers of his parents are $2n$ and $2n + 1$. We can thus construct or analyse any register number with great facility. It is not worth while giving an example of construction, but I may give one of analysis. I write down the number and append to it a series of successive halvings, so far as the numbers are, or come out, even; otherwise I substract 1 before taking their halves. Then I write f (= father of), or m (= mother of), as the case may be, below each entry. Let 253 be the number, then I get—

253	126	63	31	15	7	3	child
m	*f*	*m*	*m*	*m*	*m*	*m*	child

For purposes of exhaustive inquiry into the antecedents of a family, this notation has the advantage of an index, and of showing very compendiously how much has been done, and how much remains to do.

FRANCIS GALTON

"Stachys palustris" as Food

THERE is no reason to think that *Stachys palustris*, L., is anywhere used now for food in the British Isles. The cultivation of the potato must have long since put it out of court for any such purpose. But that it was once so employed there seems abundant evidence. The part eaten, however, was not the "rhizomes," but the subterranean tubers. That the use of these is now quite forgotten may be inferred from the fact that the tubers themselves are not even mentioned in standard systematic books. Yet Irmisch (see *Botanical Gazette*, vol. ii. p. 293) gives the potato and *Stachys palustris* as well known instances of dicotyledonous plants producing stem tubers which become detached by the dying away of the older parts of the parent plant which produced them.

Johnson, in the second edition of Gerarde's "Herbal" (1636), has nothing to say about the edibleness of the tubers. But he

evidently thought that they required notice. He has had them added to a figure of the plant which he reproduces from "Tabernæmontanus," as he explains, "with addition of the joynted tuberous roots as they are in Winter; yet by the Caruer's fault they are not altogether so exquisitely exprest as I intended." Withering ("Botanical Arrangement of British Plants," ed. ii., 1787, p. 613) has, "The roots, when dried and powdered, will make bread." And in Bromfield's "Flora Vectensis" (1856), the note occurs:—"The roots of *S. palustris* are said to become edible by cultivation. See Curtis, 'Brit. Entom.,' vi." This last is a book to which I have not convenient access; but the reference may give your correspondent a useful clue.

I may mention that I do not find the name "Bare Horehound" given at all in Britten and Holland's very comprehensive dictionary of English plant-names. The old name for *Stachys palustris* was "Clown's All-heal."

W. T. THISELTON DYER

Garfish

HAVING been absent from England for some time, I have only just noticed the two letters published in NATURE for July 5 and 12 (pp. 226 and 245), on "Garfish." I have little doubt that the fish described by Mr. S. Archer as having cut a slit in a felt hat was, as he believes, a garfish, a large Belone, not a Hemirhamphus, and not a swordfish or sawfish of any kind, as suggested by Mr. Goodsir. It is the constant habit of large Belones, some species of which attain, according to Dr. Günther, a length of five feet, when startled to move along the surface of the water by a series of rapid bounds for thirty or forty yards at a time, with astonishing rapidity. I have often seen them thus spring out of the water when scared by a boat. I was told that in some of the Pacific Islands these fish not uncommonly cause the death of the natives, who, when wading in the water, have their naked abdomens speared by the sharp snouts of the fish, with the result of causing peritonitis. The fish appear to bound blindly away from danger, and strike any obstacle in their way haphazard. As a good many natives wade in together in many of their fishing operations, as at Fiji, for example, where one party drives the fish into the nets held by another, such accidents may easily occur. I do not think a sawfish could possibly jump over a boat. I have described the jumping habits of the large garfish, and alluded to their fatal effects in "Notes by a Naturalist on the *Challenger*," p. 480.

H. N. MOSELEY

Continuous Registration of Temperature

IN your issue of July 26 (p. 306) there is a description of an apparatus lately devised by Messrs. Negretti and Zambra, by which a record of twelve temperatures in succession can be obtained by the somewhat elaborate arrangement of twelve thermometers, a clock, and a series of electromagnets and battery. I wish to bring under your notice a simple machine invented by Mr. Bowkett, late resident medical officer of the Leeds Fever Hospital, by which a continuous record of atmospheric temperature can be obtained by means of an apparatus consisting solely of a "bourdon" steam gauge tube, a clock, and a writing lever, costing little more than a few shillings.

Mr. Bowkett devoted great mechanical skill during several years of experimentation to the perfecting of a form of this instrument sufficiently small and accurate to be used for medical purposes, *i.e.* to register the temperature of the human body. For this purpose the instrument has to be somewhat more complicated, and accordingly more costly. Many of these are in use in our hospitals and elsewhere, and are of the greatest possible value.

These instruments can be made of any size, and when large are of very great strength, and might easily be applied for thermal regulation by attachment to valves or other ventilating arrangements. The instrument constructed by Mr. Bowkett for registering the temperature of rooms was of the size of a small clock, of the simplest possible character, requiring very little care in its use. Messrs. Salt of Birmingham are the licensees of the patent.

ERNEST M. JACOB

12, Park Street, Leeds

Aurora and Thunderstorm

A DISPLAY of aurora was seen here on the 30th ult., which may perhaps be of sufficient interest for insertion in NATURE.

A thunderstorm passed from west to south during the afternoon. Thunder and lightning commenced between 3 and 4 p.m. and continued till about 9 o'clock. The storm centre was about two miles from the city; no rain fell here, though a heavy hail shower fell to the west in the afternoon. Lightning was vivid till past midnight in the south. From 11.30 to midnight an auroral light passed over the zenith from west to east, of well defined nebulous light. It was 10° in width as measured by a sextant. This was joined on the north by a horizontal band of aurora 18° altitude. There were no great flashing lights from this.

On the northern horizon was a small arc throwing up short flashes. The horizontal band was the brightest.

The temperature and force of wind and barometer readings were as under:—

Barometer.	Wind.	Temperature. Dry Bulb.	Wet Bulb.
3 p.m., 29·857 ...	...	79°·8 ...	71°·9
6 ,, 29·855 ...	...	71·4 ...	64·0
9 ,, 29·862 ...	S.S.E. 5 miles ...	66·4 ...	64·0
12 ,, 29·851 ...	S.S.E. 4 miles ...	60·9 ...	60·3

Maximum shade reading of the day, 83°·5.
,, ,, of the 31st, 82°·0.

The observatory is 764 feet above the sea level, and I am indebted to the observer for the above figures.

ALAN MACDOUGALL

Winnipeg, Manitoba, August 10

A Complete Solar Rainbow

ON Thursday, August 16, while R.M.S. *Norham Castle* was in lat. 2° 20′ N., long. 13° 58′ W., a phenomenon entirely new, at least to the officers and passengers on board, appeared at 11 a.m., and lasted until 12.30 p.m. This consisted of a complete rainbow round the sun, when nearly and at the zenith, having an inner diameter—taken by Capt. Winchester, R N.R.—of 43° 08′. The day was bright and warm, with a slight haze above. The rainbow appeared to crown the whole of the upper dome of the sky, and to possess all the normal colours, only very slightly dimmed. Whether connected with this appearance or not I cannot say, but the next two days were squally, with heavy rains.

D. MORRIS

Kew, September 5

Animal Intelligence

I AM a constant reader of NATURE, and have read with much pleasure the several instances recently communicated by correspondents of animal intelligence, a subject in which I take great interest.

It has struck me that some of your readers might in turn be interested in hearing of the intelligence and powers of observation of a collie bitch called "Winifred," my constant companion.

In one of the fields attached to my house there is a large pond well stocked with fish, and especially with eels. I very often spend an evening fishing for these latter, using several lines at different points round the pond, the rods lying on the grass, each one receiving my attention whenever its respective float indicates that there is a bite.

The collie "Winifred" is constantly with me on these occasions, and has always taken the greatest interest in her master's proceedings, watching every movement most intently. It was for a long time a source of considerable amusement to me to notice that by constant observation the dog had come to understand the connection between the bobbing and final disappearance of a float and the subsequent exciting proceedings of pulling up an eel, disengaging it from the hook, and putting it into the creel. The cocked ears, head on one side, and eager eyes of "Winifred" when she saw a float bobbing gave plain proof that she was as much interested in the fishing as her master.

One evening some six weeks ago it happened that I was at one end of the pond baiting a hook, while the dog had remained at the other end, lying on the grass close to one of the rods. Suddenly I observed her showing signs of excitement, and, on looking across, saw one of my floats finally disappearing under the water. As I did not come, "Winifred" got very excited indeed, uttered one or two sharp yelps, and ended by seizing the rod in her mouth and "backing" with it, attempting to pull out the line from the water. I hurried to take the rod from her, fearing the effects on my tackle of the lack of skill of this canine

disciple of Izaak Walton! There was a goodly eel on the hook, sure enough.

Since then "Winifred" has once again attempted to pull out the line under exactly similar circumstances. Surely this conduct shows powers of observation and of inference of no mean order?

I may add that the collie is now three years old. She saw me fishing many times last summer, and, as I said before, always showed great interest in what was going on. But it was not till six weeks ago that I had any idea how much she was profiting by what she saw.

Perhaps some of your other readers who fish, and are accompanied by intelligent dogs, may have observed similar instances of reasoning power. Seeing to what perfection dogs can be trained to take part in other branches of sport, perhaps it is not very surprising that here and there one should show a little appreciation of the leading points of the "gentle art" of angling.

MORGAN J. ROBERTS

The Hollies, Cwm Newydd, Holywell, North Wales, August 31

Copper and Cholera

NEED we go to Sweden to test the theory that copper is a preservative against cholera? The year before the 1865 epidemic I travelled by train past Swansea, and my attention was called to the utter want of verdure in the surrounding country, due, I was told, to the copper fumes.

Now, according to the official report, the deaths from cholera in Swansea were 88 in 10,000 in 1866, in Neath 79, in Llanelly, 76—all places in the same neighbourhood; thus showing a far greater mortality for the copper-smelting district than any other in England or Wales. The mortality for all England was only 13 in 10,000, and for London 18. The only two places which in any degree approach Swansea are West Ham with 50, and Liverpool with 54; in both of which it is well known cholera was especially severe. The epidemics of 1849 and 1854 present Swansea in a more favourable light.

Perhaps some of your Swansea readers, by giving the number of deaths—if any—among the actual workers in the metal, can help those who, like myself, are inclined to believe in copper as a prophylactic; in what way I scarcely know, unless it be according to the principles of homœopathy, as my experience on three occasions—and a lively time I had of it—lead me to believe that copper added to plums to preserve their colour should be eschewed, at any rate in cholera times.

Dulwich, September 1 B. G. JENKINS

The Meteor of August 19

THE same meteor was undoubtedly seen by Mr. Crispin at Wimbledon, Mr. Pooley at Cheltenham, and myself at Llandudno, and I think I can remove Mr. Crispin's difficulty.

The apparent fall of meteors towards the earth is generally an effect of perspective. An object at a great height moving directly away from the observer appears to move perpendicularly downwards. If moving away obliquely to right or left, it appears to have a more or less horizontal path with a downward inclination.

This meteor was evidently not moving towards the earth, but was one of those that skim the upper atmosphere, white-hot at their surfaces while the resistance is sufficient, and dark again as soon as they pass into a thinner medium. I suppose it to have first become luminous when directly over Essex, not far from Chelmsford, at a height of about seventy miles, passing northeast over the sea, and vanishing near the Texel. Its appearance along such a path would agree very fairly with the three observations, except that, if Mr. Pooley saw it first quite south-east by compass, it must have been luminous for a second or two before Mr. Crispin or myself observed it, and the starting-point would be nearly over London.

I was wrong at first in referring to the Yorkshire coast. The visible path was clearly south of the Humber.

ALBERT J. MOTT

Crickley Hill, Gloucester, September 2

THE ISCHIAN EARTHQUAKE OF JULY 28, 1883

SINCE my last letter to NATURE most of my time has been occupied in visiting different parts of the island, and although there are still a number of objects to be carefully examined the general features of the catastrophe I hope to have cleared up.

The actual moment of the earthquake is unknown, but seems to have been about 9.25 p.m.; so, supposing the shock registered at Naples and Vesuvius to be identical with that of Casamicciola, had the observation of time at the latter locality been correct, we could calculate the velocity of transmission, but which it is to be feared is impossible.

As mentioned in NATURE, the shock was preceded by general seismic disturbances throughout Southern Europe. In the island itself we have the most contradictory statements as to premonitory signs and symptoms. One gentleman noticed on two occasions previously his watch, which was suspended by a nail to the wall, swing backwards and forwards. The assertion about the water at Gurgitella being much hotter some days before is of little value without proper thermometric observations, since it is known commonly to vary 20° C., and may reach more than 40° from time to time, and I am acquainted with a thermal spring at Bagnoli, near Naples, that varies 23° C., ranging from 13° to 36° C. Perhaps the most remarkable of these kinds of statements was couched in these words:—"The syndic of Serrara Fontana (a town on the south of the island) telegraphed to the Minister of Public Affairs to the effect that in that country a fissure one kilometre long, thirty metres broad, and of unknown depth, from which were issuing dense columns of vapour." On reading this I started immediately for Serrara, and there the syndic placed at my disposal his two informants as guides. After a climb of three hours and a half along the almost impassable sides of Epomeo, we came to its northern slope over Lacco Ameno, with the two landslips I had visited and photographed thirty-six hours after the shock. The fissures were such as take place along the edges of all landslips. No vapour was issuing, and its presence for a short time after the earthquake could be easily explained: the locality is part of the old fumarole area of Monte Cito, where alum was manufactured centuries ago; the rock is much decomposed by the continual escape of acid vapour, and only required the earthquake to shake it down; when the displacement took place a large surface of hot and moist tufa was exposed, and no doubt for some time gave off a quantity of vapour.

It will be seen that not a single point of the size, locality, and characters of the fissure described by the newspaper was correct or free from gross exaggeration. I have visited with care all similar sites of supposed fissures, but after some days of want of shelter, sleep, an abominable starvation diet of bad bread and rotten cheese, combined with continual climbing from daybreak to sunset in an extraordinarily hot Neapolitan summer in the hope of finding some evidence of volcanic action, I did not meet with the slightest success. I was accompanied in these excursions by my friend, Prof. P. Franco of Naples, who shared my disappointment and disgust. Holding as I do the volcanic nature of the earthquake, the appearance of any such phenomena would have been greedily accepted.

If we draw isoseismal lines over the injured districts, we find that they assume the form of elongated ellipsoids whose major axes run nearly east and west.

The fourth isoseismal area, in which houses are only very slightly fissured, not only includes the whole island but must extend into the sea some distance.

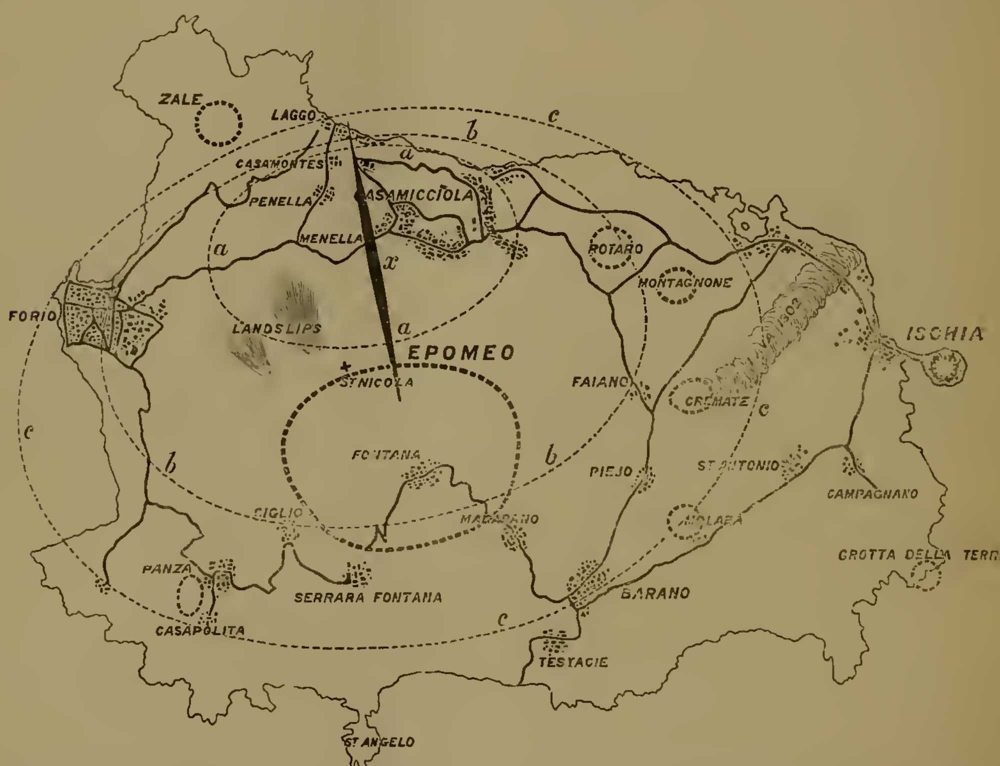
One remarkable fact is the manner in which the houses of the marinas have suffered much less than others in their immediate neighbourhood, or even farther away from the seismic vertical. This is no doubt due to their foundation reposing on sea sand, which, from the looseness of its particles and therefore inelastic nature, acted as a mattress and absorbed the earth waves. The same fact is observable in all buildings that have their founda-

tions of the loose alluvial tufas and ash of the plains and valley bottoms, whereas the destruction of houses built on the rock was terrible, thus reversing the well known parable.

Putting aside, however, the speculative side of the question, let us look to facts. From a careful examination of observed azimuths and angles of emergence all point to a plate-shaped focus, whose strike extends in a line from Fontana, just west of Menella, to near the beach at Lacco. The plane of this fissure is probably roughly perpendicular to the surface, but may slightly dip towards the east as the isoseismals are slightly nearer on the eastern side of the seismic vertical, which as a necessity is not represented by a point, but a line on the surface. The rupturing of this plate-like fissure was apparently greatest at a point nearly midway between its extremities.

The remarkable fact that I observed in the earthquake of 1881 that at Fontana the shock was almost vertical like that at Menella is again repeated; for which I then proposed as an explanation the conduction along a tube or column of highly elastic trachyte, filling the old chimney of Epomeo, whilst the surrounding districts were protected by the less elastic mattress of subjacent tufa. On the present occasion Fontana, as before, has all its houses fissured in such a manner as to demonstrate a vertical shock; but besides there is another set of cracks which show a north to south path for a wave at a low angle of emergence. At St. Nicola part of the altar has rocked in the same direction, and between these two localities a rock has been ruptured and projected to the south. Whatever may be the explanation accepted, the fact remains as unique in seismological history.

That Vesuvius did not, or only to a slight extent, sympathise with the seismic movement in the island of



Sketch Map of the Island of Ischia. Scale 1 to 80,000. Isoseismals marked thus ....; ancient eruptive centres and craters in dotted circles. *a*, mesoseismal area = total destruction; *b*, first isoseismal = many houses fallen, rest require rebuilding; *c*, second isoseismal = severely fissured. The third isoseismal does not seem to cut the land; its limits, therefore are unknown. *x*, fissure, or focal centre.

Ischia is no proof against the volcanic origin of the earthquake. Admitting the hypothesis of a seismic wave traversing a large tract of volcanic matter underlying Southern Europe, such a wave may produce very slight variation at volcanic vents, but may yet be sufficient to determine the extension by rupture of a fissure, where the resistance of the rock, and the tension of the volcanic matter near the point of extension, are nearly equal. In a paper lately read before the Geological Society of London I endeavoured to prove that the explosive violence of lava is due to the assimilation or solution of water taken up from the water-bearing strata it traverses in its passage from the main source towards the surface. Under such conditions we may have very violent phenomena produced, locally, without any sympathy in neighbouring volcanic vents; which at the same time explains I believe one volcano bursting out in a violent paroxysmal eruption, whilst a near companion is in no way affected. Why therefore should an abortive paroxysm disturb a neighbouring active vent, as in the present example?

In various points that I have examined the coast of the island, there was no apparent change of level, nor did any success attend endeavours to discover any signs of depression of the surface.

It has been stated that the sea receded, but I could not obtain any confirmation of the fact. As the steamer that lay at anchor in the roads felt the shock, it may probably have been due to the disturbance of the water by the earth waves.

As far as my inquiries have gone, the first symptom was a distant sound like that of a carriage, almost immediately accompanied by a tremor, then a terrific explosion shading off into a number of reports. Most people not in the mesoseismal area felt first the "susultatorio" or vertical movement, followed by the undulatory, or, more properly, lateral motion. This, as is well known, is due

to the arrival first of the emergent wave, followed by the progress outwards and along the surface of others from the seismic vertical. On the contrary, those in the meso-seismal area felt the blow and report apparently simultaneously; the walls fell before any attempt at an escape could be made. It appears therefore that the sound waves travel faster than those of the earth, though the difference in arrival is inappreciable at short distances from the seismic vertical.

Since the principal shock the following minor ones have been felt:—

July 29,	1883	... ...	Slight shock in the morning?
Aug. 1,	,,	... ...	,, 3.10 p.m.
,, 1,	,,	... ...	,, 4 50 ,,
,, 1,	,,	... ...	,, 11.15 ,,
,, 2,	,,	... ...	,, 2.30 ,,
,, 3,	,,	... ...	,, 1.15 or 2.15?[1]
,, 8,	,,	... ...	,, 10.40 a.m.
,, 12,	,,	... ...	,, morning?

The accessibility of the island, the advanced state of our geological knowledge of it, and the small extension of the earthquake area, make it most suitable and convenient for the study of its terrestrial movements. What is required is a number of seismographs scattered over the island, which should be capable of registering azimuth, angle of emergence or molecular velocity, with the exact time of each movement so as to obtain velocity of transmission. These should be distributed in two circles around the seismic vertical, and should be at least sixteen in number, eight being in each circle, one or more for registering vertical waves to be placed along the seismic vertical. Accurate thermometric measurements of the principal fumaroles and mineral springs to be registered hourly, and if possible some device for measuring quantity of outflow of mineral waters, and pressure of vapour in fumaroles. To these it might be useful to add microseismic observations. The changes in sea level would be of interest if compared with those of Naples.

The principal expense would be providing the instruments, which could be placed in caves cut in the solid tufa, of which there are hundreds in the island that could be obtained for almost nothing, if not entirely free of expense.

By such means we might study the true nature of these shocks, the progress of the focus towards the surface, and verify whether any premonitory signs are to be depended upon preceding an earthquake.

I would impress on all persons charitably inclined that money spent on such an enterprise would be productive of far more good than when distributed to be spent in rebuilding the perilous houses of masonry in preparation for another catastrophe. Not six days after the terrible event, masons were at work repairing the most dangerous walls, and many inhabitants have already returned to reside in their fissured and crumbling abodes. Besides, if another shock occurs more violent than the last, a large number of additional localities would suffer, such as Forio and Ischia, besides the villages on the south coast of the island. H. J. JOHNSTON-LAVIS

P.S.—In collecting evidence of the Ischian earthquake a very remarkable fact was communicated by Mr. Petersen, the engineer of the Zoological Station at Naples. Whilst dredging on the north side of the unfortunate island, about opposite the cemetery of Casamicciola, a number of pieces of pumice were found floating on the water, some of them as large as a man's head; they had quite a fresh appearance. The conclusion is that there has been a submarine eruption somewhere near the island. Such would explain the sensations felt on board the steamers and the apparent disturbance of the coast line. On the other hand it is strange that the eruption left no other signs, and that nothing was observable the next morning. No dead fish were noticed. The pumice might be derived from loose deposits containing that material, which form some of the sea cliffs which were shaken down by the earthquake. Whatever be the real cause, we propose to investigate it thoroughly by dredging and diving, as the water rarely exceeds twenty to thirty fathoms at the most. H. J. J.-L.

Naples, August 31

[1] Which was much stronger and produced slight damage.

THE BERNISSART IGUANODON[1]

THE wonderful discovery of remains of Iguanodons made at Bernissart in 1878 caused quite a sensation amongst naturalists at the time, and the publication of the scientific results of that grand find have been awaited ever since with eager expectation. Nevertheless, as five years have elapsed since the discovery was announced, it is well that the memory should be refreshed by a few brief details as to the circumstances of the find itself before the results as to the nature of the Iguanodons themselves, lately made public, are referred to. Bernissart is in Belgium, situate between Mons and Tournai, close to the French frontier. In the spring of 1878, in one of the galleries of a coal mine there, were discovered in Wealden clays a large number of bones. Specimens of these bones were forwarded to Professor P. J. van Beneden, who at once recognised them as belonging to Iguanodon.

It is to M. Fagès the director general of the Bernissart Mining Company that the discovery is due. He interested himself greatly in the matter, and from first to last the mining company has most generously and meritoriously devoted its best resources to the recovery from the depths of the earth in the most perfect condition possible of these most remarkable scientific treasures. It has presented them all to the Royal Museum of Brussels. The actual removal of the specimens from their beds and their transmission to the surface, was performed under the immediate superintendence of Mr. Gustave Arnould, chief engineer, and of M. De Pauw, the latter being the superintendent of workshops at the Brussels Museum, who has since successfully mounted the enormous skeleton shown in the accompanying engraving. So immensely abundant were the remains found to be that Mr. De Pauw assumed for three years the habits of a miner, watching and controlling the removal of every specimen. He invented an ingenious method of hardening the bones *in situ* which prevented their crumbling when exposed to the air, which at first occurred. The bones exposed on the surfaces of the blocks excavated were covered with a coating of plaster for protection, and the masses thus formed were then raised to the surface, a distance of more than 1,000 feet and removed to cellars under the natural history galleries of the Brussels Museum, to be worked out at leisure. M. Dupont, Director of the Museum, confirmed Professor van Beneden's determination of the bones, and at the same time fixed the exact age of the deposits in which they occurred.

Some surprise has certainly been felt by naturalists that so very little information about the Bernissart skeletons has been published during the time which has elapsed since their discovery, but it must be borne in mind that it took three years even to get the rough material out of the pit, and that every mass of matrix containing bones requires a great deal of most careful labour to be expended on it before the bones in it are fully exposed for study. M. L. Dollo, a distinguished former pupil

[1] M. L. Dollo, "Première Note sur les Dinosauriens de Bernissart." *Bulletin du Musée Royal d'Hist. Nat. de Belgique*, T. i. 1882. Deuxième note, *Ibid.*, *l.c.* Troisième note, *Ibid.*, T. ii. 1883. "Note sur la présence chez les oiseaux du Troisième Trochanter des Dinosauriens et sur la function de celui-ci," *Ibid.*, *l.c.* "Les Iguanodons de Bernissart." *Bulletin Scientifique de pédagogique de Bruxelles*, April 1, 1883, No. 2, p. 25

of Prof. Giard of Lisle, was appointed about two years ago as assistant naturalist to the Museum for the purpose of investigating the Iguanodons. He is full of enthusiasm, as an ardent naturalist such as he is well may be with the whole Bernissart material before him. He works incessantly at the subject, but he does not see prospect of publishing the complete monograph on the Iguanodons which he intends to issue sooner than five or six years hence. He will not of course venture to prepare the final monograph until he has the whole of the material concerned before him. He estimates the number of individuals represented by skeletons in the find as twenty-three, two of which belong to the species *I. Mantelli*, and twenty-one to *I. Bernissartensis*. Of these twenty-three, fifteen have as yet been chiselled out of the blocks ready for study, eight remain as yet to be worked at, and although four or five skilled artificers are constantly at work on the specimens progress is necessarily slow. The cellars full of the material present an astonishing appearance. One first enters an extensive, dimly lighted vault, the whole floor of which is covered with large blocks, many still in the condition in which they came from the mine, of all shapes, and lying in all sorts of positions, so closely placed that it is very difficult to get about amongst them to inspect them more closely. All contain huge bones, forming parts of the skeletons of the Iguanodons, often covered up by the protective plaster, but with here a hand, there a foot, elsewhere a range of vertebræ showing out. In an adjoining cellar is the workshop where various blocks are seen in the process of the removal of the matrix, whilst at one end, hung up to stout beams, are the results of the operation, a vast collection of all imaginable segments of the skeletons of Iguanodons suspended in the air, and suggesting the idea of joints of meat in the shop of some Brobdingnagian butcher.

As before mentioned, one of the skeletons of *Iguanodon Bernissartensis* has been restored and mounted by Mr. J. F. de Pauw. The specimen is almost entirely complete, only a few phalanges and one or two minor details having required to be reconstructed. It was not found possible to detach the bones from one another before mounting them. They are mostly mounted still joined to one another in sections by the matrix as removed from the mine. It was therefore impossible to give to the skeleton as natural a pose as might have been wished, and as M. de Pauw hopes to accomplish with some of the other specimens more favourably preserved; but taking all circumstances into consideration the present result of his work is a marvellous success, in which it needs a very trained eye indeed to detect anything amiss. The grand skeleton is set up in a huge glass chamber in the court of the Museum. As it stands in the natural attitude of progression of the animal on land, erect on its hind limbs, the top of its snout is at an elevation of a few inches over 14 feet from the ground, whilst from the tip of the tail outstretched behind to a point immediately beneath the tip of the snout the skeleton covers a horizontal space of floor about 23 feet in length.

As soon as M. Dollo set to work on the details of the structure of the Iguanodons, he very wisely determined to publish at once a series of preliminary notes giving the main results of his investigations. Four of these have now been issued as enumerated at the commencement of the present article, and from the third memoir is copied the figure of the entire skeleton, here reproduced somewhat reduced in size. From these notices is taken the information which follows.

M. Dollo's first care was to determine the species of the Iguanodons with which he has to deal. It will be remembered that his predecessor, M. G. A. Boulenger, who left Brussels to join the zoological staff of the British Museum, recognised among the remains a new species of Iguanodon, characterised by having six sacral vertebræ instead of five as in *I. Mantelli* and four in *I. Prestwichii*. Professor P. J. van Beneden, however, in the absence of further detailed information, held the opinion that the number of the sacral vertebræ could not be regarded as a specific character amongst Iguanodons, and that our knowledge then on the matter could only be expressed by stating that in the Dinosauria the sacral vertebræ vary in number from four to six. He did not therefore accept M. Boulenger's determination as valid, but regarded the whole of the specimens as belonging to *I. Mantelli*. M. Dollo, however, confirms M. Boulenger's conclusions; he finds that there are two forms of Iguanodons present, a large one and a small one, and the small one is certainly not the young of the large one. It is a remarkable fact that there are no young examples amongst the whole of the Bernissart Dinosaurians, as is shown by the facts that in all of them the cranial sutures are obliterated, and the sternal bones fully ossified, that the neurocentral sutures have disappeared in all the vertebræ and that the osseous tissue is equally dense in all the specimens. Traces of young have been most carefully sought for, but most unfortunately not a bone of a young animal has been found.

The differences between the two forms of Iguanodon are also not merely sexual. They are well marked and certainly of specific value. The number of sacral vertebræ seems to be quite constant in the several species of Iguanodon, and Prof. Marsh, who has had several hundred individuals of Dinosaurians through his hands, representing numerous genera and species, has made use, amongst other characters, of the number of sacral vertebræ present as generic distinctions. After carefully comparing full size drawings of the bones with those of the type specimen of *I. Mantelli* (Owen) in the British Museum, M. Dollo is quite convinced that his smaller form with five sacral vertebræ is identical with this. There are two other well identified species of Iguanodon known, namely, *I. Prestwichii* and *I. Seeleyi* of Hulke. The larger form from Bernissart cannot be *I. Prestwichii*, which has only four sacral vertebræ, but it is just possible that it may be identical with *I. Seeleyi*, since its large bones resemble closely those described by Mr. Hulke as characteristic of that species. There is, however, this remarkable discrepancy. Mr. Hulke discovered bony plates, forming as he believes a dermal armour over the tibia of *I. Seeleyi*. Now amongst the remains obtained from Bernissart are specimens of the integument of both *I. Mantelli* and the larger form. And these indicate that the skins of both these animals were either quite naked or at the most covered with epidermic scabs. M. Boulenger's name, *I. Bernissartensis*, is retained for the larger Bernissart form, for even if *I. Seeleyi* should prove in the end to be identical with it that name must fall through lack of priority. M. Dollo, taking into consideration the results as yet attained by him, characterises the order Ornithopoda of the Dinosauria to which the family Iguanodontidæ belongs as follows :—

Ornithopoda.—Foot digitigrade, ungulate, five functional digits on the hand and from three to four on the foot. Pubis projecting freely in front; post-pubis present. Vertebræ solid. Anterior limbs reduced, limb bones hollow. Premaxillaries toothless, at least in their distal region.

And the family Iguanodontida thus :—

A single row of teeth. Three functional digits on the foot. Two symmetrical sternal plates.

The pair of sternal plates were mistaken by Professor Marsh, who studied them in specimens in the British Museum, for clavicles ; and the presence of clavicles was included by him in his definition of the family Iguanodontidæ ; but in the Bernissart specimens the pair of bones are found in many specimens preserved in their natural relations, and are seen at once to be sternal, clavicles being altogether absent. A specimen is figured by M. Dollo, in which the two sternal bones with the coracoid and scapula of one side are seen *in situ*, all in their proper

relative positions, and the humerus with its head still within the glenoid cavity. The circumstance that in the case of these Bernissart skeletons the bones are so largely preserved in their immediate natural relations adds immensely to their importance, for the position of every bone can be determined with certainty. The nearest approach to the peculiar structure of the sternum in Iguanodon appears to M. Dollo to be that existing in some young birds, especially in *Vanellus cristatus* as figured by Parker. Professor Marsh regarded the supposed presence of clavicles in Iguanodon as an important point in them of resemblance to birds; the point must now drop, but there are abundance of others in the Iguanodon skeleton in which the remarkable resemblances between the Ornithopoda and birds, pointed out by Professor Huxley with such surpassing sagacity more than twelve years ago, are borne out in a most remarkable manner. Professor Huxley had very imperfect material to guide him in his ideal restoration of the Iguanodon skeleton, and it is wonderful in how few matters of detail his results need correction now that one can stand at Brussels with a perfectly complete skeleton of Iguanodon towering over one's head, and test his results with as it were a complete solution of the puzzle at command. First of all there seems to be little doubt possible that the Iguanodons walked, as he pointed out, on their hind limbs erect like birds, in somewhat the attitude shown in the accompanying figure. Several

Iguanodon Bernissartensis, B'gr. At the Brussels Royal Museum of Natural History. Restored and mounted by M. L. F. De Pauw. Head, *a*, left nostril; *b*, left orbit; *c*, left temporal fossa. Vertebral column, *d*, cervical region; *e*, dorso-lumbar region; *f*, sacral region; *g*, caudal region; *h*, left scapula; *i*, left coracoid; *k*, left humerus; *l*, left ulna; *m*, left radius; *n*, sternum; *o*, left ilium; *p*, left pubis; *q*, left post-pubis; *r*, left ischium; *s*, left femur; *t*, left tibia; *u*, left fibula; *v*, third (fourth) trochanter. I, II, III, IV, V, digits. *X*, diagrammatic transverse section of the body between the fore and hind limbs.

different lines of evidence, as M. Dollo points out, tend to prove this. Firstly the remarkable resemblances between the structure of the pelvis and the posterior limbs of birds and the corresponding parts in the Iguanodons. The points of resemblance of the ilium and ischium, pointed out by Professor Huxley, are fully confirmed by the Bernissart specimens; with regard to the pubis Huxley only recognised a part in Iguanodon, the post-pubis; and Hulke was the first to give a nearly correct figure of the whole. The actual pubis is very large in Iguanodon, as will be seen in the figure, and projects forwards and outwards, forming an obtuse angle with the post-pubis. Mr. Hulke was therefore not quite correct in his conclusions as to its attitude, and there is no symphysis pubis present; the post-pubis is long and slender, and directed backwards alongside the ischium, as in birds, for a considerable distance beyond the ischial tuberosity. It is not incomplete, as supposed by Marsh (from the examination of drawings of Bernissart specimens in which it was imperfect). M. Dollo is inclined to follow Professor Marsh in identifying the Dinosaurian pubis with the pectineal process of the pelvis of birds, a conclusion which receives most interesting support in the valuable memoir lately published by Miss Alice Johnson of Cambridge on "The Development of the Pelvic Girdle in the Chick,"[1] in which it is shown that in the embryo fowl the cartilaginous representative of the pectineal process is at first much larger and more prominent in proportion to the dimensions of the pelvis than subsequently, and becomes gradually reduced as development proceeds. The peculiar form of the

[1] *Quarterly Journal o Microscopical Science*, July, 1883.

pelvis is no doubt directly connected with the muscular requirements concerned in the erect posture, originated probably in the Dinosauria, and transmitted to birds, in which it has been improved upon by the elimination, almost complete, of the original pubis through disuse.

M. Dollo takes the view that the post-pubis is a bone peculiar to Dinosaurians and birds. As he pointed out to me in the mounted specimen, probably a male, the aperture inclosed between the two ischiatic bones posteriorly is a very narrow slit through which, if the Iguanodon was by any chance oviparous, no egg of size proportionate to the animal could have passed, and it is, he thinks, just possible that in females he may find the ischia bowed so as to inclose a widely open passage above the symphysis.

In a separate memoir M. Dollo has pointed out an additional resemblance in the femur of Iguanodon to that of birds to those already pointed out by former observers, namely that the third trochanter present in the former is represented, though feebly, in the femur of many birds. This third trochanter in birds, as he has shown by dissection in the duck serves for the origin of a small muscle first described by Meckel, which is attached to the tail, and by which the lateral movements of the tail are performed; he terms the muscle "caudo-femoral." The great development of the third trochanter in Iguanodon must, he concludes, have been in relation with very large similar caudo-femoral muscle concerned in the movement of the immense tail of the animal in the act of swimming. For reasons which he gives, he proposes to call the trochanter in future the fourth trochanter. It is not necessary to enter here into the further well known details in which the hind limb of Iguanodon shows intimate resemblance to that of birds, and especially in birds in the young condition.

The reduction of the anterior limbs in proportion to the posterior and their difference in structure are further evidence, though not conclusive, of the erect posture of the Iguanodons. In *I. Mantelli* the fore limbs are of about half the length of the hinder, whilst in *I. Bernissartensis* the difference is less, the proportion in length being two-thirds to one.

The reduction in the volume of the head and thorax as compared with those of quadruped reptiles is further evidence on the same side. The head is comparatively small and very narrow in Iguanodon, the neck flexible and light as in birds.

One of the most remarkable new points discovered in the Bernissart Iguanodons, also a strongly birdlike feature, is the presence in them of a series of completely ossified ligaments stretching along the sides of the dorsal spines of the vertebræ (see figure), and binding the whole dorso-lumber region into a rigid mass as in birds, whilst the region of the neck and hinder region of the tail are free from any such ligaments. No traces of ossified tendons, such as occur in birds, have been found in connection with the limbs of the Iguanodons.

M. Dollo sums up as follows:—"In short the position of the occipital condyle, the length and the mobility of the neck, the rigid attachment of the dorso-lumbar region to the pelvis, the number of the sacral vertebræ, the massive nature of the tail, in fact, the entire structure of the vertebral column, agree in demonstrating that Iguanodon was biped in its gait. "But the most convincing proof of all, perhaps, lies in the evidence afforded by the footprints of Iguanodon in the Wealden strata. Of the eight Dinosauria known from the Wealden, Iguanodon is the only one which could leave tridactyle footprints. M. Dollo obtained a series of casts of the tridactyle Wealden footprints from Mr. Struchman from the neighbourhood of Hanover; choosing one of the right size, he introduced the three toes of the corresponding foot of one of the Bernissart *I. Mantelli*, and also the three metatarsals still united together, giving them a digitigrade position, the only one in which they would enter the impression, and an exact fit of the whole was the result. There can remain no doubt as to the complete correspondence of the two in the mind of any one who has seen the foot and impression thus fitted together. The hand of Iguanodon (see fig.) is pentadactyle, with the thumb transformed into a huge spur which must have been covered with a horny spine when the animal was living. If the animal had walked on all fours, it is impossible but that pentadactyle impressions should have occurred with the tridactyle, but such is not the case. Long series of the tridactyle prints are found without a trace of pentadactyle marks. The arrangement of the tridactyle tracks shows that Iguanodon walked on its hind feet, and did not spring like a kangaroo with the aid of its tail. This merely dragged lightly behind and has left no impression in connection with the foot tracks. The spur-like thumbs were formerly supposed to be the cores of horny appendages of the head. They are much smaller in *I. Mantelli* than in *I. Bernissartensis*, and M. Dollo thinks it will possibly turn out that they are larger in the males than the females.

M. Dollo has not yet published a preliminary account of the skull of Iguanodon, he is now at work on this subject, and a notice of it will shortly appear. In a popular account of the Iguanodons (the last cited in the list) he writes briefly as follows:—

"The head is relatively small, and very much compressed from side to side" (this is a most striking feature when the mounted skeleton is viewed from in front). "The nostrils are spacious and chambered in their anterior region, the orbits are of moderate size, elongated in a vertical direction. The temporal fossa is limited above and below by a bony arch, an arrangement which occurs else only in Hatteria. The distal extremities of both upper and lower jaws are devoid of teeth. They were no doubt during life covered by a horny beak; in the hinder part of the jaws are ninety-two teeth." One of the most remarkable features of the skull is the presence at the symphysis of the lower jaw of a curious separate mass of bone shaped somewhat like a horse's hoof (see figure) which forms the distal extremity of the mandible, fitting in to an excavation on the upper surface of the symphysis. Along its upper rounded margin this bone is dentated. This is believed by M. Dollo to be a bone special to Iguanodon, but not without homologues elsewhere which he will in the future point out, and forming part of the lower jaw. Other observers have considered the bone as the intermaxillary, and have thus concluded that the opening of the mouth lay between the bone and the distal extremity of the lower jaw, and that thus the upper jaw was shaped something like a parrot's beak, shutting into a depression at the symphysis of the lower. A slight inspection of the complete cranium and lower jaw cleared completely of the matrix, which M. Dollo has before him, seems sufficient to carry conviction that his view as to the position of the bone and mouth aperture is the correct one.

The roof of the mouth of Iguanodon in its anterior region is moulded into rounded, ridge-like prominences, which as M. Dollo pointed out have some curious resemblances in form to those occurring in the corresponding position in a duck. The animal was an inhabitant of marshes—as far as yet known apparently of freshwater marshes only—and fed probably largely on ferns, abundance of which were found with the Bernissart specimen. No results of importance as to this question have as yet been obtained from the examination of their coprolites.

The outline of the body shown in the present figure was roughly sketched in by M. Dollo on request, in order to give an idea of his present conjecture as to the probable shape of the living Iguanodon. It is most distinctly to be regarded as merely tentative he reserves any expression of final opinion till the whole material has passed through his hands. On examining the outline, it will be seen that the

Iguanodon probably was shaped, excepting for the long huge tail, which, as Professor Owen long ago pointed out, is shaped like that of a crocodile, being a powerful swimming organ, somewhat like a duck. In accordance with the birdlike modification of the pelvis a large mass of the viscera were post-acetabular in position, as in a greater degree in birds, thus tending to aid the long tail to erect the head and fore part of the body by depressing the hinder region of the spinal column on the acetabular axis as a fulcrum. Like the head the body was very much compressed laterally, so that its transverse section was somewhat as represented in the diagram, X. The neck of the Iguanodon was comparatively slender, and is found to be capable of very free movements. The necks of the fossilised specimens are found to be twisted without dislocation into most varied attitudes. The skin, as already mentioned, was in *I. Mantelli* and *I. Bernissartensis* smooth or covered only with epidermic scales.

Several observers have concluded from the examination of the footprints that a slight web was present between the toes. Judging from observations made on the crocodile and Amblyrhynchus of the Galapagos Islands, the animal when in the water, in which it spent a considerable part of its time, when swimming slowly, used for the purpose both its fore and hind limbs and tail, but when going fast fixed its fore limbs close beside its body and drove itself along with its hind limbs and tail only.

M. Dollo suggests that one of the principal advantages gained by the Iguanodons by their erect posture on land was their being enabled thereby to discern at great distances amongst the vegetation the large carnivorous animals of their age to which as herbivora they must have formed a prey. Possibly when attacked they seized their aggressor in their short arms and made use of their thumb spurs as daggers.

M. Dollo is in every way to be congratulated on the results of his investigations, so far as they have yet gone, and his final monograph may be looked forward to as a work of the utmost value and interest, but with the completion of the Iguanodons the working up of the Bernissart find will be anything but exhausted. With the Dinosaurians were found crocodiles and turtles, and a vast quantity of fishes, of which piles upon piles of specimens await his energies in the future. He has already discovered two most interesting new genera of crocodiles, and an equally interesting new genus of Chelonians amongst this material. Every naturalist who has an opportunity should certainly find his way to Brussels to see the skeleton here figured. It is proposed in process of time, when the Iguanodon skeletons are all prepared from the matrix and mounted as far as necessary, to build a new museum of natural history at Brussels in the Parc Leopold, formerly the zoological garden, and in this museum to construct a special gallery to contain all the Bernissart fossils, a rotunda of twenty-five metres in diameter.

H. N. MOSELEY

THE JAVA UPHEAVAL

THE details which have reached us during the past week of the terrible seismical manifestation at Java prove it to be one of the most disastrous on record; probably, moreover, it is the greatest phenomenon in physical geography which has occurred during at least the historical period, in the same space of time. The accompanying sketch-map will afford some idea of the extent and nature of the change which has taken place, and the character of the sea bed and the land in the region affected. Next week we shall attempt to show what light science can shed on the occurrence; meantime we shall content ourselves with gathering together the facts that have come to hand.

The volcanic Island of Krakatoa lies about the middle of the north part of the passage between Java and Sumatra, a passage which has formed an important commercial route. The strait is about seventy miles long and sixty broad at the south-west end, narrowing to thirteen miles at the nort-east end. The island, seven miles long by five broad, lay about thirty miles from the coast of Java, and northwards the strait contracts like a funnel, the two coasts in that direction approaching very near to each other. A few weeks ago, as we intimated at the time, the volcano on the island began to manifest renewed activity. The whole region is volcanic, Java itself having at least sixteen active volcanoes, while many others can only be regarded as quiescent, not extinct. Various parts of the island have been frequently devastated by volcanic outburts, one of the most disastrous of these having proceeded from a volcano which was regarded as having been long extinct. The present outburst in Krakatoa seems to have reached a crisis on the night of August 26. The detonations were heard as far as Soerakarta, and ashes fell at Cheribon, about 250 miles eastwards on the north coast of Java. The whole sky over western Java was darkened with ashes, and when investigation became possible it was found that the most widespread disaster had occurred. The greater part of the district of North Bantam has been destroyed, partly by the ashes which fell, and partly by an enormous wave generated by the widespread volcanic disturbance in the bed of the strait. The town of Anjer and other towns on the coast have been overwhelmed and swept away, and the loss of life is estimated at 100,000 The Island of Krakatoa itself, estimated to contain eight thousand million cubic yards of material, seems to have been shattered and sunk beneath the waters, while sixteen volcanic craters have appeared above the sea between the site of that island and Sibisi Island, where the sea is comparatively shallow. The Soengepan Volcano has split into five, and it is stated that an extensive plain of "volcanic stone" has been formed in the sea near Lampong, Sumatra, probably at a part of the coast dotted with small islands. A vessel near the site of the eruption had its deck covered with ashes 18 inches deep, and passed masses of pumice stone 7 feet in depth. The wave reached the coast of Java on the morning of the 27th, and, 30 metres high, swept the coast between Merak and Tjiringin, totally destroying Anjer, Merak, and Tjiringin. Five miles of the coast of Sumatra seem to have been swept by the wave, and many lives lost. At Taujong Priok, fifty-eight miles distant from Krakatoa, a sea seven feet and a half higher than the ordinary highest level suddenly rushed in and overwhelmed the place. Immediately afterwards it as suddenly sank ten feet and a half below the high-water mark, the effect being most destructive. We shall probably hear more of this wave, as doubtless it was a branch of it which made its way across the Pacific, and that with such rapidity that on the 27th it reached San Francisco Harbour, and continued to come in at intervals of twenty minutes, rising to a height of one foot for several days. The great wave generated on May 10, 1877, by the earthquake at Iquique, on the coast of Peru, spread over the Pacific as far north as the Sandwich Islands, and south to New Zealand and Australia; while that at Arica, on August 13-14, 1869, extended right across the Pacific to Yokohama (NATURE, vol. i. p. 54). It is misleading to speak of such waves as tidal; they are evidently due to powerful, extensive, and sudden disturbances of the ocean bed, and are frequently felt in the Pacific when no earthquake has been experienced anywhere, though doubtless due to commotions somewhere in the depths of ocean. So far these are all the facts that are known in connection with this last stupendous outburst of volcanic energy. It has altered the entire physical geography of the region and the con-

dition of the ocean bed. The existing charts of the strait with their careful soundings are useless for purposes of navigation, and when quiescence is restored a new series of soundings will be necessary. Doubtless the results of

Heights in feet, Depths in fathoms

the outbreak will receive minute attention at the hands of the Dutch Government, and when all the data are collected they will form valuable material for the study of the physical geographer.

NOTES

THE next meeting of the American Association for the Advancement of Science will be held in Philadelphia, probably during the first week in September, 1884. At the session in Minneapolis the following persons were chosen as officers for the Philadelphia meeting:—President, Dr. J. P. Leslie, of Philadelphia; Vice-Presidents: Section A (Mathematics and Astronomy), Prof. H. T. Eddy, of Cincinnati; B (Physics), Prof. John Trowbridge, of Cambridge; C (Chemistry), Prof. J. W. Langley, of Ann Arbour; D (Mechanical Science), Prof. R. H. Thurston, of Hoboken; E (Geology and Geography), Prof. N. H. Winchell, of Minneapolis; F (Biology), Prof. E. D. Cope, of Philadelphia; G (Histology and Microscopy), Prof. T. G. Wormley, of Philadelphia; H (Anthropology), Prof. E. S. Morse, of Salem; I (Economic Science and Statistics), Hon. John Eaton, of Washington; permanent secretary, Mr. F. W. Putnam, of Cambridge; general secretary, Dr. Alfred Springer, of Cincinnati; assistant general secretary, Prof. E. S. Holden, of Madison.

M. JANSSEN, who has returned from Caroline Island, was present at the meeting of the Academy of Sciences of September 3. He read the first part of the documents he brings with him, viz. the reports drawn up by Palisa, Tacchini, and himself, while Trouvelot read his own account. The reading was long and interesting, and will be continued next week. M. Janssen stated that he believed the region around the sun was full of material almost corpuscular, and reflecting the light from the sun. He was received enthusiastically, and M. Blanchard, the president, spoke in praise of his merits and efforts for the promotion of science. M. Janssen returned thanks, acknowledging that great efforts must be made by him to be worthy of such a reception.

WE regret to announce the death of Mr. Cromwell Fleetwood Varley, F.R.S., M.I.C.E., &c., on Sunday night last, at his residence at Bexley Heath, Kent. He was born in Kentish Town, April 6, 1828. He devoted himself to the engineering branch of telegraphy, and devised a method of locating distant faults in land wires which attracted the special attention of engineers and electricians. Distinguishing himself by one discovery after another, Mr. Varley finally became chief engineer and electrician to the Electric and International Telegraph Company, and held this office until the taking over of the telegraphs by the Government. His inventions were very numerous. Prominent among his early inventions was an apparatus for transmitting electrical signals, which so much increased the

sensitiveness and trustworthiness of the relay that it became practicable for the first time to work from London to Edinburgh direct—a feat impossible in the conditions of insulation previously existing. Mr. Varley was associated with Robert Stephenson, Sir William Fairbairn, and others in devising the first Atlantic cable which may be said to have achieved success. By means of a working model apparatus he demonstrated approximately the speed of electricity when on its travels.

MR. V. T. CHAMBERS, an entomologist well known for his studies on Tineina, died at his residence in Covington, Ky., U.S., on August 7.

DURING the past year, we learn from *Science*, original investigations in the following subjects, among others, have been carried on in the physical laboratory of Johns Hopkins University under the direction of Prof. Rowland and Dr. Hastings: on the photography of the spectrum by means of the concave grating; on the determination of the B. A. unit of electrical resistance in absolute measure; the determination of the specific resistance of mercury; the variation of the specific heat of water with the temperature; the relative wave-lengths of the lines of the spectrum by means of the concave grating; the effect of difference of phase in the harmonics on the timbre of sound; and on the variation of the magnetic permeability of nickel by change of temperature.

MR. THOMAS PLANT, the well-known meteorologist of Birmingham, died suddenly last week. Mr. Plant was sixty-four years of age, and was a native of Lowmoor, Yorkshire. From early manhood he had a passion for the study of the wind and the weather. This passion took a very systematic shape in the compilation of regular records of rainfall, windage, and temperature; and, to the student of meteorology, these records, the result of Mr. Plant's life-long study, will doubtless prove valuable. They are said to be complete for upwards of forty-six years. In 1862 he read a paper before the British Association at Cambridge on "Osler's Anemometer at the Birmingham and Midland Institute," and described the working of the instrument by means of lithographed drawings which he had himself prepared. Three years later he read another paper before the same Association at Birmingham on the "Anomalies of our Climate." A paper on the "Health of the Borough of Birmingham" was read in 1868 by Mr. Plant before the Social Science Congress at Birmingham. He frequently lectured on meteorology, and was a constant contributor to the local press on the same subject.

THE Earl of Crawford and Balcarres has been elected an honorary member of the Berlin Academy of Sciences.

DR. HICKS is reported to have made an interesting discovery in a cave at the back of the Ffynnon Beuno, Flintshire. The cave is a waterworn cave in the limestone rock, similar, though on a smaller scale, to the celebrated Cefn bone caves on the other side of the Vale. Dr. Hicks, after a general inspection of the interior, determined to examine beneath the floor of the cave at the entrance. The removal of a few inches of surface debris disclosed a virgin floor of stalagmite, so well known to cave explorers. Below this were found pieces of bone belonging most evidently to the mammoth or rhinoceros. One piece was embedded in the stalagmite floor. The largest piece—nearly six inches by four—must have formed part of a bone some eighteen inches in circumference. Below was another floor of stalagmite covering a quantity of drift gravel which rested on the bottom of the cave.

MR. FLOYD DELAFIELD of Noroton, Conn., has brought out new dynamo, the novel feature being that the armature is a tube of copper. One of the field magnets is terminated at either end by a tubular pole piece; within this pole piece rotates a tubular armature. On either side of the central magnet runs an auxiliary magnet, which is attached to the axle of the armature. Thus the tubular armature has one pole as its axle, whilst the other pole completely surrounds it. The current is drawn off at either end of the cylinder by brushes. The machine is so arranged that one armature can be used to excite the magnets, whilst the other is used for the main circuit, which gives a good current for plating purposes, or, when required for incandescent lighting, the magnets may be excited by a small high tension dynamo, and then the two armatures may be used for main circuit purposes.

SCIENTIFIC authorities are not at rest with giving Philipp Reiss the merit of inventing the telephone. The latest claimant put forth is Charles Bourseul, a Frenchman, who is said to have invented the telephone in 1854. This invention is said to have been communicated in 1854 to the French Academy, and to have appeared in the *Didaskalia*, a supplementary paper to the *Frankfurter Journal*, for September 28th, 1854. M. le Comte du Moncel is advocating the claims of Bourseul.

M. BERTHELOT has been investigating the speed of gaseous explosions. For this purpose he used an iron tube 16 inches long and ¼ inch bore. The gases were exploded by a spark, and the explosion registered at the centre and end of the tube. The gases he used were carbonic oxide and oxygen, their rate of explosion he observed to be 2500 metres per second. This is a far greater speed than was expected.

IN the experiments which have been made at Grenoble for the transmission of electric force from a distance of 14 kilometres, the wire was of silicated bronze 2 mm. diameter, instead of iron as on former occasions. According to *L'Électricité* the results have been very poor, a motive power of 45 horses having been required to convey 7½ horse-power.

THE observatory at Montmartre, Paris, which belongs to Dr. Gruby, has been reorganised, and M. Cassé has been appointed director. It is a private establishment devoted to meteorology, the results being published in a number of the Paris daily papers. It is built in the vicinity of the Moulin de la Golette, and is now, except the latter establishment, the most elevated point in Paris.

MM. TISSANDIER have completed the construction of their apparatus for preparing hydrogen by a continuous process for filling large balloons. It was tried with a balloon of 300 cubic metres, which conveyed the two brothers to some distance from Paris. This system is a simplification of the apparatus which was used by M. Giffard in his large captive balloon. It will be used for filling the electric balloon now being built by MM. Tissandier.

DR. LIEBSCHER of Jena University sends us some remarks in reply to Mr. B. Kotô's article on "Agriculture in Japan" in NATURE, vol. xxviii. p. 231. With regard to Mr. Kotô's statement that in describing the climate of Japan Dr. Liebscher entirely disregarded the fact that the empire "is surrounded on all sides by a large body of water," he refers to his map of Japan and to p. 8 of his work, where he says: "The summer or south-west monsoon, which on its way from the tropics sweeps over the warm Pacific and is saturated with steam . . ." With regard to what Mr. Kotô says concerning Lake Hakone, Dr. Liebscher maintains that the Hakone Pass is situated not at the foot of the Fuji San, but at a distance of thirty-three miles from its foot, or about fifty miles from the summit, on quite a differen range of mountains. Moreover, Dr. Liebscher points out that Fuji San is not an active volcano "which sends out an enormous quantity of scoriæ" like Vesuvius; nobody, Dr. Liebscher states, has ever seen any trace of scoria or smoke about it since the year 1707. As to Mr. Kotô's statement that "the climate of Japan is not so ineffective as Dr. Liebscher has depicted in his

work; in reality it is far more conducive to fertility than that of Germany," Dr. Liebscher maintains that in his book the very contrary of what Mr. Kotô implies will be found, indicating especially the conclusion of what he says on the natural foundation of agriculture in Japan (p. 58). There it is stated that, "owing to the climate rather than to the rich soil, an amazingly large number of people can live in Japan on the produce of one field." Similar misunderstanding, Dr. Liebscher writes, has been shown by Mr. Kotô in his remarks on the geology and the soil of Japan, in his opinion concerning the Japanese land-tax system, in what he says on the religion of his countrymen, and in denying the existence of polygamy among them.

A SHARP shock of earthquake was felt at 8 o'clock on September 2 at Frascati, on the Alban Hills, twelve miles from Rome. The movement was undulatory and lasted several seconds, but without causing any damage. The instruments in the observatory of the Roman College noted at the same hour a sensible undulatory movement, in the direction of from north-east to south-west. The earthquake was felt simultaneously at Albano, Ariccia, Genzano, Rocca di Papa, Monte Porzio, and other towns on the Alban Hills. At Rocca di Papa a slight shock also occurred a few days ago. New York papers report an earthquake at Pachuca, in Mexico, by which twenty persons lost their lives. A shock was felt at Fjösanger in Bergens Stift, Norway, on August 17, at 10 p.m.

A CORRESPONDENT points out that an account of Prof. Edlund's theory of the connection between thunderstorms and auroræ will be found in *Petermann's Mittheilungen* for 1879, p. 76.

IT is stated that an important oyster bed has been discovered in the Medway. It is estimated to contain over a quarter of a million of young oysters. The Medway was formerly a famous oyster fishery, and it is hoped from this discovery that it is about to become so again.

THE additions to the Zoological Society's Gardens during the past week include a Rhesus Monkey (*Macacus erythræus* ♀) from India, presented by Miss Garwood; a Golden Eagle (*Aquila chrysaetos*) from Scotland, presented by Mr. Bertram B. Hagen; two Long-eared Owls (*Asio otus*), British, presented by Mr. Percy F. Fordham; a Mocking Bird (*Mimus polyglottus*) from North America, presented by Mr. A. Townsend; two Marsh Harriers (*Circus æruginosus*), European, presented by Lieut.-Col. E. Delme Radcliffe; two Barbary Apes (*Macacus inuus*) from North Africa, deposited; a Silvery Gibbon (*Hylobates leuciscus* ♂) from Java, an Indian Muntjac (*Cervulus muntjac*) from India, four Passerine Doves (*Chamæpelia passerina*) from America, a Malabar Parrakeet (*Palæornis columboides*) from Southern India, a Boatbill (*Cancroma cochlearia*), an Anaconda (*Eunectes murinus*) from South America, a Sharp-nosed Crocodile (*Crocodilus cataphractus*) from Central America, purchased; two Ostriches (*Struthio camelus* ♂ ♀) from Africa, received on approval.

OUR ASTRONOMICAL COLUMN

TEMPEL'S COMET, 1873 II.—M. Schulhof of Paris has published elements and an ephemeris of this comet for the approaching return to perihelion. The following is the predicted orbit:—

Epoch, 1883 *October* 20·0 *M.T. at Berlin*

	° ′ ″	
Mean anomaly	354 5 43·5	
Longitude of perihelion	306 7 4·4	M. Eq. 1880·0.
" ascending node ...	121 2 8·5	
Inclination	12 45 17·1	
Angle of eccentricity	33 32 29·5	
Mean daily sidereal motion ...	681″·1068	
Log. semi-axis major	0·477861	

From these elements we find the time of perihelion passage November 20·17155 G.M.T., and the period of revolution 1902·77 days. M. Schulhof's ephemeris so far published extends from August 28 to November 8; during which period the comet is slowly receding from the earth. We extract a few positions:—

At Berlin Midnight

	R.A. h. m. s.	N.P.D ° ′	Log. distance from Earth.	Log. distance from Sun.
Sept. 21	15 19 50	101 51·0	0·2732	0·1739
23	15 25 3	102 24·8		
25	15 30 22	102 58·3	0·2739	0·1687
27	15 35 47	103 31·8		
29	15 41 17	104 4·6	0·2744	0·1637
Oct. 1	15 46 53	104 37·2		
3	15 52 35	105 9·4	0·2750	0·1589
5	15 58 22	105 41·2		

Unless the comet is observed at the present return, observations will hardly be possible before the spring of 1894.

THE GREAT COMET OF 1882.—Dr. B. A. Gould, director of the Observatory at Cordova, informs us that this comet was last seen there with the naked eye on March 7, when Mr. Thome found it already very faint in the telescope, and no nuclear condensation perceptible. His last observation was on June 1, but it was not possible to use the filar micrometer, and he had to depend upon the circles of the equatorial. Had it not been less than an hour high at nightfall, he thinks he could have observed it for a month longer. The Cordova refractor is of ten inches aperture. On March 7 the distance of the comet from the earth was 3·07.

THE MINOR PLANET, NO. 234.—Prof. Krueger communicates in a circular two observations of the small planet last discovered, telegraphed by Mr. O. C. Wendell, from which it appears that the daily motion in N.P.D. is as much as 21′, or, reducing the places for August 12 and 24 to longitude and latitude, we find a change of latitude of 3° 13′ in the interval, the descending node being passed on August 23. This seems to point to a considerable inclination of the orbit. The Harvard positions are as follow:—

G.M.T.	R.A. ° ′ ″	N.P.D. ° ′ ″
1883, August 21·7470	318 57 43	105 24 9
24·7274	318 36 0	106 20 34

Of the large number of these bodies now known, *Pallas*, the second in order of discovery, still retains the greatest orbital inclination, 34° 44′ at present.

GEOGRAPHICAL NOTES

IN the interests of anthropology, Dr. A. B. Meyer, curator of the Dresden Ethnological Museum, has just issued some practical suggestions addressed to the officers of the German Imperial Navy visiting the Indo-Pacific waters. The chief object of this "Denkschrift" is the completion of the Dresden ethnographic collection, whose desiderata are mentioned in detail, and special instructions are given as regards the Chinese seaboard, the South Sea Islands, the north-west coast of America, Madagascar, the Eastern Archipelago, and in general such places as lie on the ordinary route of the German Navy. Here is still to be gathered a rich harvest of materials illustrating the usages, traditions, religions, and social culture, especially of the Polynesian, Papuan, Indo-Chinese, Malay, and North American races. Many objects may thus be brought together calculated to throw light on such important historic and religious movements as the spread of Buddhism from India throughout East Asia, and the influence of Hinduism in past times on the local cultures in Further India and Malaysia. Amongst the miscellaneous wants particular mention is made of fishing gear, boat models, and musical instruments from Formosa; blowpipes, krisses, shields, and brass armour from the Sulu Archipelago and Palawan; nets, harpoons, magic wands from Corea and Yesso; wood carvings and idols from New Guinea and New Britain; clubs, spears, stone hatchets, tattoo designs, figures of men and animals in wood or stone from Melanesia; objects of fetish worship from Micronesia; jade ornaments from Polynesia; carved wooden masks of men and animals, clay or stone vessels, tobacco pipes and nephrite objects from the north-west coast of America; talismans, idols, house utensils, and weapons from Madagascar; wicker-work, burnt clay figures of evil spirits, woven materials

from Ceylon; specimens of figure or picture writings on palm leaves from the Nicobar Islands. Some of these hints may be found useful by English travellers and others willing to promote anthropological work in the Indo-Pacific regions.

Mr. J. T. Last contributes a paper of unusual interest to the September number of the *Proceedings of the Royal Geographical Society*; he describes a visit to the little known Masai country, the region through which Mr. Joseph Thomson had to pass. Mr. Thomson himself sends a long letter giving an account of the first part of his journey and his forced return to the coast. He was to set out again on July 8, *viâ* the north side of Kilimanjaro for Mosera, far on the way to the south shore of Victoria Nyanza. Meantime it is announced that Dr. Fischer, the German explorer who preceded Mr. Thomson on the same route and excited the hostility of the people, has returned to the coast. It seems impossible that he can have reached his proposed goal, and probably, like Mr. Thomson, has been compelled to turn back.

On August 28 the gunboat *Urd* arrived at Tromsö with the members of the Swedish Circumpolar Expedition on board, who have wintered at Spitzbergen. During the *Urd's* voyage to the island she encountered a fog off Beeren Island, which continued to Spitzbergen, but only a small quantity of ice was seen, viz. at South Cape. The vessel arrived at Cape Thordsten on August 10. The observations were continued until 12 midnight on August 23, in order to have a full year's magnetical observations. On the 24th the houses were cleared, the windows nailed up, and the doors locked, and on the 25th the *Urd* steamed out of the Icefjord. In Green Harbour the post was taken on board from the Norwegian hunters, and steering west of the Beeren Island the coast of Norway was sighted on the 28th. No ice was encountered. The ship is expected in Gothenburg on the 6th inst.

We are glad to learn that both the Dutch International Polar Expedition and the Danish Expedition under Lieut. Hovgaard are safe. A Reuter's telegram from Vardoe says:—The steamer *Obi*, belonging to M. Sibiriakoff, has arrived here. The captain picked up on the 25th ult., near Waigatz, the members of the Dutch Polar Expedition steamer *Varna*, which foundered on July 24 in lat. 71, long. 63. The captain further states that the Danish exploring vessel *Dijmphna* had been ice-bound in that region throughout the winter. All was, however, well on board, and the captain of the *Dijmphna* felt confident of getting into open water. The crew of the *Varna*, which left the *Dijmphna* on the 1st ult., will be brought to Hammerfest by the steamer *Nordenskjöld*. The *Varna* had on board the Dutch section of the International Polar Expedition. She left Amsterdam on July 5, 1882, bound for Dickson's Harbour, at the mouth of the Yenisei. The Danish Polar steamer *Dijmphna*, under command of Lieut. Hovgaard, left Copenhagen on July 18, 1882, also bound for the Arctic Seas, and the *Nordenskjöld*, Swedish exploring steamer, left Tromsö about July 3, 1882, bound for Novaya Zemlya. The *Louise* is a trading steamer which left Bremen of June 27 last, and Hammerfest on July 17, bound for the Yenesei.

ELECTRICAL UNITS

THE following is the Report (omitting the appendix) to the Lords of the Committee of Council on Education by the Committee of Advice[1] with respect to the International Congress for the Determination of Electrical Units to be held at Paris in October, 1883.

The first International Electrical Exhibition was held in Paris during the months of August, September, and October, 1881, under the auspices of the French Government, who supplemented it by calling together a Congress of the leading scientific and practical electricians of all countries. England was represented by the following official delegates:—

The Ambassador to France, Sir F. Abel, C.B., F.R.S., Prof. W. G. Adams, F.R.S., Lieut. R. W. Anstruther, R.E., Prof. W. E. Ayrton, F.R.S., Prof. W. F. Barrett, Sir Charles Bright, M.I.C.E., Commissioner at the International Electrical Exhibition, Paris, Prof. Chrystal, F.R.S., Mr. Latimer Clark, M.I.C.E., Prof. R. B. Clifton, F.R.S., the Earl of Crawford and Balcarres, F.R.S., Commissioner-General at the International Electrical Exhibition, Paris, Mr. W. Crookes, F.R.S., Mr. Warren de la Rue, D.C.L., F.R.S., Prof. J. Dewar, F.R.S., Prof. J. D. Everett, F.R.S., Prof. G. Fitzgerald, F.R.S., Prof. G. Carey Foster, F.R.S., Dr. J. H. Gladstone, F.R.S., Mr. J. E. H. Gordon, Mr. E. Graves, Engineer-in-Chief, Postal Telegraphs, Dr. J. Hopkinson, F.R.S., Prof. Hughes, F.R.S., Commissioner at the International Electrical Exhibition, Paris, Prof. Fleeming Jenkin, F.R.S., Mr. J. F. Moulton, F.R.S., Mr. W. H. Preece, F.R.S., Lord Rayleigh, F.R.S., Sir W. Siemens, D.C.L., LL.D., F.R.S., Prof. H. Smith, F.R.S., Mr. Willoughby Smith, Mr. C. E. Spagnoletti, Mr. W. Spottiswoode, D.C.L., LL.D., P.R.S., Mr. A. Stroh, Prof. P. G. Tait, F.R.S.E., Sir William Thomson, LL.D., F.R.S., Prof. J. Tyndall, D.C.L., LL.D., F.R.S., Mr. Cromwell Varley, F.R.S., Mr. C. V. Walker, Lieut.-Col. Webber, R.E., Commissioner at the International Electrical Exhibition, Paris.

Many very important electrical questions were fully discussed, and a universal and international system of units for expressing the results of electrical measurements and observations was determined upon. All parts of the globe being now connected together by a great network of telegraphy, constructed and maintained by every civilised nation, it has become a matter of great commercial as well as scientific importance that uniformity should be introduced in modes of working, measuring, and observing throughout the world. The Paris Congress of 1881 has laid the foundation of such a desirable result.

The Congress of 1881 referred certain questions to a second Conference, held in the month of October, 1882.

This second Conference was divided into three Sections (*i.e.* Commissions); the first dealing with "Electrical Units"; the second with "Earth Currents and Lightning Protectors"; and the third with the question of "A Standard of Light."

Lord Rayleigh, Sir William Thomson, Prof. Carey Foster, Prof. Fleeming Jenkin, and Dr. Hopkinson were nominated as delegates from England, but Sir William Thomson was the only one present, and he devoted his time principally to the first question.

First Commission.—The Electrical Congress of 1881 adopted, as the fundamental system of units for scientific purposes, a system founded upon the employment of the Centimetre, the Gramme, and the Second as units of length, mass, and time respectively, and hence known as the C.G.S. system of units. The Congress also defined, and adopted a nomenclature for, a system of electrical standards of such magnitudes as to be as far as possible generally convenient for practical use, each practical standard being a decimal multiple or submultiple of the corresponding C.G.S. unit.[1] Of these standards, those to which reference is most frequently required are the following, namely:—

The Ohm, defined as one thousand million C.G.S. units of electric resistance.

The Volt, defined as one hundred million C.G.S. units of electromotive force.

The Ampere, defined as one-tenth of a C.G.S. unit of electric current, being the current maintained by an electromotive force of one volt in a conductor of resistance one ohm.

It was further agreed by the Congress that, with a view especially to facility of reproduction, the resistance denoted by the ohm should be stated as being the resistance of a column of mercury at the temperature of melting ice, of one square millimetre in cross-section, and of a length to be ascertained by experiment.

Accordingly the principal question referred to the first section of the Conference of 1882 was the determination of the length of a column of mercury, of the above-mentioned cross-section and temperature, which had an electrical resistance of one thousand million C.G.S. units. In reference to this question the Conference adopted the following resolutions, namely:—

First Resolution.—"The Conference considers that the determinations hitherto made do not present the necessary degree of concordance for fixing the numerical value of the ohm in terms of a column of mercury.

[1] The President of the Royal Society, the late Mr. W. Spottiswoode, was a member of the Committee, but his illness and death prevented his taking part in its proceedings.

[1] It is satisfactory to your Committee to be able to say that the C.G.S. system of units was widely used among English physicists before its adoption by the Electrical Congress in 1881, it having been recommended by a Committee of the British Association in 1875; and also that the system of practical standards adopted by the Congress is nearly identical with that previously in use in England and first suggested in a paper by Mr Latimer Clark and Sir Charles Bright read before the British Association in Manchester in 1861.

"It is therefore of opinion that it is necessary to continue investigations in relation to this question."

Second Resolution.—"The Conference expresses the wish that the French Government should take the necessary measures for placing one or several standard resistances at the disposal of those men of science who are devoting themselves to the investigation of absolute units, in order to facilitate the comparison of results."

Third Resolution.—"The Conference is of opinion that so soon as the results of the various investigations shall be so far accordant that it is possible to guarantee an accuracy of one part in a thousand, it will be proper to accept this degree of approximation for the purpose of fixing the value of the practical standard of resistance."

Fourth Resolution.—"The Conference expresses the wish that the French Government may be pleased to communicate to the Governments represented at the Conference a desire to the effect that each of them, in view of the importance and urgency of arriving at a practical solution, should take the necessary steps to encourage investigations, on the part of its own nation, in relation to the determination of the electrical units."

Upon these resolutions your Committee have to observe that experiments made in the Cavendish laboratory of the University of Cambridge by Lord Rayleigh and other experimenters working in conjunction with him, confirmed by independent experiments by different methods also made in the Cavendish laboratory, appear to have attained a greater degree of accuracy than that agreed upon by the Conference as sufficient for present requirements.

Your Committee are therefore of opinion that, so far as the determination of the standard of electrical resistance is concerned, it is unnecessary to advise the Government to take any steps in the matter until fur her researches raise fre h questions relating thereto, as the results obtained at Cambridge seem to possess all the accuracy obtainable at present.

In the Second Commission, which dealt with earth currents and lightning protectors, various resolutions were carried, which it will be convenient to deal with separately.

The first resolution proposed that the different Governments should organise regular observations upon the behaviour of atmospheric electricity. In reference to this your Committee understand that regular and continuous observations have been made for some years at Greenwich and at Kew, and without further and more detailed recommendations on the part of the Congress as to the special observations they propose, your Committee are not in a position to recommend any further steps to be taken by the Government.

The second resolution expressed the wish that a detailed study should be made of the effects of thunderstorms upon telegraphic lines and telephonic lines, and upon buildings connected with wires.

In regard to this your Committee have nothing to advise until the Commission have formulated their requirements in more detail; when this has been done, it is understood from the delegates of the Post Office that the fullest consideration will be given to the matter, with a view to afford every assistance in the power of that department.

The third resolution dealt with the question of the observation of earth currents. Your Committee would observe that continuous records are made by photography at Greenwich of all earth currents occurring upon two telegraphic lines proceeding from the Observatory nearly at right angles to each other; and careful returns are collected from all the principal Post Offices in the United Kingdom of every unusual and disturbing magnetic storm; and they recommend that a description of the methods employed in this country, which, with notes bearing on the subject, is appended, be submitted to the Congress, with the view to their universal adoption, if approved, in order that similar observations may be carried out throughout the world. Your Committee at the same time are of opinion that, with a view to meet the wishes of the Congress, some effort might be made to secure observations on Sundays on those telegraphic lines where the staff is necessarily present, but where the number of messages sent is very small.

The fourth resolution suggests the establishment of an international network of telegraph wires for the purpose of automatically registering at a central station meteorological changes.

In view of the great expense that would be incurred in establishing a system of wires for automatically recording telemeteorographical observations, your Committee concur with the Congress in considering the time has not arrived for adopting that proposal.

The fifth resolution expresses the view that lightning protectors and conductors should everywhere be submitted to a periodical inspection.

This recommendation is at present carried out by the War Office in connection with the buildings under its charge, and the subject has been considered by a Committee of Delegates from the Society of Telegraph Engineers, the Physical and Meteorological Societies, and the Institute of British Architects. There is not in England any authority legally competent to discharge the duty as far as the general public is concerned, and it is therefore impossible in England to carry out this proposal in its entirety; but the Committee concur in the advisability of adopting that course where it is found possible to do so.

The sixth resolution implies that the returns of storms and their effects upon buildings and telegraphic lines should be subject to statistical examination.

Your Committee consider that the observations necessary for ascertaining the effects of lightning other than on telegraph wires cannot be carried out by the Government, owing to the non-existence of a competent staff throughout the Kingdom and that such observations must of necessity be left to private observers. Your Committee recommend, however, that the Meteorological Office be supplied with forms of questions such as may be finally adopted by the Congress for distribution to meteorological observers throughout the United Kingdom. The information desired by the Congress would, it is hoped, thus be obtained.

With reference to the effect of lightning on telegraph lines, the delegates of the Post Office who attended the meeting of the Committee stated that their department would be able to adopt any form of questions, on which returns could be made, finally proposed by the Congress.

Your Committee recommend that the Government should procure such adoption by the Post Office in the United Kingdom, and should also use its influence to cause the same form to be adopted by the Indian and Colonial Administrations and by the various submarine and other telegraphic and telephonic companies at home and abroad.

THIRD COMMISSION.—This Commission dealt with the establishment of a standard of light by reference to which various electric and other lights could be measured. At the present moment there are two in existence: the one is the French Carcel Lamp, and the other is the English Standard Candle, the former being nearly ten times the latter. No better standard was proproposed at the Conference. This question remains in abeyance for further investigation.

Your Committee fully recognise the importance of the recommendation to adopt a uniform standard of light.

A Committee appointed by the British Association are now considering the question, and pending their Report your Committee have at present no recommendations to make.

It will be seen from this Report that there are matters of high scientific and practical importance which will be brought before the approaching Congress, and your Committee are of opinion that England should be represented, to bring the views above expressed before it, and to assist at its deliberations. The value of the decisions at which the Congress may arrive depends mainly on its international character, and the non-representation of this country would be a serious blow to the authority of its utterance, and perhaps cause the same confusion in electrical science which now exists in others where international accord has not been established.

(Signed) W. G. ADAMS.
R. Y. ARMSTRONG, Maj. R.E.
W. H. M. CHRISTIE.
G. C. FOSTER.
J. F. MOULTON.
RAYLEIGH.
C. W. SIEMENS.
G. G. STOKES.
W. THOMSON.
E. GRAVES, W. H. PREECE } representing the Post Office.
J. F. D. DONNELLY, Col. R.E., W. de W. ABNEY, Capt. R.E. } representing the Science and Art Department.

August 8, 1883

SOME UNSOLVED PROBLEMS IN GEOLOGY[1]

MY predecessor in office remarked, in the opening of his address, that two courses are open to the retiring president of this Association in preparing the annual presidential discourse,—he may either take up some topic relating to his own specialty, or he may deal with various or general matters relating to science and its progress. A geologist, however, is not necessarily tied up to one or the other alternative. His subject covers the whole history of the earth in time. At the beginning it allies itself with astronomy and physics and celestial chemistry. At the end it runs into human history, and is mixed up with archæology and anthropology. Throughout its whole course it has to deal with questions of meteorology, geography, and biology. In short, there is no department of physical or biological science with which geology is not allied, or at least on which the geologist may not presume to trespass. When, therefore, I announce as my subject on the present occasion some of the unsolved problems of this universal science, you need not be surprised if I should be somewhat discursive.

Perhaps I shall begin at the utmost limits of my subject by remarking that in matters of natural and physical science we are met at the outset with the scarcely solved question as to our own place in the nature which we study, and the bearing of this on the difficulties we encounter. The organism of man is decidedly a part of nature. We place ourselves, in this aspect, in the sub-kingdom vertebrata, and class mammalia, and recognise the fact that man is the terminal link in a chain of being extending throughout geological time. But the organism is not all of man; and, when we regard man as a scientific animal, we raise a new question. If the human mind is a part of nature, then it is subject to natural law; and nature includes mind as well as matter. On the other hand, without being absolute idealists, we may hold that mind is more potent than matter, and nearer to the real essence of things. Our science is in any case necessarily dualistic, being the product of the reaction of mind on nature, and must be largely subjective and anthropomorphic. Hence, no doubt, arise much of the controversy of science, and much of the unsolved difficulty. We recognise this when we divide science into that which is experimental, or depends on apparatus, and that which is observational and classificatory,—distinctions, these, which relate not so much to the objects of science as to our methods of pursuing them. This view also opens up to us the thought that the domain of science is practically boundless; for who can set limits to the action of mind on the universe, or of the universe on mind? It follows that science must be limited on all sides by unsolved mysteries; and it will not serve any good purpose to meet these with clever guesses. If we so treat the enigmas of the sphinx Nature, we shall surely be devoured. Nor, on the other hand, must we collapse into absolute despair, and resign ourselves to the confession of inevitable ignorance. It becomes us, rather, boldly to confront the unsolved questions of nature, and to wrestle with their difficulties till we master such as we can, and cheerfully leave those we cannot overcome to be grappled with by our successors.

Fortunately, as a geologist, I do not need to invite your attention to those transcendental questions which relate to the ultimate constitution of matter, the nature of the ethereal medium filling space, the absolute difference or identity of chemical elements, the cause of gravitation, the conservation and dissipation of energy, the nature of life, or the primary origin of bioplasmic matter. I may take the much more humble *rôle* of an inquirer into the unsolved or partially solved problems which meet us in considering that short and imperfect record which geology studies in the rocky layers of the earth's crust, and which leads no farther back than to the time when a solid rind had already formed on the earth and was already covered with an ocean. This record of geology covers but a small part of the history of the earth and of the system to which it belongs, nor does it enter at all into the more recondite problems involved; till it forms, I believe, some necessary preparation, at least, to the comprehension of these.

What do we know of the oldest and most primitive rocks? At this moment the question may be answered in many and discordant ways; yet the leading elements of the answer may be given very simply. The oldest rock formation known to geologists is the lower Laurentian, the fundamental gneiss, the Lewisian formation of Scotland, the Ottawa gneiss of Canada. This formation of enormous thickness corresponds to what the older geologists called the fundamental granite,—a name not to be scouted, for gneiss is only a stratified granite. Perhaps the main fact in relation to this old rock is that it is a gneiss; that is, a rock at once bedded and crystalline, and having for its dominant ingredient the mineral orthoclase,—a compound of silica, alumina, and potash,—in which are embedded, as in a paste, grains and crystals of quartz and hornblende. We know very well, from its texture and composition, that it cannot be a product of mere heat; and, being a bedded rock, we infer that it was laid down layer by layer, in the manner of aqueous deposits. On the other hand, its chemical composition is quite different from that of the muds, sands, and gravels usually deposited from water. Their special characters are caused by the fact that they have resulted from the slow decay of rocks like these gneisses, under the operation of carbonic acid and water, whereby the alkaline matter and the more soluble part of the silica have been washed away, leaving a residue mainly siliceous and aluminous. Such more modern rocks tell of dry land subjected to atmospheric decay and rainwash. If they have any direct relation to the old gneisses, they are their grandchildren, not their parents. On the contrary, the oldest gneisses show no pebbles, or sand, or limestone—nothing to indicate that there was then any land undergoing atmospheric waste, or shores with sand and gravel. For all that we know to the contrary, these old gneisses may have been deposited in a shoreless sea, holding in solution or suspension merely what it could derive from a submerged crust recently cooled from a state of fusion, still thin, and exuding here and there through its fissures heated waters and volcanic products.

It is scarcely necessary to say that I have no confidence in the supposition of unlike composition of the earth's mass on different sides, on which Dana has partly based his theory of the origin of continents. The most probable conception seems to be that of Lyell; namely, a molten mass, uniform except in so far as denser material might exist towards its centre, and a crust, at first approximately even and homogeneous, and subsequently thrown into great bendings upward and downward. This question has recently been ably discussed by Mr. Crosby in the London *Geological Magazine*.[1]

In short, the fundamental gneiss of the lower Laurentian may have been the first rock ever formed; and in any case it is a rock formed under conditions which have not since recurred, except locally. It constitutes the first and best example of these chemico-physical, aqueous, or aqueo-igneous rocks, so characteristic of the earliest period of the earth's history. Viewed in this way, the lower Laurentian gneiss is probably the oldest kind of rock we shall ever know,—the limit to our backward progress, beyond which there remains nothing to the geologist except physical hypotheses respecting a cooling, incandescent globe. For the chemical conditions of these primitive rocks, and what is known as to their probable origin, I must refer you to my friend Dr. Sterry Hunt, to whom we owe so much of what is known of the older crystalline rocks,[2] as well as of their literature and the questions which they raise. My purpose here is to sketch the remarkable difference which we meet as we ascend into the middle and upper Laurentian.

In the next succeeding formation, the true lower Laurentian of Logan, the Grenville series of Canada, we meet with a great and significant change. It is true, we have still a predominance of gneisses which may have been formed in the same manner with those below them; but we find these now associated with great beds of limestone and dolomite, which must have been formed by the separation of calcium and magnesium carbonates from the sea water, either by chemical precipitation or by the agency of living beings. We have also quartzite, quartzose gneisses, and even pebble beds, which inform us of sand-banks and shores. Nay, more, we have beds containing graphite, which must be the residue of plants, and iron ores which tell of the deoxidation of iron oxide by organic matters. In short, here we have evidence of new factors in world-building,—of land and ocean, of atmospheric decay of rocks, of deoxidising processes carried on by vegetable life on the land and in the waters, of limestone-building in the sea. To afford material for such rocks, the old Ottawa gneiss must have been lifted up into continents and mountain masses. Under the slow but sure action of the carbonic dioxide dissolved in rain water, its felspar had crumbled

[1] Address of the retiring president of the American Association for the Advancement of Science, Principal J. W. Dawson, LL.D., F.R.S., at Minneapolis, August 15, 1883. Advance proofs of this and other addresses to follow have been kindly sent us by the Editor of *Science*.

[1] June, 1883.

[2] Hunt, "Essays on Chemical Geology."

down in the course of ages. Its potash, soda, lime, magnesia, and part of its silica, had been washed into the sea, there to enter into new combinations, and to form new deposits. The crumbling residue of fine clay and sand had been also washed down into the borders of the ocean, and had been there deposited in beds.[1] Thus the earth had entered into a new phase, which continues onward through the geological ages; and I place in your hands one key for unlocking the mystery of the world when I affirm that this great change took place, this new era was inaugurated, in the midst of the Laurentian period.

Was not this time a fit period for the first appearance of life? Should we not expect it to appear, independently of the evidence we have of the fact? I do not propose to enter here into that evidence, more especially in the case of the one well characterised Laurentian fossil, *Eozoon canadense*. I have already amply illustrated it elsewhere. I would merely say here, that we should bear in mind that, in this later half of the lower Laurentian or, if we so choose to style it, middle Laurentian period, we have the conditions required for life in the sea and on the land; and since in other periods we know that life was always present when its conditions were present, it is not unreasonable to look for the first traces of life in this formation, in which we find for the first time the completion of those physical arrangements which make life, in such forms of it as exist on our planet, possible.

This is also a proper place to say something of the doctrine of what is termed "metamorphism." The Laurentian rocks are undoubtedly greatly changed from their original state, more especially in the matters of crystallisation and the formation of disseminated minerals by the action of heat and heated water. Sandstones have thus passed into quartzites, clays into slates and schists, limestones into marbles. So far, metamorphism is not a doubtful question; but, when theories of metamorphism go so far as to suppose an actual change of one element for another, they go beyond the bounds of chemical credibility; yet such theories of metamorphism are often boldly advanced, and made the basis of important conclusions. Dr. Hunt has happily given the name "metasomatosis" to this imaginary and impossible kind of metamorphism, which may be regarded as an extreme kind of evolution, akin to some of those forms of that theory employed with reference to life, but more easily detected and exposed. I would have it to be understood that, in speaking of the metamorphism of the older crystalline rocks, it is not to this metasomatosis that I refer, and that I hold that rocks which have been produced out of the materials decomposed by atmospheric erosion can never, by any process of metamorphism, be restored to the precise condition of the Laurentian rocks. Thus there is in the older formations a genealogy of rocks, which, in the absence of fossils, may be used with some confidence, but which does not apply to the more modern deposits. Still, nothing in geology absolutely perishes or is altogether discontinued; and it is probable, that, down to the present day, the causes which produced the old Laurentian gneiss may still operate in limited localities. Then, however, they were general, not exceptional. It is further to be observed that the term "gneiss" is sometimes of wide and even loose application. Beside the typical orthoclase and hornblendic gneiss of the Laurentian, there are micaceous, quartzose, garnetiferous, and many other kinds of gneiss; and even gneissose rocks, which hold labradorite or anorthite instead of orthoclase, are sometimes, though not accurately, included in the term.

The Grenville series, or middle Laurentian, is succeeded by what Logan in Canada called the upper Laurentian, and which other geologists have called the Norite or Norian series. Here we still have our old friends the gneisses, but somewhat peculiar in type; and associated with them are great beds rich in lime-felspar,—the so-called labradorite and anorthite rocks. The precise origin of these is uncertain, but this much seems clear; namely, that they originated in circumstances in which the great limestones deposited in the lower or middle Laurentian were beginning to be employed in the manufacture, probably by aqueo-igneous agencies, of lime-felspars. This proves the Norian rocks to be much younger than the Laurentian, and that, as Logan supposed, considerable earth-movements had occurred between the two, implying lapse of time.

Next we have the Huronian of Logan,—a series much less crystalline and more fragmentary, and affording more evidence of land elevation and atmospheric and aqueous erosion than any of the others. It has great conglomerates, some of them made up of rounded pebbles of Laurentian rocks, and others of quartz pebbles, which must have been the remains of rocks subjected to very perfect erosion. The pure quartz rocks tell the same tale, while limestones and slates speak also of chemical separation of the materials of older rocks. The Huronian evidently tells of movements in the previous Laurentian, and changes in its texture so great, that the former may be regarded as a comparatively modern rock, though vastly older than any part of the palæozoic series.

Still later than the Huronian is the great micaceous series called by Hunt the Mont Alban or White Mountain group, and the Taconian or lower Taconic of Emmons, which recalls in some measure the conditions of the Huronian. The precise relations of these to the later formations, and to certain doubtful deposits around Lake Superior, can scarcely be said to be settled, though it would seem that they are all older than the fossiliferous Cambrian rocks which practically constitute the base of the palæozoic. I have, I may say, satisfied myself, in regions which I have studied, of the existence and order of these rocks as successive formations, though I would not dogmatise as to the precise relations of those last mentioned, or as to the precise age of some disputed formations which may either be of the age of the older eozoic formations, or may be peculiar kinds of palæozoic rocks modified by metamorphism. Probably neither of the extreme views now agitated is absolutely correct.

After what has been said, you will perhaps not be astonished that a great geological battle rages over the old crystalline rocks. By some geologists they are almost entirely explained away, or referred to igneous action or to the alteration of ordinary sediments. Under the treatment of another school, they grow to great series of pre-Cambrian rocks, constituting vast systems of formations, distinguishable from each other, not by fossils, but by differences of mineral character. I have already indicated the manner in which I believe the dispute will ultimately be settled, and the president of the geological section will treat it more fully in his opening address.

After the solitary appearance of Eozoon in the Laurentian, and of a few uncertain forms in the Huronian and Taconian, we find ourselves in the Cambrian, in the presence of a nearly complete invertebrate fauna of protozoa, polyps, echinoderms, mollusks, and crustacea; and this not confined to one locality merely, but apparently extended simultaneously through out the ocean. This sudden incoming of animal life, along with the subsequent introduction of successive groups of invertebrates, and finally of vertebrate animals, furnishes one of the greatest of the unsolved problems of geology, which geologists were wont to settle by the supposition of successive creations. In an address delivered at the Detroit meeting of the Association in 1875, I endeavoured to set forth the facts as to this succession, and the general principles involved in it, and to show the insufficiency of the theories of evolution suggested by biologists to give any substantial aid to the geologist in these questions. In looking again at the points there set forth, I find they have not been invalidated by subsequent discoveries, and that we are still nearly in the same position with respect to these great questions that we were in at that time,—a singular proof of the impotency of that deductive method of reasoning which has become fashionable among naturalists of late. Yet the discussions of recent years have thrown some additional light on these matters; and none more so than the mild disclaimers with which my friend Dr. Asa Gray and other moderate and scientific evolutionists have met the extreme views of such men as Romanes, Haeckel, Lubbock, and Grant Allen. It may be useful to note some of these as shedding a little light on this dark corner of our unsolved problems.

It has been urged, on the side of rational evolution, that this hypothesis does not profess to give an explanation of the absolute origin of life on our planet, or even of the original organisation of a single cell or of a simple mass of protoplasm, living or dead. All experimental attempts to produce by synthesis the complex albuminous substances, or to obtain the living from the non living, have so far been fruitless; and, indeed, we cannot imagine any process by which such changes could be effected. That they have been effected we know; but the process employed by their maker is still as mysterious to us as it probably was to him who wrote the words, "And God said, Let the waters swarm with swarmers." How vast is the gap in our knowledge and our practical power implied in this admission, which must, however, be made by every mind not absolutely

[1] Dr. Hunt has now in preparation for the press an important paper on this subject, read before the National Academy of Sciences.

blinded by a superstitious belief in those forms of words which too often pass current as philosophy.

But if we are content to start with a number of organisms ready made,—a somewhat humiliating start, however,—we still have to ask, How do these vary so as to give new species? It is a singular illusion in this matter, of men who profess to be believers in natural law, that variation may be boundless, aimless, and fortuitous, and that it is by spontaneous selection from varieties thus produced that development arises. But surely the supposition of mere chance and magic is unworthy of science. Varieties must have causes, and their causes and their effects must be regulated by some law or laws. Now, it is easy to see that they cannot be caused by a mere innate tendency in the organism itself. Every organism is so nicely equilibrated, that it has no such spontaneous tendency, except within the limits set by its growth and the law of its periodical changes. There may however, be equilibrium more or less stable. I believe all attempts hitherto made have failed to account for the fixity of certain, nay, of very many, types throughout geological time; but the mere consideration that one may be in a more stable state of equilibrium than another so far explains it. A rocking stone has no more spontaneous tendency to move than an ordinary boulder, but it may be made to move with a touch. So it probably is with organisms. But, if so, then the causes of variation are external, as in many cases we actually know them to be; and they must depend on instability or change in surroundings, and this so arranged as not to be too extreme in amount, and to operate in some determinate direction. Observe how remarkable the unity of the adjustments involved in such a supposition. How superior they must be to our rude and always more or less unsuccessful attempts to produce and carry forward varieties and races in definite directions! This cannot be chance. If it exists, it must depend on plans deeply laid in the nature of things, else it would be most monstrous magic and causeless miracle. Still more certain is this conclusion when we consider the vast and orderly succession made known to us by geology, and which must have been regulated by fixed laws, only a few of which are as yet known to us.

Beyond these general considerations, we have others of a more special character, based on palæontological facts, which show how imperfect are our attempts, as yet, to reach the true causes of the introduction of genera and species.

One is the remarkable fixity of the leading types of living beings in geological time. If instead of framing, like Haeckel, fanciful phylogenies, we take the trouble, with Barrande and Gaudry, to trace the forms of life through the period of their existence, each along its own line, we shall be greatly struck with this, and especially with the continuous existence of many low types of life through vicissitudes of physical conditions of the most stupendous character, and over a lapse of time scarcely conceivable. What is still more remarkable is, that this holds in groups which, within certain limits, are perhaps the most variable of all. In the present world no creatures are individually more variable than the protozoa; as, for example, the foraminifera and the sponges. Yet these groups are fundamentally the same from the beginning of the palæozoic until now; and modern species seem scarcely at all to differ from specimens procured from rocks at least half way back to the beginning of our geological record. If we suppose that the present sponges and foraminifera are the descendants of those of the Silurian period, we can affirm, that, in all that vast lapse of time, they have, on the whole, made little greater change than that which may be observed in variable forms at present. The same remark applies to other low animal forms. In forms somewhat higher and less variable, this is equally noteworthy. The pattern of the venation of the wings of cockroaches, and the structure and form of land-snails, gally-worms, and decapod crustaceans, were all settled in the Carboniferous age in a way that still remains. So were the foliage and the fructification of club-mosses and ferns. If at any time members of these groups branched off, so as to lay the foundation of new species, this must have been a very rare and exceptional occurrence, and one demanding even some suspension of the ordinary laws of nature.

Certain recent utterances of eminent scientific men in England and France are most instructive with reference to the difficulties which encompass this subject. Huxley, at present the leader of English evolutionists, in his "Rede Lecture"[1] delivered at Cambridge, England, holds that there are only two "possible alternative hypotheses" as to the origin of species,—(1) that of "construction," or the mechanical putting-together of the materials and parts of each new species separately; and (2) that of "evolution," or that one form of life "proceeded from another" by the "establishment of small successive differences." After comparing these modes, much to the disadvantage of the first, he concludes with the statement that "this was his case for evolution, which he rested wholly on arguments of the kind he had adduced;" these arguments being the threadbare false analogy of ordinary reproduction and the transformation of species, and the mere succession of forms more or less similar in geological time, neither of them having any bearing whatever on the origin of any species or on the cause of the observed succession. With reference to the two alternatives, while it is true that no certain evidence has yet been obtained—either by experiment, observation, or sound induction—as to the mode of origin of any species, enough is known to show that there are numerous possible methods, grouped usually under the heads of absolute creation, mediate creation, critical evolution, and gradual evolution. It is also true that almost the only thing we certainly know in the matter is that the differences characteristic of classes, orders, genera, and species, must have arisen, not in one or two, but in many ways. An instructive commentary on the capacity of our age to deal with these great questions is afforded by the fact that this little piece of clever mental gymnastics should have been practised in a university lecture and in presence of an educated audience. It is also deserving of notice, that, though the lecturer takes the development of the Nautili and their allies as his principal illustration, he evidently attaches no weight to the argument in the opposite sense deduced by Barrande—the man of all others most profoundly acquainted with these animals—from the palæozoic cephalopods.

Another example is afforded by a lecture recently delivered at the Royal Institution in London by Professor Flower.[1] The subject is "The Whales, Past and Present, and their Probable Origin." The latter point, as is well known, Gaudry had candidly given up. "We have questioned," he says, "these strange and gigantic sovereigns of the tertiary oceans as to their ancestors,—they leave us without reply." Flower is bold enough to face this problem; and he does so in a fair and vigorous way, though limiting himself to the supposition of slow and gradual change. He gives up at once, as every anatomist must, the idea of an origin from fishes or reptiles. He thinks the ancestors of the whales must have been quadrupedal mammals. He is obliged for good reasons to reject the seals and the otters, and turns to the ungulates, though here, also, the difficulties are formidable. Finally he has recourse to an imaginary ancestor, supposed to have haunted marshes and rivers of the mesozoic age, and to have been intermediate between a hippopotamus and a dolphin, and omnivorous in diet. As this animal is altogether unknown to geology or zoology, and not much less difficult to account for than the whales themselves, he very properly adds, "Please to recollect, however, that this is a mere speculation." He trusts, however, that such speculations are "not without their use"; but this will depend upon whether or not they lead men's minds from the path of legitimate science into the quicksands of baseless conjecture.

Gaudry, in his recent work, "Enchaînements du Monde Animal,"[2] though a strong advocate of evolution, is obliged in his final *résumé* to say, "Il ne laisse point percer le mystère qui entoure le developpement primitif des grandes classes du monde animal. Nul homme ne sait comment ont été formés les premiers individus de foraminifères, de polypes, d'étoiles de mer, de crinoides, &c. Les fossiles primaires ne nous ont pas encore fourni de preuves positives du passage des animaux d'une classe à ceux d'une autre classe."

Professor Williamson of Manchester, in an address delivered in February last before the Royal Institution of Great Britain, after showing that the conifers, ferns, and lycopods of the palæozoic have no known ancestry, uses the significant words, "The time has not yet arrived for the appointment of a botanical king-at-arms and constructor of pedigrees."

Another caution which a palæontologist has occasion to give with regard to theories of life has reference to the tendency of biologists to infer that animals and plants were introduced under embryonic forms, and at first in few and imperfect species. Facts do not substantiate this. The first appearance of leading types of life is rarely embryonic. On the contrary, they often appear in highly perfect and specialised forms; often, however, of composite type, and expressing characters afterwards so

[1] Report in NATURE, June 21 (p. 187), corrected by the author.

[1] Reported in NATURE. [2] Paris, 1883.

separated as to belong to higher groups. The trilobites of the Cambrian are some of them of few segments, and, so far, embryonic; but the greater part are many-segmented, and very complex. The batrachians of the carboniferous present many characters higher than those of their modern successors, and now appropriated to the true reptiles. The reptiles of the Permian and trias usurped some of the prerogatives of the mammals. The ferns, lycopods, and equisetums of the Devonian and carboniferous were, to say the least, not inferior to their modern representatives. The shell-bearing cephalopods of the palæozoic would seem to have possessed structures now special to a higher group, that of the cuttle-fishes. The bald and contemptuous negation of these facts by Haeckel and other biologists does not tend to give geologists much confidence in their dicta.

Again: we are now prepared to say that the struggle for existence, however plausible as a theory, when put before us in connection with the productiveness of animals, and the few survivors of their multitudinous progeny, has not been the determining cause of the introduction of new species. The periods of rapid introduction of new forms of marine life were not periods of struggle but of expansion,—those periods in which the submergence of continents afforded new and large space for their extension and comfortable subsistence. In like manner it was continental emergence that afforded the opportunity for the introduction of land animals and plants. Further, in connection with this, it is now an established conclusion that the great aggressive faunas and floras of the continents have originated in the north, some of them within the Arctic circle; and this in periods of exceptional warmth, when the perpetual summer sunshine of the Arctic regions coexisted with a warm temperature. The testimony of the rocks thus is, that not struggle, but expansion, furnished the requisite conditions for new forms of life, and that the periods of struggle were characterised by depauperation and extinction.

But we are sometimes told that organisms are merely mechanical, and that the discussions respecting their origin have no significance, any more than if they related to rocks or crystals, because they relate merely to the organism considered as a machine, and not to that which may be supposed to be more important, namely, the great determining power of mind and will. That this is a mere evasion, by which we really gain nothing, will appear from a characteristic extract from an article by an eminent biologist, in the new edition of the "Encyclopædia Britannica,"—a publication which, I am sorry to say, instead of its proper *rôle* as a repertory of facts, has become a strong partisan, stating extreme and unproved speculations as if they were conclusions of science. The statement referred to is as follows: "A mass of living protoplasm is simply a molecular machine of great complexity, the total results of the working of which, or its vital phenomena, depend on the one hand on its construction, and, on the other, on the energy supplied to it; and to speak of vitality as anything but the name for a series of operations is as if one should talk of the horologity of a clock." It would, I think, scarcely be possible to put into the same number of words a greater amount of unscientific assumption and unproved statement than in this sentence. Is "living protoplasm" different in any way from dead protoplasm, and, if so, what causes the difference? What is a "machine"? Can we conceive of a self-produced or uncaused machine, or one not intended to work out some definite results? The results of the machine in question are said to be "vital phenomena;" certainly most wonderful results, and greater than those of any machine man has yet been able to construct. But why "vital"? If there is no such thing as life, surely they are merely physical results. Can mechanical causes produce other than physical effects? To Aristotle, life was "the cause of form in organisms." Is not this quite as likely to be true as the converse proposition? If the vital phenomena depend on the "construction" of the machine, and the "energy supplied to it," whence this construction, and whence this energy? The illustration of the clock does not help us to answer this question. The construction of the clock depends on its maker, and its energy is derived from the hand that winds it up. If we can think of a clock which no one has made and which no one winds, a clock constructed by chance, set in harmony with the universe by chance, wound up periodically by chance,—we shall then have an idea parallel to that of an organism living, yet without any vital energy or creative law; but in such a case we should certainly have to assume some antecedent cause, whether we call it "horologity" or by some other name. Perhaps the term "evolution" would serve as well as any other, were it not that common sense teaches that nothing can be spontaneously evolved out of that in which it did not previously exist.

There is one other unsolved problem, in the study of life by the geologist, to which it is still necessary to advert. This is the inability of palæontology to fill up the gaps in the chain of being. In this respect, we are constantly taunted with the imperfection of the record; but facts show that this is much more complete than is generally supposed. Over long periods of time and many lines of being we have a nearly continuous chain; and, if this does not show the tendency desired, the fault is as likely to be in the theory as in the record. On the other hand, the abrupt and simultaneous appearance of new types in many specific and generic forms, and over wide and separate areas at one and the same time, is too often repeated to be accidental. Hence palæontologists, in endeavouring to establish evolution, have been obliged to assume periods of exceptional activity in the introduction of species, alternating with others of stagnation,—a doctrine differing very little from that of special creation as held by the older geologists.

The attempt has lately been made to account for these breaks by the assumption that the geological record relates only to periods of submergence, and gives no information as to those of elevation. This is manifestly untrue. In so far as marine life is concerned, the periods of submergence are those in which new forms abound for very obvious reasons already hinted. But the periods of new forms of land and fresh-water life are those of elevation, and these have their own records and monuments, often very rich and ample; as, for example, the swamps of the carboniferous, the transition from the cretaceous subsidence to the Laramie elevation, the tertiary lake-basins of the west, the terraces and raised beaches of the pleistocene. Had I time to refer in detail to the breaks in the continuity of life which cannot be explained by the imperfection of the record, I could show at least that nature, in this case, does advance *per saltum*,—by leaps, rather than by a slow continuous process. Many able reasoners, as Le Conte in this country, and Mivart and Collard in England, hold this view.

Here, as elsewhere, a vast amount of steady conscientious work is required to enable us to solve the problems of the history of life. But, if so, the more the hope for the patient student and investigator. I know nothing more chilling to research, or unfavourable to progress, than the promulgation of a dogmatic decision that there is nothing to be learned but a merely fortuitous and uncaused succession, amenable to no law, and only to be covered, in order to hide its shapeless and uncertain proportions, by the mantle of bold and gratuitous hypothesis.

So soon as we find evidence of continents and oceans, we raise the question, "Have these continents existed from the first in their present position and form, or have the land and water changed places in the course of geological time?" In reality both statements are true in a certain limited sense. On the one hand, any geological map whatever suffices to show that the general outline of the existing land began to be formed in the first and oldest crumplings of the crust. On the other hand, the greater part of the surface of the land consists of marine sediments which must have been derived from land that has perished in the process, while all the continental surfaces, except, perhaps, some high peaks and ridges, have been many times submerged. Both of these apparently contradictory statements are true; and, without assuming both, it is impossible to explain the existing contours and reliefs of the surface.

In the case of North America, the form of the old nucleus of Laurentian rock in the north already marks out that of the finished continent, and the successive later formations have been laid upon the edges of this, like the successive loads of earth dumped over an embankment. But in order to give the great thickness of the palæozoic sediments, the land must have been again and again submerged, and for long periods of time. Thus, in one sense, the continents have been fixed; in another, they have been constantly fluctuating. Hall and Dana have well illustrated these points in so far as eastern North America is concerned. Professor Hull of the Geological Survey of Ireland has recently had the boldness to reduce the fluctuations of land and water, as evidenced in the British Islands, to the form of a series of maps intended to show the physical geography of each successive period. The attempt is probably premature, and has been met with much adverse criticism; but there can be no doubt that it has an element of truth. When we attempt to calculate what could have been supplied from the old eozoic

nucleus by decay and aqueous erosion, and when we take into account the greater local thickness of sediments towards the present sea-basins, we can scarcely avoid the conclusion that extensive areas once occupied by high land are now under the sea. But to ascertain the precise areas and position of these perished lands may now be impossible.

In point of fact, we are obliged to believe in the contemporaneous existence in all geological periods, except perhaps the very oldest, of three sorts of areas on the surface of the earth: 1. Oceanic areas of deep sea, which must always have occupied the bed of the present ocean, or parts of it; 2. Continental plateaus, sometimes existing as low flats or as higher tablelands, and sometimes submerged; 3. Areas of plication or folding, more especially along the borders of the oceans, forming elevated lands rarely submerged, and constantly affording the material of sedimentary accumulations.

Every geologist knows the contention which has been occasioned by the attempts to correlate the earlier palæozoic deposits of the Atlantic margin of North America with those forming at the same time on the interior plateau, and with those of intervening lines of plication and igneous disturbance. Stratigraphy, lithology, and fossils are all more or less at fault in dealing with these questions; and, while the general nature of the problem is understood by many geologists, its solution in particular cases is still a source of apparently endless debate.

The causes and mode of operation of the great movements of the earth's crust which have produced mountains, plains, and tablelands, are still involved in some mystery. One patent cause is the unequal settling of the crust towards the centre; but it is not so generally understood as it should be that the greater settlement of the ocean bed has necessitated its pressure against the sides of the continents in the same manner that a huge ice-floe crushes a ship or a pier. The geological map of North America shows this at a glance, and impresses us with the fact that large portions of the earth's crust have not only been folded, but bodily pushed back for great distances. On looking at the extreme north, we see that the great Laurentian mass of central Newfoundland has acted as a protecting pier to the space immediately west of it, and has caused the Gulf of St. Lawrence to remain an undisturbed area since palæozoic times. Immediately to the south of this, Nova Scotia and New Brunswick are folded back. Still farther south, as Guyot has shown, the old sediments have been crushed in sharp folds against the Adirondack mass, which has sheltered the tableland of the Catskills and of the Great Lakes. South of this again, the rocks of Pennsylvania and Maryland have been driven back in a great curve to the west. Nothing, I think, can more forcibly show the enormous pressure to which the edges of the continents have been exposed, and at the same time the great sinking of the ocean beds. Complex and difficult to calculate though these movements of plication are, they are more intelligible than the apparently regular pulsations of the flat continental areas, whereby they have alternately been below and above the waters, and which must have depended on somewhat regular recurring causes, connected either with the secular cooling of the earth, or with the gradual retardation of its rotation, or with both. Throughout these changes, each successive elevation exposed the rocks for long ages to the decomposing influence of the atmosphere. Each submergence swept away, and deposited as sediment, the material accumulated by decay. Every change of elevation was accompanied with changes of climate and with modifications of the habitats of animals and plants. Were it possible to restore accurately the physical geography of the earth in all these respects, for each geological period, the data for the solution of many difficult questions would be furnished.

It is an unfortunate circumstance that conclusions in geology arrived at by the most careful observation and induction do not remain undisturbed, but require constant vigilance to prevent them from being overthrown. Sometimes, of course, this arises from new discoveries throwing new light on old facts; but when this occurs it rarely works the complete subversion of previously received views. The more usual case is, that some over-zealous specialist suddenly discovers what seems to him to overturn all previous beliefs, and rushes into print with a new and plausible theory, which at once carries with him a host of half-informed people, but the insufficiency of which is speedily made manifest.

Had I written this address a few years ago, I might have referred to the mode of formation of coal as one of the things most surely settled and understood. The labours of many eminent geologists, microscopists, and chemists in the Old and the New Worlds had shown that coal nearly always rests upon old soil surfaces penetrated with roots, and that coal-beds have in their roofs erect trees, the remains of the last forests that grew upon them. Logan and I have illustrated this in the case of the series of more than sixty successive coal-beds exposed at the South Joggins, and have shown unequivocal evidence of land-surfaces at the time of the deposition of the coal. Microscopical examination has proved that these coals are composed of the materials of the same trees whose roots are found in the under-clays, and their stems and leaves in the roof shales; that much of the material of the coal has been subjected to sub-aërial decay at the time of its accumulation; and that, in this, ordinary coal differs from bituminous shale, earthy bitumen, and some kinds of cannel, which have been formed under water; that the matter remaining as coal consists almost entirely of epidermal tissues, which, being suberose in character, are highly carbonaceous, very durable, and impermeable by water,[1] and are hence the best fitted for the production of pure coal; and finally that the vegetation and the climatal and geographical features of the coal period were eminently fitted to produce in the vast swamps of that period precisely the effects observed. All these points and many others have been thoroughly worked out for both European and American coal-fields, and seemed to leave no doubt on the subject. But several years ago certain microscopists observed on slices of coal layers filled with spore-cases,—a not unusual circumstance, since these were shed in vast abundance by the trees of the coal forests, and because they contain suberose matter of the same character with epidermal tissues generally. Immediately we were informed that all coal consists of spores; and, this being at once accepted by the unthinking, the results of the labours of many years are thrown aside in favour of this crude and partial theory. A little later, a German microscopist has thought proper to describe coal as made up of minute algæ, and tries to reconcile this view with the appearances, devising at the same time a new and formidable nomenclature of generic and specific names, which would seem largely to represent mere fragments of tissues. Still later, some local facts in a French coal-field have induced an eminent botanist of that country to revive the drift theory of coal, in opposition to that of growth *in situ*. A year or two ago, when my friend Professor Williamson of Manchester informed me that he was preparing a large series of slices of coal with the view of revising the whole subject, I was inclined to say that, after what had been done by Lyell, Goeppert, Logan, Hunt, Newberry, and myself, this was scarcely necessary; but, in view of what I have just stated, it may be that all he can do will be required to rescue from total ruin the results of our labours.

An illustration of a different character is afforded by the controversy now raging with respect to the so-called fucoids of the ancient rocks. At one time the group of fucoids, or algæ, constituted a general place of refuge for all sorts of unintelligible forms and markings; graptolites, worm-trails, crustacean tracks, shrinkage-cracks, and, above all, rill-markings, forming a heterogeneous group of fucoidal remains distinguished by generic and specific names. To these were also added some true land-plants badly preserved, or exhibiting structures not well understood by botanists. Such a group was sure to be eventually dismembered. The writer has himself done something toward this,[2] but Professor Nathorst has done still more;[3] and now some intelligible explanation can be given of many of these forms. Quite recently, however, the Count de Saporta in an elaborate illustrated memoir,[4] has come to the defence of the fucoids, more especially against the destructive experiments of Nathorst, and would carry back into the vegetable kingdom many things which would seem to be mere trails of animals. While writing this address, I have received from Professor Crié of Rennes a paper in which he not only supports the algal nature of Ru ichnites, Arthrichnites, and many other supposed fucoids, but claims for the vegetable kingdom even Receptaculites and Archæocyathus. It is not to be denied that some of the facts which he cites, respecting the structure of the Siphoniæ and of certain modern incrusting algæ, are very suggestive, though I cannot agree with his conclusions. My own experience has convinced me that, while non-botanical geologists are prone to mistake all kinds of

[1] "Acadian Geology," third edition, supplement, p. 68.
[2] "Footprints and Impressions on Carboniferous Rocks," *Amer. Journ. Sc.*, 1873.
[3] Royal Swedish Academy, Stockholm, 1881.
[4] "Apropos des Algues Fossiles," Paris, 1883.

markings for plants, even good botanists, when not familiar with the chemical and mechanical conditions of fossilisation, and with the present phenomena of tidal shores, are quite as easily misled, though they are very prone, on the other hand, to regard land-plants of some complexity, when badly preserved, as mere algæ. In these circumstances it is very difficult to secure any consensus, and the truth is only to be found by careful observation of competent men. One trouble is that these usually obscure markings have been despised by the greater number of palæontologists, and probably would not now be so much in controversy were it not for the use made of them in illustrating supposed phylogenies of plants.

It would be wrong to close this address without some reference to that which is the veritable *pons asinorum* of the science, the great and much debated glacial period. I trust that you will not suppose that, in the end of an hour's address, I am about to discuss this vexed question. Time would fail me even to name the hosts of recent authors who have contended in this arena. I can hope only to point out a few landmarks which may aid the geological adventurer in traversing the slippery and treacherous surface of the hypothetical ice-sheet of pleistocene times, and in avoiding the yawning crevasses by which it is traversed.

No conclusions of geology seem more certain than that great changes of climate have occurred in the course of geological time; and the evidence of this in that comparatively modern period which immediately preceded the human age is so striking that it has come to be known as preeminently the ice age, while, in the preceding tertiary periods, temperate conditions seem to have prevailed even to the Pole. Of the many theories as to these changes which have been proposed, two seem at present to divide the suffrages of geologists, either alone, or combined with each other. These are, (1) the theory of the precession of the equinoxes in connection with the varying eccentricity of the earth's orbit, advocated more especially by Croll; and (2) the different distribution of land and water as affecting the reception and radiation of heat and the ocean currents,—a theory ably propounded by Lyell, and subsequently extensively adopted, either alone or with the previous one. One of these views may be called the astronomical; the other, the geographical. I confess that I am inclined to accept the second or Lyellian theory for such reasons as the following: 1. Great elevations and depressions of land have occurred in and since the pleistocene, while the alleged astronomical changes are not certain, more especially in regard to their probable effect on the earth; 2. When the rival theories are tested by the present phenomena of the Southern Polar region and the North Atlantic, there seem to be geographical causes adequate to account for all except extreme and unproved glacial conditions; 3. The astronomical cause would suppose regularly recurring glacial periods of which there is no evidence, and it would give to the latest glacial age an antiquity which seems at variance with all other facts; 4. In those more northern regions where glacial phenomena are most pronounced, the theory of floating sheets of ice, with local glaciers descending to the sea, seems to meet all the conditions of the case; and these would be obtained, in the North Atlantic at least, by very moderate changes of level, causing, for example, the equatorial current to flow into the Pacific, instead of running northward as a gulf stream; 5. The geographical theory allows the supposition not merely of vicissitudes of climate quickly following each other in unison with the movements of the surface, but allows also of that near local approximation of regions wholly covered with ice and snow, and others comparatively temperate, which we see at present in the north.

If, however, we are to adopt the geographical theory, we must avoid extreme views; and this leads to the inquiry as to the evidence to be found for any such universal and extreme glaciation as is demanded by some geologists.

The only large continental area in the northern hemisphere supposed to be entirely ice and snow clad is Greenland; and this, so far as it goes, is certainly a local case, for the ice and snow of Greenland extend to the south as far as 60° N. latitude, while both in Norway and in the interior of North America the climate in that latitude permits the growth of cereals. Further, Grinnel Land, which is separated from North Greenland only by a narrow sound, has a comparatively mild climate, and, as Nares has shown, is covered with verdure ln summer. Still further, Nordenskjöld, one of the most experienced Arctic explorers, holds that it is probable that the interior of Greenland is itself verdant in summer, and is at this moment preparing to attempt to reach this interior oasis. Nor is it difficult, with the aid of the facts cited by Woeikoff and Whitney,[1] to perceive the cause of the exceptional condition of Greenland. To give ice and snow in large quantities, two conditions are required,—first, atmospheric humidity; and, secondly, cold precipitating regions. Both of these conditions meet in Greenland. Its high coast-ranges receive and condense the humidity from the sea on both sides of it and to the south. Hence the vast accumulation of its coast snow-fields, and the intense discharge of the glaciers emptying out of its valleys. When extreme glacialists point to Greenland, and ask us to believe that in the glacial age the whole continent of North America as far south as the latitude of 40° was covered with a continental glacier, in some places several thousands of feet thick, we may well ask, first, what evidence there is that Greenland, or even the Antarctic continent, at present shows such a condition; and, secondly, whether there exists a possibility that the interior of a great continent could ever receive so large an amount of precipitation as that required. So far as present knowledge exists, it is certain that the meteorologist and the physicist must answer both questions in the negative. In short, perpetual snow and glaciers must be local, and cannot be continental, because of the vast amount of evaporation and condensation required. These can only be possible where comparatively warm seas supply moisture to cold and elevated land; and this supply cannot, in the nature of things, penetrate far inland. The actual condition of interior Asia and interior America in the higher northern latitudes affords positive proof of this. In a state of partial submergence of our northern continents, we can readily imagine glaciation by the combined action of local glaciers and great ice-floes; but, in whatever way the phenomena of the boulder clay and of the so-called terminal moraines are to be accounted for, the theory of a continuous continental glacier must be given up.

I cannot better indicate the general bearing of facts, as they present themselves to my mind in connection with this subject, than by referring to a paper by Dr G. M. Dawson on the distribution of drift over the great Canadian plains east of the Rocky Mountains.[2] I am the more inclined to refer to this, because of its recency, and because I have so often repeated similar conclusions as to eastern Canada and the region of the Great Lakes.

The great interior plain of western Canada, between the Laurentian axis on the east and the Rocky Mountains on the west, is seven hundred miles in breadth, and is covered with glacial drift, presenting one of the greatest examples of this deposit in the world. Proceeding eastward from the base of the Rocky Mountains, the surface, at firse more than four thousand feet above the sea-level, descends by successive steps to twenty-five hundred feet, and is based on cretaceous and Laramie rocks, covered by boulder clay and sand, in some places from one hundred to two hundred feet in depth, and filling up preexisting hollows, though itself sometimes piled into ridges. Near the Rocky Mountains the bottom of the drift consists of gravel not glaciated. This extends to about one hundred miles east of the mountains, and must have been swept by water out of their valleys. The boulder clay resting on this deposit is largely made up of local debris, in so far as its paste is concerned. It contains many glaciated boulders and stones from the Laurentian region to the east, and also smaller pebbles from the Rocky Mountains; so that at the time of its formation there must have been driftage of large stones for seven hundred miles or more from the east, and of smaller stones from a less distance on the west. The former kind of material extends to the base of the mountains, and to a height of more than four thousand feet. One boulder is mentioned as being forty-two by forty by twenty feet in dimensions. The highest Laurentian boulders seen were at an elevation of forty-six hundred and sixty feet, on the base of the Rocky Mountains. The boulder clay, when thick, can be seen to be rudely stratified, and at one place includes beds of laminated clay with compressed peat, similar to the forest beds described by Worthen and Andrews in Illinois, and the so-called inter-glacial beds described by Hinde on Lake Ontario. The leaf-beds on the Ottawa River, and the drift-trunks found in the boulder clay of Manitoba, belong to the same category, and indicate that throughout the glacial period there were many forest oases far to the north. In the valleys of the Rocky Mountains opening on these plains there are evidences of large local glaciers

[1] "Memoir on Glaciers," Geol. Soc. Berlin, 1881. "Climatic Changes," Boston, 1883.
[2] *Science*, July 1, 1883.

now extinct, and similar evidences exist on the Laurentian highlands on the east.

Perhaps the most remarkable feature of the region is that immense series of ridges of drift piled against an escarpment of Laramie and cretaceous rocks, at an elevation of about twenty-five hundred feet, and known as the "Missouri coteau." It is in some places thirty miles broad and a hundred and eighty feet in height above the plain at its foot, and extends north and south for a great distance; being, in fact, the northern extension of those great ridges of drift which have been traced south of the Great Lakes, and through Pennsylvania and New Jersey, and which figure on the geological maps as the edge of the continental glacier,—an explanation obviously inapplicable in those western regions where they attain their greatest development. It is plain that in the north it marks the western limit of the deep water of a glacial sea, which at some periods extended much farther west, perhaps with a greater proportionate depression in going westward, and on which heavy ice from the Laurentian districts on the east was wafted south-westward by the Arctic currents, while lighter ice from the Rocky Mountains was being borne eastward from these mountains by the prevailing westerly winds. We thus have in the west, on a very wide scale, the same phenomena of varying submergence, cold currents, great ice-floes, and local glaciers producing icebergs, to which I have attributed the boulder clay and upper boulder drift of eastern Canada.

A few subsidiary points I may be pardoned for mentioning here. The rival theories of the glacial period are often characterised as those of land glaciation and sea-borne icebergs. But it must be remembered that those who reject the idea of a continental glacier hold to the existence of local glaciers on the high lands more or less extensive during different portions of the great pleistocene submergence. They also believe in the extension of these glaciers seawards and partly water-borne, in the manner so well explained by Mattieu Williams; in the existence of those vast floes and fields of current and tide borne ice whose powers of transport and erosion we now know to be so great; and in a great submergence and re-elevation of the land, bringing all parts of it and all elevations up to five thousand feet successively under the influence of these various agencies, along with those of the ocean currents. They also hold that, at the beginning of the glacial submergence, the land was deeply covered by decomposed rock, similar to that which still exists on the hills of the southern states, and which, as Dr. Hunt has shown, would afford not only earthy debris, but large quantities of boulders ready for transportation by ice.

I would also remark that there has been the greatest possible exaggeration as to the erosive action of land ice. In 1865, after a visit to the Alpine glaciers, I maintained that in these mountains glaciers are relatively protective rather than erosive agencies, and that the detritus which the glacier streams deliver is derived mostly from the atmospherically wasted peaks and cliffs that project above them. Since that time many other observers have maintained like views, and very recently Mr. Davis of Cambridge and Mr. A. Irving have ably treated this subject.[1] Smoothing and striation of rocks are undoubtedly important effects both of land glaciers and heavy sea-borne ice; but the levelling and filling agency of these is much greater than the erosive. As a matter of fact, as Newberry, Hunt, Belt, Spencer, and others have shown, the glacial age has dammed up vast numbers of old channels which it has been left for modern streams partially to excavate.

The till, or boulder clay, has been called a "ground moraine," but there are really no Alpine moraines at all corresponding to it. On the other hand, it is more or less stratified, often rests on soft materials which glaciers would have swept away, sometimes contains marine shells, or passes into marine clays in its horizontal extension, and invariably in its embedded boulders and its paste shows an unoxidised condition which could not have existed if it had been a sub-aërial deposit. When the Canadian till is excavated and exposed to the air, it assumes a brown colour, owing to oxidation of its iron; and many of its stones and boulders break up and disintegrate under the action of air and frost. These are unequivocal signs of a sub-aqueous deposit. Here and there we find associated with it, and especially near the bottom and at the top, indications of powerful water-action, as if of land torrents acting at particular elevations of the land, or heavy surf and ice action on coasts; and the attempts to explain these by glacial streams have been far from successful. A singular objection sometimes raised against the sub aqueous origin of the till is its general want of marine remains, but this is by no means universal; and it is well known that coarse conglomerates of all ages are generally destitute of fossils, except in their pebbles; and it is further to be observed that the conditions of an ice-laden sea are not those most favourable for the extension of marine life, and that the period of time covered by the glacial age must have been short compared with that represented by some of the older formations.

This last consideration suggests a question which might afford scope for another address of an hour's duration,—the question how long time has elapsed since the close of the glacial period. Recently the opinion has been gaining ground that the close of the ice age is very recent. Such reasons as the following lead to this conclusion: the amount of atmospheric decay of rocks and of denudation in general, which have occurred since the close of the glacial period, are scarcely appreciable; little erosion of river-valleys or of coast-terraces has occurred. The calculated recession of waterfalls and of production of lake-ridges leads to the same conclusion. So do the recent state of bones and shells in the pleistocene deposits, and the perfectly modern facies of their fossils. On such evidence the cessation of the glacial cold and settlement of our continents at their present levels are events which may have occurred not more than six thousand or seven thousand years ago, though such time estimates are proverbially uncertain in geology. This subject also carries with it the greatest of all geological problems, next to that of the origin of life; namely, the origin and early history of man. Such questions cannot be discussed in the closing sentences of an hour's address. I shall only draw from them one practical inference. Since the comparatively short post-glacial and recent periods apparently include the whole of human history, we are but new comers on the earth, and therefore have had little opportunity to solve the great problems which it presents to us. But this is not all. Geology as a science scarcely dates from a century ago. We have reason for surprise in these circumstances that it has learned so much, but for equal surprise that so many persons appear to think it a complete and full-grown science, and that it is entitled to speak with confidence on all the great mysteries of the earth that have been hidden from the generations before us. Such being the newness of man and of his science of the earth, it is not too much to say that humility, hard work in collecting facts, and abstinence from hasty generalisation, should characterise geologists, at least for a few generations to come.

In conclusion, science is light, and light is good; but it must be carried high, else it will fail to enlighten the world. Let us strive to raise it high enough to shine over every obstruction which casts any shadow on the true interests of humanity. Above all, let us hold up the light, and not stand in it ourselves.

[1] *Proc. Bost. Soc. Nat. Hist.* xxii. *Journ. Geol. Soc. Lond.*, Feb., 1883.

UNIVERSITY AND EDUCATIONAL INTELLIGENCE

Dr. Matthew Hay, assistant to the Professor of Materia Medica in the University of Edinburgh, has been appointed to the Chair of Medical Logic and Medical Jurisprudence in the University of Aberdeen, *vice* Prof. Ogston resigned.

The constitution of the College for North Wales, which is to be established at Bangor, having been approved by the Education Department, arrangements are actively progressing for its opening in January, in order to secure the annual grant of 4000*l.* which has been offered by Government. As in South Wales, temporary premises will be acquired, and possibly the Masonic Hall, a commodious building lately erected by Major Platt, will be so utilised. Nothing definite is yet arranged as to the site of the College; but it is understood that Lord Penrhyn, who has evinced a very active interest in the movement, and to whom will probably be offered the honour of being first president, will afford every facility to the executive committee. About 30,000*l.* has been promised in subscriptions towards the building fund.

SCIENTIFIC SERIALS

Journal of the Russian Chemical and Physical Society, vol. xv. fasc. 6.—On the action of haloidhydric acids upon oxymethylene, by B. Tischenko.—On the constitution of the waters that accompany naphtha and are ejected by mud volcanoes, by A. Potilitzin.—On the formation of bromides of aromatic hydrocarbons by the action of bromine and bromide of aluminium on the volatile parts of naphtha, by G. Gustavson.—On the

formation of tertiary alcohols by the method of Butleroff, by W. Markovnikoff.—On propyl-allyl dimethyl carbinol, by M. Putochin.—On the determination of carbon in cast-iron and steel, by G. Zabudsky.—On the decomposition of orthoclase by putrefied matter, by S. Meschersky.—Notes by W. Tikhomiroff and A. Lidoff.—On the application of centres of acceleration of a superior order to the parallelogram of Tchebycheff, by N. Joukovsky.—On the magnetic momentum of bundles of iron-wire, by P. Bakhmetieff.

Bulletin de la Société des Naturalistes de Moscow, 1882, No. 4.—New mints, especially the European ones, by M. Gandoger, being a description (in Latin) of forty-two new species of *Pulegium*, four species of *Preslia*, Opiz., and 135 species of *Mentha*.—On the arrangement of plants for keeping upright, and on the supply of water for exhalation, by V. Meschajeff, being a preliminary account (in German) of researches into the distribution and functions of the so-called mechanical tissue.—On the great comet 1882 II., by Th. Bredichin (in French).—Scientific results of the borings undertaken at Moscow for water supply and canalisation, by H. Trautschold (in German), being the result of twenty-three borings made at Moscow which have pierced the boulder-clay 0·6 to 8 metres thick, or alluvial sands in the valleys; a sheet of eluvium; the four Upper Jurassic layers of green sandstone with *Ammonites fulgens*, Aucella deposits with *Aucella mosquensis* and *Ammonites subditus*; black sand with *Ammonites virgatus*, and the usual black Jurassic clay which affords a compact and widely spread layer; a series of red and mottled clays, which may be Permian, underlie the Jurassic deposits and cover the Upper Carboniferous limestone.—Observations on atmospheric electricity at Murom, by N. Zvorykin.—New additions to the kinetic of liquids, by Th. Sludsky (both in Russian).—The European and Asiatic species of *Erirrhinus*, *Notaris*, *Scaris*, and *Dorytomus*, revised by J. Faust (in German).

Journal de Physique Théorique et Appliqué, August.—On a gravity barometer, by M. Mascart (three diagrams).—Description of a new form of equatorial telescope and its installation at the Paris Observatory, by M. Lœwy (one diagram).—On a synthetic apparatus for producing circular double refraction; on the radiation of silver at the moment of solidification, by M. J. Violle.—The index of refraction of Iceland spar, by M. E. Sarazin.—Selective absorption of solar energy, by Mr. Langley.—On an instrument for correcting gaseous volume, by Mr. A. Vernon-Harcourt.—Change in volume of hydrated salts under the action of heat; the corresponding chemical changes, by M. E. Wiedemann.

Archives des Sciences Physique et Naturelle (*de Genève*).—Memoirs on the new registering barometer in the meteorological observatory of Lausanne, by MM. H. Dufour and H. Amstein.—The structure of glaciers, by M. Ed. Hagenbach-Bischoff.—The rheolyser, by M. E. Wartmann.—On the rotation of polarisation of quartz, by MM. G. L. Soret and E. Sarazin.—Observations on cometary refraction, by M. W. Meyer.—On the amount of hail that fell during the storms of August 21, 1881, and July 13, 1788, and some remarks on the history of hail protectors, by M. Ch. Dufour.

SOCIETIES AND ACADEMIES

Paris

Academy of Sciences, August 27.—M. Blanchard, president, in the chair.—A telegraphic despatch received by M. Dumas, through M. Pasteur, from the French Cholera Mission in Egypt, announces several important discoveries of a constant character, which will be communicated in detail later on.—New researches on the mode of action of the antiseptics used in staunching sores, by M. Gosselin. From experiments made on rabbits and frogs, it results that phenic acid, camphorated spirits, and similar solutions, are useful in two ways, partly by destroying germs, and thus preventing putrefaction, partly as astringents, by coagulating the albumen of the blood.—On the law of sequence in the evolution of the first vessels in the leaves of the Cruciferæ (second part), by M. A. Trécul.—Astrophotographic studies, by M. Ch. V. Zenger.—On the production of the fundamental telluric groups A and B of the solar spectrum by an absorbing layer of oxygen, by M. Egoroff.—Remarks on a fœtus which remained fifty-six years in its mother's womb, by M. Sappey.—On some methods for determining the positions of the circumpolar stars, by M. O. Callandreau.—On the measurement of time; a reply to the observations of E. J. Stone, by M. A. Gaillot.—On a formula relative to the velocity of waves; a reply to M. Gouy, by Lord Rayleigh. In the *Comptes Rendus* for May, 1882, M. Gouy, referring to Lord Rayleigh's correspondence in NATURE during the year 1881, recalls a memoir previously published by him in the *Comptes Rendus* for November, 1880, in which occurs the formula

$$U = \frac{dn}{dk} = \frac{d\,\frac{1}{T}}{d\,\frac{1}{\lambda}}.$$

To this Lord Rayleigh replies that this formula had already been given by him in the first volume of his work on "The Theory of Sound," published in 1877.—Researches on the groups of finite order contained in the group of the homogeneous quadratic substitutions with three variables, by M. L. Autonne.—On the absorption of the ultra-violet rays by the aqueous humours of the eye and by some other substances, by M. J. L. Soret.—On the measurement of the potential differences and resistances between electrodes, by M. G. Cabanellas.—A new method of preparing the oxychloride of phosphorus, by M. E. Dervin.—Researches on the influence of the recurrent nerves on the respiratory movements, and on the modifications of these movements under the influence of anæsthesia, by M. Laffont.—On a falling star observed at Lille on the evening of August 11, by M. Hequet.

Vienna

Imperial Academy of Sciences, July 5.—T. V. Tanovsky, on amido-azobenzene-parasulphonic acid.—E. Meissl and F. Strohmann, on the formation of fat by hydrocarbons in the animal body.—A. Gehmacher, researches on the influence of the pressure exerted by the bark on the growth and structure of the tree.—L. von Frankl and C. Freund, on the atrophy of skeletal muscles.—E. Auer von Welsbach, on the earths of gadolinite of ytterbium.—T. Kachler and F. V. Spitzer, on oxy-camphor prepared from camphor-bibromide.—T. Wiesner and R. von Wettstein, researches on the laws of growth of vegetable organs.—S. Fuchs, the histogenesis of the cortex cerebri of man.—A. Lustig, the knowledge of the course of nerve-fibres in the spinal cord of man.—F. K. Ginzel, astronomical researches on eclipses (part 1).—E. von Fleischl, on the laws of nerve irritability (part 7): on the irritability of currentless nerves.

CONTENTS

THURSDAY, SEPTEMBER 13, 1883

SCIENTIFIC ASPECTS OF THE JAVA CATASTROPHE

CAUTION and scepticism, which are necessary to the student of every branch of natural science, ought to be the especial attributes of the vulcanologist and seismologist. No other natural phenomena so strikingly affect the imagination or so powerfully excite the fancy as do the volcanic outburst and the earthquake. These catastrophes usually occur too with such startling suddenness and with such an entire absence of warning that the witnesses are not unnaturally paralysed by fear and terror. Under such circumstances the wildest and most improbable stories are received and circulated with easy credence, and no attempt is made to separate the real from the imaginary.

Illustrations of these remarks might be adduced in connection with each of the great subterranean disturbances which have taken place during recent years. Thus the accounts received of the earthquake of Agram stated that fissures had opened in the ground from which smoke and flames issued and along which volcanic cones were thrown up. The report of the Commission appointed by the Hungarian Government to investigate the facts of the case upon the spot proves conclusively that these stories had no other foundation than the emission of small jets of water and the formation by them of sand-cones, a phenomenon frequently witnessed during earthquake shocks. An article in the last number of this journal upon the recent earthquake in Ischia shows that similar discrepancies exist between the first hastily-published accounts and the soberer testimony of careful observers.

In the case of the Java catastrophe, however, there appears to have been at least one attempt to hoax the newspaper-reading public by deliberately manufactured accounts of the event. A detailed statement purporting to come from an eye-witness, and telegraphed by way of America, was published in many of the daily papers. The circumstances recorded in this statement would have been startling indeed had they been true, but, as a writer in the *Scotsman* has already pointed out, the account bears too manifestly on its face abundant proofs of its want of genuineness.

Setting aside these fictitious accounts, and making every allowance for the exaggeration naturally resulting from terror, and the difficulty which under the circumstances of the case there must be of obtaining reliable information, sufficient remains to prove that the recent catastrophe resulted from one of the grandest and most destructive volcanic outbursts which have occurred in modern times.

The scene of this outburst was at what is at the present day probably the focus of the most intense volcanic activity upon the face of the globe. The Island of Java contains no less than forty-six great volcanic mountains, nearly one half of which have been in activity during historical times. This chain of volcanoes is continued in the southern part of Sumatra. Since the colonisation of these islands several volcanic eruptions on the very grandest scale have taken place, at points not very distant from the scene of the recent catastrophe. In 1772 occurred the great eruption of Papandayang, when the whole upper part of the mountain was blown away, leaving a vast crater fifteen miles long by six miles broad. The quantity of material ejected during this eruption was so great that, according to Dr. Junghuhn, an area of seven miles radius around the mountain was in a single night covered with scoriæ and ashes to the depth of nearly fifty feet. Forty native villages were overwhelmed, and 3000 persons perished. In 1822 the neighbouring volcano of Galunggong was in eruption, and 114 villages were buried beneath the scoriæ and ashes, while the destruction of human life was so great that more than 4000 killed were recorded in the official reports.

According to the most reliable accounts received up to the present time, the recent outbreak would appear to have been far more fatal to human life than either of its predecessors, and the most potent agent of destruction in this, as in so many other cases, would seem to have been the great sea-wave produced by the earthquake-shock, rather than the showers of materials ejected from the volcanoes.

Divested of their marvellous accompaniments, and read by the light of modern vulcanologic science, the accounts already received of this great catastrophe seem to prove the occurrence of the following events:—First, the ejection of enormous quantities of fragmentary materials; secondly, the production of great changes in the form and outlines of the volcanic Island of Krakatoa; thirdly, the throwing up of a line of new volcanic cones on a fissure opened in the sea-bed between Java and Sumatra; and. fourthly, the occurrence of one or more earthquake shocks, giving rise to forced sea-waves of great destructiveness.

The quantity of materials ejected during these eruptions is proved by two facts recorded in the accounts already published: firstly, the widespread and long-continued darkness, doubtless produced by the clouds of finely comminuted dust carried away from the volcano by the wind; and, secondly, by the vast mass of scoriæ which seems to have accumulated upon and floated over many portions of the surface of the surrounding seas.

Concerning the extent and nature of the changes of the features of Krakatoa we must await further and reliable evidence. As in the case of Papandayang, the destruction of the volcano was doubtless primarily due to the eruptive action, which truncated the cone and formed a gigantic crater, and whether or not this action was accompanied by subsidence, whereby the disappearance of the island was consummated, it remains for further investigations to determine. It is well, however, to bear in mind that many reputed cases of the submergence of islands have on further examination resolved themselves into the removal of materials by explosive action, just as most instances of the elevation of volcanic islands above the sea-level have been doubtless due to the piling up of the materials above the level of the waves.

The position and relations of the new line of volcanic cones must be determined by the surveying vessels which will doubtless be sent to the spot so soon as it is considered safe to do so. Fortunately a number of admirable charts of these seas have been constructed by the hydrographers of this and other countries, and by a comparison of these with the new charts which will now have to be

made we shall be able to judge of the actual changes in the features of this part of the globe which have been wrought by this great outburst. It appears to have been the first belief of the naval authorities upon the spot that these changes were of such magnitude as to render it unsafe for vessels to attempt to pass the Straits of Sunda until new surveys had been made. Later accounts, however, prove that the principal channel by which vessels traverse the straits has remained unaffected by the eruptions.

We may confidently hope that a comparison of the times at which the great sea-wave, produced by the earthquake, reached various ports will enable us to correct and extend our knowledge concerning the depth of certain portions of the Pacific and Indian Oceans. For this, as for many other details of great importance to science, we must await the careful collection and sifting of evidence which will doubtless be undertaken by a Commission appointed by the Dutch Government.

The portion of the Island of Java visited by this terrible calamity is exceedingly fertile, rich, and populous, and if the present estimate of the loss of life be not excessive, this catastrophe must probably rank as the greatest which has occurred in modern times, so far as the destruction of human life is concerned.

The repeated eruptions of Vesuvius and Etna have failed to drive away the vine-dressers from the fertile slopes of those mountains, and in the same way the forces of destruction which evidently lie dormant beneath Java only produce temporary interruptions in its story of plenty and prosperity. As it is now, so was it in past geological times. The districts of Hungary, Auvergne, and the Inner Hebrides, which in former geological periods were subjected to subterranean disturbances similar in character and violence to those which now affect Java, were, in the intervals between the volcanic outbursts, rich and fertile, a fact which is testified to by the remains of forests and of the wild animals which roamed through them, found in the deposits lying between successive lava-flows. Volcanic eruptions are frequently very destructive; earthquake shocks are often still more fatal to man and his works; but fortunately successive catastrophes of both kinds are usually separated by long intervals of time, and it is the recognition of this fact which leads men to brave alike both kinds of danger.

AUTUMN SANITATION

IT is not only the steady decline of cholera in Egypt that gives substantial assurance that we shall now escape any epidemic in this country, but it is also the advancing season. There are, however, few subjects concerning which less is known than the influence of climate and season on the progress of the infectious diseases. But, as regards cholera, we know from experience that it is not very likely to make its appearance in this country when once the colder weather has set in. It has generally first shown itself with us during the hot summer months, and it is probable that a foul, damp air, together with a certain degree of warmth, are most favourable to its prevalence. It is not that we have never suffered from it during the colder months, for it was somewhat widely prevalent in October and November of 1853, the year which preceded the great epidemic of 1854, when so many cities, both in the Old and New World, were devastated. And even though actual winter has, even on such an occasion as that referred to, for a time completely checked the further progress of cholera, yet there is no reason to believe that any cold which the human frame can bear has the power of destroying the infection. At Moscow and at Orenburg in 1830 cholera prevailed in spite of a temperature of $-4°$ F. And judging from analogy it would appear that much lower degrees of temperature than these fail to destroy infections such as that of cholera. Thus, tubes containing the characteristic spores of the bacillus anthracis have been exposed to a temperature of $-32°$ F.; and yet on being thawed they have remained potent for harm as before. Indeed, we may infer that, provided other conditions necessary for the life of the contagion are present, warmth is not essential, and that no amount of cold is absolutely incompatible with the development either of the cholera poison or of the infection of many other contagia. Still, cholera has been with us essentially a summer epidemic, and as each week of the present month passes away without its being imported into the country we may feel more and more assured that we have succeeded in escaping the danger of an outbreak.

There is also another disease that with the commencement of autumn rapidly subsides. We refer to that form of diarrhœa known as infantile, a specific disease that causes year by year a large fatality, especially in certain of our manufacturing towns. This disease, too, is, to a certain extent, one of season. At Leicester, Preston, and Nottingham, the death-rate from this cause is always exceptionally high during the third quarter of the year, its main incidence being on the first two months of the quarter. Thus, taking the year 1881, it appears that, whereas the mortality from this cause in the twenty large towns and cities of England was 409 and 593 respectively, it rose to 4390 in the third quarter. But temperature alone does not account for this large mortality. Oldham, Rochdale, and Halifax resemble the three towns abovenamed in many important social and other respects; they do not materially differ from them as regards climate, and yet the infantile diarrhœa death-rate is with them always exceptionally low. Indeed the difficult problems connected with the etiology of this disease are such that the Government have commissioned Dr. Ed. Ballard to make a comprehensive inquiry into its causes, and it is hoped that his investigations, which have now been in progress for more than two years, will throw important light on the whole subject.

But as the diseases of one season subside those of another make their appearance. Many of the public are under the vague impression that cold weather and a good sharp frost have some effect in "clearing the air" and in getting rid of infection. But, as regards some diseases, this is altogether a mistake. Thus, typhus fever and small-pox, which are at their lowest ebb, or altogether disappear, during the hot summer months, tend to reappear as the autumn sets in, and they assume their greatest force at the depth of winter. But this again is probably not all due to seasonal causes. The cold with which these diseases are so specially related forces those who are poor and ill-clad to remain huddled together

indoors; the greater the cold, the worse the overcrowding in the densely-peopled portions of our cities, and hence opportunities for personal infection, which are at their minimum in the hot summer months when doors and windows are open, reach their maximum in the coldest months.

Some diseases find the autumn months especially congenial to their development and spread, and of these the one that merits most attention as the present season advances is enteric fever, or typhoid fever as it is more commonly, but less appropriately, named. So peculiarly is this affection identified with the autumn months that amongst its best-known synonyms the terms autumnal fever or fall fever are well known; and under ordinary circumstances the largest number of attacks occurs in the month of October; November follows next, and then come September and August. Fortunately, as regards enteric fever also, something more than season is needed to favour its appearance and spread. The infection of enteric fever is of all others the one that in our climate can most easily be rendered harmless. For its development it needs that special form of filth which is associated with human excreta, and whether these foul the air of our dwellings by reason of defective means of drainage, or whether they pollute the soil on which we live or from whence we derive our water supplies, it matters little. Wherever the contamination is there is a soil adapted to the reception and cultivation of the infection. In this respect enteric fever resembles cholera, and, if the warnings which have been so widely circulated throughout the country during the past few months with regard to the measures that should be taken with a view to the prevention of the latter disease have not been unheeded by the public and by our sanitary authorities, we should this autumn feel more satisfied than we ever have done that the conditions necessary to the spread of this autumnal fever do not prevail amongst us as they have done heretofore. Scarlet fever, again, often reaches its widest prevalence towards the commencement of the fourth quarter of the year; and respiratory diseases, including pneumonia, which has now come to be regarded as much more frequently a specific pulmonary affection associated with defective local sanitary circumstances than a mere result of cold, as a rule rise steadily in prevalence until about the middle of November, when they again tend to subside.

Seasons and their predisposing influences must necessarily go and come, but they alone do not suffice for the production of the specific infections. As the science of preventive medicine progresses, we may hope that other conditions, as necessary to the development of infection as are the climatic ones, will steadily be removed, and that our sense of security against preventable disease may not be troubled by mere considerations of season. For the moment the indications are to secure that the air in our dwellings, as also our water, milk, and other food supplies, shall be as far as practicable free from the risk of all contaminating influences; to maintain, as regards our homes and our bodies, the utmost procurable cleanliness; and so to clothe ourselves that we shall be able to resist the depressing effects of the damp and cold which are sure to alternate with the finest weather an autumn season can produce.

TROPICAL AGRICULTURE

The Tropical Agriculturist: a Monthly Record of Information for Planters of Coffee, Tea, Cocoa, Cinchona, Indiarubber, Sugar, Tobacco. Cardamoms, Palms, Rice, and other Products suited for Cultivation in the Tropics. Compiled by A. M. and J. Ferguson, of the *Ceylon Observer.* (London: J. Haddon and Co., 3, Bouverie Street, 1882.)

A BULKY volume containing thirteen monthly numbers and occupying more than a thousand pages can hardly fail to contain a large amount of varied and useful information, especially when it deals with such a subject as tropical agriculture. Not only tropical but subtropical regions are laid under tribute, the latter being represented chiefly by Southern Australia, New Zealand, and China, while Ceylon and the various provinces of India receive, as might be expected, the greatest share of attention. There are, moreover, abundant references to several oceanic islands which have within recent years been invested with more or less political interest. Thus of Fiji it is stated that the planters are chiefly concerned in growing sugar-cane, coffee, and cotton, and though it is claimed that the first-named is indigenous, the best kinds of cane grown in the plantations have been introduced. The Sea Island cotton is easily cultivated, but the production has lately fallen off, the quotations being too low to tempt the planter. Tobacco answers well, and it is believed that cocoa, tapioca, ginger, pepper, and all sorts of spices, camphor, and vanilla, might also be profitably grown. Madagascar appears to have bright agricultural prospects before it, as it is admirably adapted to the cultivation of sugar and coffee, and indeed as a sugar-growing country it seems likely that it will before many years leave Mauritius in the background. The small islands between Madagascar and the mainland are enthusiastically spoken of as a new planting region: "situated in a most salubrious climate, between the southern tropic and the line, they are admirably adapted for the cultivation of sugar, coffee, vanilla, cocoa, spices, cloves, and other products, many of which are pure articles of luxury, and will always command a high price in the European market."

Judging from the space allotted to them and the amount of interest that appears to centre round them, the staple crops of tropical agriculture are tea, coffee, cocoa, and sugar; cinchona and tobacco; indiarubber, cotton, and gums, to say nothing of rice. Of the first group, tropical countries may rest fairly securely in the cultivation of tea, coffee, and cocoa, and although the sugar-cane is largely planted in the southern United States and the sugar-beet is so extensively grown in Europe, yet we gather that sugar cultivation is a thriving industry in India, Java, Mauritius, the Malay peninsula, Queensland, Fiji, Brazil, Jamaica, and Trinidad. Cinchona is of course a highly popular subject, and from this volume alone a very large amount of useful information may be gleaned. On account of the rapid development of the electrical industries and of the increasing use of elastic tires for wheels, the demand for indiarubber and guttapercha is continually increasing, and this will no doubt be met by the extended cultivation of these products. The official papers relating to the introduction of the Para and Ceara rubber plants into

India are reproduced; the original seeds which were obtained in South America were sown at Kew, and the young plants sent thence to the East, but the precarious nature of the undertaking may be inferred from the fact that only about three per cent. of the seeds germinated. It is pleasant to read here and there spontaneous testimony to the value of the Royal Gardens at Kew and of the Indian Botanic Gardens.

Of controversial subjects the coffee leaf disease attracted most notice, considerable space being devoted to the reports and letters of Mr. Marshall Ward, and to the discussions arising therefrom. On p. 15 is a complacent suggestion that as crops cannot always be got from the branches of the coffee tree they might be got in another form from the roots by grinding up the cockchafers that there abound and selling the beetle powder, mixed with a little coffee, as real coffee, carrying on the entire manufacture in Ceylon to prevent any tampering on the part of dishonest middlemen in London! This pleasant notion is based on the assumption that "the British public will consume anything not absolutely dirt that is sufficiently adulterated to suit their palates."

The marked contrast between our home agriculture and that of the tropics is afforded in the very few and scanty references to live stock of any kind. English agriculturists are continually relying more and more on their flocks and herds and less on their corn crops for remunerative returns. There is, indeed, a solitary reference to Aden cattle, which are bred inland, and derive their name only from the port whence they are shipped. They have a high reputation as dairy stock, and have been used with success for crossing with some of the Indian herds on the Government farm at Saidapet, Madras. The only allusion to sheep farming is to that of Australia.

Of course, in such a volume as the one before us, the matter is necessarily of a very heterogeneous character, but it is all concerned more or less directly either with agriculture itself, or with the economic and industrial aspects of the art as pursued in the hotter regions of the globe. As a record of the experience of tropical planters, of the difficulties and drawbacks of climate and of soil they have to contend with, of the good or indifferent results which have attended their efforts at acclimatisation, of the measures they have adopted to minimise the evil effects of insect or fungal attacks, and not less as an interesting historical summary of the progress of tropical agriculture, such a work as this carried out on the lines on which it has been begun cannot fail to possess a permanent value. Young men especially, who, having learnt something of the art of agriculture in the stern school of British farming, would fain try their skill under a tropical sun, will find collected here a large mass of useful information such as perhaps it would hardly be possible to obtain elsewhere. W. FREAM

OUR BOOK SHELF

Vorlesungen über Pflanzen-physiologie. Von Julius Sachs. (Leipzig: Wilhelm Engelmann, 1882-83.)

THE fourth edition of Prof. Sachs's well-known text-book of botany being nearly exhausted, his friends and publishers urged him to set about the preparation of a new edition; but the revisal necessary for the publication of the fourth edition had been so irksome that nothing would induce the author to attempt the task again. Moreover his views on many important questions concerning the physiology of plants had changed: points once considered all-important had lost much of their importance, and expanded views acquired in the progress of time could not be made to fit into the framework of the old work.

Prof. Sachs for years has been a most successful teacher of botany. His text-book, large and technical though it was, has had a most successful career in German-speaking countries; translated into French by an eminent French botanist, and into English under the auspices of the Delegates of the Clarendon Press at Oxford, there needed no higher testimonies to its worth; still, instead of being content with the success of his volume, he now refuses to look at it, and utterly casts it from him. "As long as the artist is pleased with his work, he can add a touch here and there, or can even go in for greater changes; but this is not sufficient when the work has ceased to be the expression of his idea, and this is the attitude I stand in with regard to my text-book." This state of mind has resulted in the publication of the fine volume which we now notice; in size and general appearance it differs very little from the author's text-book, but under the style of lectures it appeals to a wider circle of readers than mere college students. Ardently anxious that the very important modern views on plant physiology should be known to all fairly educated people, these lectures, without sacrificing scientific accuracy, are written in a style as free as possible from the fatiguing use of long and purely technical words; they are purposely written too in a slightly dogmatic style, for it is clearly a lecturer's duty to put before his audience his own individual views upon even debated questions; his hearers have a perfect right to know what impresion the general aggregate of scientific facts has made upon his mind, and while this would be out of place in a technical text-book of the science, it harmonises well with a course of lectures.

At the end of each lecture some—we could have wished for more—bibliographical notes are added for the benefit of those readers who wish to plunge deeper into the subject.

The publishers wished that a new revised edition of the systematic part of the text-book should have been "tagged" on to these lectures, but Prof. Sachs declared that he had neither time nor inclination for the task, which he commits to the care of Prof. Gœbel, whose separate treatise on this part of the subject has lately made its appearance. We hope the day may not be far off when these charming lectures on plant physiology will be read in English by a large number of our cultivated public. E. P. W.

Accented Five-figure Logarithms of Numbers from 1 *to* 99999 *without Differences.* Arranged and Accented by L. D'A. Jackson. (London: W. H. Allen, 1883.)

IN this work are comprised two sets of tables. The first set (pp. 1-221) is indicated by the above title-page; the second is entitled "Accented Five-figure Logarithms of Sines, Tangents, Cotangents, and Cosines of Angles from 0° to 90° to every Hundredth of a Degree" (pp. 224-270). There is, further, a one-page "Comparison of French and English Decimal Scientific Systems at 32° and 39° Fahrenheit in vacuo." The possessors of the same author's "Accented Four-figure Logarithms" are already acquainted with his principles of accentuation; to those who have not this work we need only say that *excess* and *defect* are clearly indicated in the printing, and that the degree of accuracy attainable in any piece of calculation is very rarely inferior to that reached by the longer calculations with the ordinary seven-figure tables. The logarithm of any number is seen at a glance, so that there

is no using of differences, proportional parts, or anti-logarithms. In his introduction the author works out some examples with ordinary unaccented five and seven-figure tables, and with these accented tables. On the hypothesis that the tables are correctly printed—we have detected no error—we commend this book as being one that will save much time in calculation without entailing a loss of accuracy. The figures are very clearly printed.

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications.*

The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to insure the appearance even of communications containing interesting and novel facts.]

The Earthquake of Ischia

In NATURE, August 30, p. 414, a correspondent remarks: "The recent catastrophe in the Island of Ischia has called the attention of those who make a study of such disturbances of the earth's surface to the simultaneous occurrence of earthquakes in various parts of the world"; while in NATURE of August 16, p. 368, Mr. Milne, in his article on "Earth Pulsations," says: "The directions in which these tips of the soil take place, which phenomena are noticeable in seismic as well as microseismic motions, Rossi states are related to the directions of certain lines of faulting."

With a view to call attention to the connection between earthquakes occurring in different parts of the globe, either simultaneously or successively, I submitted to the British Association at Swansea a paper on the relation between coast-line directions and localities in Europe marked by frequency of earthquakes, as also a map illustrative thereof. In this paper, and in a previous one published in the *Transactions of the Royal Irish Academy*, I started with, and endeavoured to prove, the principle laid down by Rossi as to the connection between lines of faulting and earthquake movements, and the map submitted tended to show this relation as being very marked for certain lines of direction.

I now beg leave to call attention to the following lines of action shown thereon in relation with the Ischia earthquake. Amongst the lines cited were those of the *east coast of Sweden* and *east coast of Sardinia*, both nearly parallel, and thereon marked. As regards the first, I state in the memoir (p. 508 of the *Proceedings of the Royal Irish Academy*, 2nd series, vol. iii.; *Science*, No. 8, May, 1882):—

"The section of this line between Rome and Rimini is one of the best marked earthquake lines in Italy, whilst the section between Pola and Brück is also well defined as a direction by a series of points along which shocks have been continually occurring." I further add: "Between Palermo and Naples a parallel to this coast-line seems to be marked by earthquake movements cited as having extended from one point to the other (April 16, 1817)." Now this line passes precisely at Ischia, and, being extended, passes at or near the following places noticeable for earthquakes:—

Corleone, Palermo, Ischia, Teano, Isernia, Lanciana, Grossa Islands, Neustadt (Carmola), Marburg, Semering Pass, Neuburg (near Vienna), Znaim, Glatz, Breslau. Its prolongation represents the axis of the Baltic and the coast-line of Finland from Nystad to Biorntborg.

The west coast of Sicily furnishes a parallel to this direction, and on it are the following earthquake points:—

Frosinone, Aquila, Aseoli, Laybach, Hirschberg, the Rie-engebirge, Glogau, and quite recently (May) the points in Finland—Nykerleby, Wasa, and Itterseppo.

Although unacquainted with the geology of the countries traversed by these directions, I am convinced that there occurs marked jointing both in Sicily and in Italy corresponding to the direction of these lines, about N. 10° 30′ E.

It seems to me that they furnish a means of connecting these phenomena, and that they allow of some approach being made to the determination of the laws which govern the occurrence of earthquakes in these parts of Europe.

J. P. O'REILLY

Mr. Romanes and Modern Philosophy

As an adherent of the school of thought which Mr. G. J. Romanes (NATURE, vol. xxviii. p. 387) has subjected to such a spirited criticism, I may perhaps be allowed to make a few comments upon his position.

If all science, at least all that has outgrown the mere registration of facts, consists in the application to the latter of certain necessary principles of thinking, it is at least possible that philosophy—an important part of whose function is the systematic elucidation of these principles—may be of some use in scientific procedure.

Mr. Romanes, if he accepts the fact that nature is an object of knowledge, cannot deny that it is governed by the essential conditions of knowing. It is usual, but hardly fair, to confound such obvious statements with the subjective idealism which "makes the universe revolve round the philosopher." A necessary principle is one that evidently does not apply simply to the experience of a single man, a corollary that ought to save much confusion concerning the relations between mind and matter. If, too, it had been recognised that such principles admit of no ulterior possibilities, we would have been spared the controversy anent non-Euclidean space.

With regard to their application to facts of experience, or conceptions derived from these, it may be remembered that the sciences in whose results we place the most confidence—*e.g.* mathematics, mathematical physics, and astronomy, are chiefly deductive. With regard to the portions of these which apparently consist of empirical generalisations, it should not be impossible to show that they, in common with the whole of mathematical science, really flow from the constitution of our experience of nature.

In dealing with the question of mechanism and teleology it is a common error to think that in using the higher categories there is any supersession of such principles as those of cause and effect. As objects of outer experience, organisms are certainly conditioned by the latter; but when they are regarded as subjects, as well as objects, we are compelled to recognise the one-sidedness of such categories—to read into them, as it were, our own active subjectivity. Even the most dogmatic materialist might suspect that the conception of cause and effect is not adequate to a complete solution of the problems presented by living beings. In accepting causality, then, as a truth of universal application, it is not in any sense unscientific to regard it as merely one among the principles which regulate nature.

With regard to the special applications of teleology (in the philosophical sense), it is easy to find instances of incorrect deduction, because of the undeveloped condition of this portion of the subject. Treatises upon it can therefore only be considered as suggestive. Mr. Romanes seems to fear that such efforts will create a dogmatism fatal to scientific progress, although he is aware that the tendency of the times is in exactly the reverse direction. The *a priori* of modern philosophy is of a far different nature from that of scholasticism, and may be in many cases quite as scientific as that which determines the impossibility of perpetual motion, or prophesies a transit of Venus.

Crewe, August 30 ALFRED STAPLEY

Animal Intelligence

THE columns of NATURE have sometimes been open to statements illustrating the practical sagacity of animals of the lower classes. Allow me to place before you the history of an occurrence which appears to prove the power of organisation in the common house-mouse.

The room to which I shall refer is one of several which were built as additions to the original house; it was used solely as a bedroom. I think it very probable that the old and the new apartments were so united that there was no clear mouse-way between them.

I had been sleeping alone in the room, I believe for several weeks, without any disturbance. One night I was woke up, I believe some hours after midnight, by such a grinding under the floor as I never heard before. It was evidently useless for me to attempt to interrupt it, and indeed I was rather curious to observe what would ultimately happen, and I lay quiet in bed. Daylight approached, and still the grinding continued. At last the noise suddenly ceased, and in a minute the room seemed to be filled with mice, running about in every direction. I did not, however, perceive that they mounted the bed or the bed-furniture. At last I perceived a mouse ascending the wall. In my full

front view was a long bell-pull, hanging nearly from the ceiling to the floor. A mouse (I fancied, larger than the other mice) deliberately climbed to the top, turned himself round, and for some minutes quietly surveyed the room; then deliberately descended; and, in two or three minutes, not a mouse was left in the room. I slept in the same room many weeks after this occurrence, but I never again perceived the sign of a mouse.

I imagine that the mice inhabiting the house had perceived that this room was now partially inhabited, and that they suspected that it would probably contain something interesting to them; that, acting under a general, or chief engineer, they directed the whole strength of their tribe to work an entrance into the room; that their chief engineer, as soon as an entrance was gained, proceeded to examine the contents of the captured fortress; and that, thoroughly disappointed, he gave the signal for retreat, which the whole body of mice instantly obeyed.

September 10 A. B. G.

"Cholera and Copper"

WITH reference to the letter on the above subject in this week's NATURE, it is quite true that the last visitation of cholera was especially severe here, yet in no single instance was a worker in the copper works of the neighbourhood attacked. It is the common boast of the copper-men that, although they lost many members of their family, living in the same house, by the dread disease, yet neither in the last visitation nor the previous ones was there a copper-man, *i.e.* a man working at a furnace, attacked.

There is no doubt that these men take large quantities of copper sulphate into their systems, for not only do they breathe the fine dust of regulus always floating about, but they handle their food with unwashed hands, or, if washed, not washed clean, and that their hands are covered with soluble copper salts is evidenced by their action on the iron tools which they handle, these quickly receiving a deposit of metallic copper.

This seems to be pretty fair evidence that copper is a preventive for cholera.

I may point out that it is not copper or any of its compounds which injures the vegetation, but sulphur dioxide, the principal gas evolved in smelting copper ores, and which goes by the name of "copper smoke." W. TERRILL

Ffynone Club, Swansea, September 8

Antiquities saved by Protective Resemblance

A LARGE number of pillar stones marked with crosses, early Christian inscriptions and oghams, have been destroyed in Britain by farmers during the present century; a still greater number must have been destroyed before these objects began to attract special attention. A great number of the still remaining examples have been utilised as gate-posts and rubbing-stones for cattle, *i.e.* upright stones set up in fields by Welsh farmers for cattle to rub their itching skins against. This fortuitous resemblance of the slightly squared inscribed stones has protected them from destruction. A few of the flatter examples have been utilised as bridges over narrow streams. Nearly all the examples which have not resembled the above-mentioned objects have met with destruction. It is a sort of survival of the fittest.

In Wales there are many ruined churches and monastic establishments with interiors gutted. Most of the old stone altar-slabs have so closely resembled doorsteps, that they have been saved. It is no uncommon thing to see an altar-slab with its five little crosses utilised as a doorstep to a cottage near a deserted church.

The bowl of a font often bears a sufficiently strong resemblance to a pig-trough to insure its preservation, and if the font is not visible in a ruined church the strong probability is it will be found utilised as a pig-trough in some neighbouring farmyard. Fonts with shallow bowls are specially preserved.

Stone coffins sometimes owe their preservation to their resemblance to and suitability for horse-troughs.

In some instances old churches are now used as barns, and in others as residences for farmers or farmers' men; sometimes a wooden floor has been erected across an old church and the upper part used as a store for hay, and the altar end as a pantry. I have seen the recess of the piscina furnished with a wooden door and the interior used as a cool receptacle for butter and lard. A fortuitous resemblance has protected it.

I could write out a large number of examples of the above and other curious instances of "protective resemblance" in antiquities. Indeed the above facts are so well known to antiquaries that, unless very inconvenient, no "rubbing-stone" or stone gate post is left unexamined in a strange district. Doorsteps, flat stones across streams, and stone hog-troughs are always carefully scrutinised by experienced archæologists.

WORTHINGTON G. SMITH

Meteor

A METEOR of surpassing brilliancy made its appearance here at about 4 46 p.m. on July 12. Its form might be described as somewhat rocket-like. It was observed streaming slowly from the west in an easterly direction, at an apparent altitude of about 45 degrees. Some idea of the brilliancy of this phenomenon may be formed when it is mentioned that it was seen in broad daylight, the sun setting on that day at 4.35 p.m. I notice the meteor was observed over a wide extent of country on the Canterbury Plains; it was noticed from Christchurch, and also at Rangiora, to the north. THOMAS H. POTTS

Ohinitahi, New Zealand, July 14

The Meteor of August 19

THE meteor described in your issue of August 23 (p. 389) was well seen here (lat. 1° west, long. 54° 15′ north) and formed a splendid object. It bore a little east of south, and its apparent path was nearly horizontal from west to east, towards and at about the same declination as the full moon. It would be interesting if its height above the earth were approximately ascertained and stated from the various observations made.

The Grange, Nawton, Yorkshire C. D.

HERMANN MÜLLER

THE news of the death of Hermann Müller of Lippstadt will come with a sense of personal loss to many of our readers, who have looked with interest for his frequent contributions to the columns of NATURE on the branch of natural history which he has made specially his own—the mutual relations to one another of insects and flowers in promoting cross-fertilisation. Much as we owe on this subject to some of our own naturalists, especially Darwin and Lubbock, the chief authority in it is, and probably always will be, Hermann Müller. Any future inquirer will necessarily turn, for the main part of his information, to his two great works, "Die Befruchtung der Blumen durch Insekten," published in 1873, and "Alpenblumen, ihre Befruchtung durch Insekten," published in 1881. The mass of information contained in these volumes is simply marvellous. In the first place the author has worked out with the greatest care the structure of those classes of insects which play the greatest part in the fertilisation of flowers with regard to their capacity for collecting nectar or pollen, and for carrying pollen from flower to flower. A very large proportion, including all the commoner ones, of the species which make up the phanerogamic flora of Central Europe are then taken up *seriatim*, the structure of the male and female organs described, illustrated often with very careful drawings, and always with reference to any special contrivances connected with the mode in which insects obtain the honey; and then a list is given of all the insects which he has observed visiting the flower. No one who has worked in the same field will fail to recognise the unfailing trustworthiness and accuracy of his observations. The "Befruchtung der Blumen" has only during the present year been presented to English readers in Mr. D'Arcy Thompson's translation, with an appreciative preface by the late Mr. C. Darwin, a notice of which will shortly appear in our columns. But these two works by no means exhaust Prof. Müller's labours in his favourite subject, as his numerous contributions to our columns show. He was also a frequent contributor to the German periodical *Kosmos*, discussing, with great wealth of knowledge and acute reasoning, the

origin of species, the genesis of the colours of flowers, the laws of variation, and other similar subjects. Dr. Müller's contribution, "Blumen und Insekten," to Schenk's "Handbuch der Botanik," which forms a part of the "Encyklopædie der Naturwissenschaften," now in course of publication, is an admirable *résumé* of the whole subject.

Dr. Müller died in harness, having fallen a victim to an attack of inflammation of the lungs at Prad, in Tyrol, on August 25. A. W. B.

SECOND NOTE ON THE ELECTRICAL RESISTANCE OF THE HUMAN BODY

THE fact that the note on this subject inserted in NATURE, June 14, p. 151, was copied *in extenso* by the *Electrical Review*, by the *New York Electrical World*, and I believe by some other papers, as well as the fragmentary way in which these observations must of necessity be obtained, encourages me to ask for a little further space. This is the more pardonable as the writer in the former paper, in two editorial articles which he founds on my observations, shows ignorance and misconception of certain physiological facts involved in them—a misconception the correction of which by myself he does not think fit to publish.

On August 23, during my visit to the ward, it became obvious that a hopeless and incurable case of renal disease was rapidly sinking. It occurred to me that the patient, being in a state of uræmic drowsiness almost amounting to coma, there would be no inhumanity in adding small electrical currents to the other stimulants which as a last chance we were sedulously administering. I accordingly immersed his feet, which were rapidly getting cold, in hot baths of salt and water connected with Wheatstone's bridge. This and the brandy caused a decided rally, and the temperature became normal, viz. 98° F. The resistance then taken was 1100 ohms from one foot to the other. At 3 p.m., however, he rather suddenly relapsed, his hands and nose becoming cold. The following series of observations was taken :—

2.55	p.m.—temperature 98°	1100
3.0	,, ...	900
3.5	,, ...	870
3.7	,, ...	850
3.12	,, ...	840
3.13	,, ...	820
3.22	,, temperature 95°	800

We concluded that death was imminent, and I ceased the experiment, intending to renew it after the event. But on returning to the ward at 4.36 I found him somewhat better and warmer. I applied the large leaden poles, to which I will refer presently, to both feet, so as to reduce the resistance to a minimum. The following remarkable series of resistances was obtained. The thermometer,[1] being found too slow in its action to follow the flickerings of the expiring lamp of life, was not used, the hand applied to the skin being quite competent to detect the great changes of surface heat :—

4.36	p.m.	640
4.40	,, ...	600
4.45	,, ...	570
4.50	,, (rally)	750
4.55	,, (relapse)...	700
5.0	,, (great rally)	770

He was still very cold, but began to ramble in his usual incoherent way (having been slightly deranged for several years), and I therefore left him for the night. On returning next morning early I found he had died an hour and a half later. Had I not been greatly fatigued myself, I should have stopped to secure an observation during post-mortem refrigeration, and before the access of rigor mortis. As it was I found the corpse in full state of rigidity. We managed to have the testing apparatus set up by 12.30, and without any great disturbance of the body I applied the leaden poles.

[1] In the axilla. I hope to use surface thermometers on a future occasion. In my former paper the axillary temperature obviously lags behind that of the extremities.

After some preliminary tests I obtained two excellent observations with reversed currents, and found them both exactly alike at 1150 ohms.

Then came the last experiment with which I now have to trouble you, namely, the question of skin resistance. A tremendous hubbub has been made about this since the time of Duchenne. I believe it has been enormously exaggerated. My anonymous critic of the *Electrical Review* quaintly says : "We *most of us*" (*sic*) "know the effect of keeping the feet in salt and water, or water alone" (he does not name soap and water !) "for any length of time. The skin turns white and swells, *enlarging the pores* (*sic*) ; indeed nearly the whole of the outside skin is of a spongy nature." I need not prolong the quotation, because I simply deny his facts, except where foot-washings have been "like angels' visits," &c., &c. The carefully-prepared epidermis of my patients is entirely free from this hypothetical and inaccurately stated cause of error. So I hope is mine ; indeed I feel the full value of the implied limitation of the cautious phrase "most of us." Seriously speaking, it is too bad that an observer of average capacity, and I hope moderate honesty, should be accused of such elementary blunders on mere *a priori* grounds. Now for fact : Before going to the deadhouse I had provided myself with two silver needles, used for the electro-puncture of aneurisms, and intended to convey a very strong coagulating current from a powerful battery. I inserted one of these to the depth of three inches into the plantar muscles of each foot of the corpse, leaving everything else untouched. I expected the enormous reduction of resistance above named. To my surprise the Wheatstone bridge gave 1200 ohms in either direction of current, or 50 more than with large lead and salt-water electrodes. This alleged skin resistance is then only true in the dry state, and is easily conquered by very simple means. Cases of diabetes have been cited in confirmation of this supposed resistance, and it has been explained by the peculiar dryness of the skin in this complaint. A patient now in my ward, though a tall emaciated man with long spindle shanks, only gives 1340 ohms from foot to foot, with either salt-water baths, or with the lead electrodes as here described. This is rather under than over the average.

One word as to the lead electrodes themselves, and the manner of using them. The intelligent and kind lady nurses of our hospital, whom I like to call by their grand old name of "sisters," and who throughout this inquiry have seconded me in the most self-sacrificing way, are instructed to get ready certain patients for me each morning. The process consists in wrapping both hands and feet in coarse flannel saturated with strong warm brine for an hour before the experiment. Sometimes the process so graphically described by my commentator occurs, and is dealt with accordingly. I then proceed to wrap the members one by one in a surgical covering of flannel soaked in the same conducting solution. Over this I fold, also in surgical fashion, a strip of thin sheet lead about eighteen inches long, and one and a half inches broad. On the top of all is an ordinary spiral bandage, which moulds the whole to the shape of the limb, and squeezes out superfluous fluid. An indiarubber covered wire leads to my testing table. I may add that each hand or foot is separately deposited on one of the vulcanised rubber waterproof sheets commonly used in the wards, and which I find to be excellent insulators. The first few observations are commonly rejected ; always if they show any suspicion of diminishing. But after even half an hour's maceration this is rarely the case. Between every two observations I put the patient himself on short circuit, to discharge any currents of polarisation

Every measurement is at least double, and made with currents in opposite directions.

In conclusion I may remark first, that this, like my former note, only deals with part of a larger inquiry; and secondly, that the results above stated were open to all comers, and were carefully watched by Dr. Percy Smith. Dr. Shepherd, and others of my colleagues and pupils.

W. H. STONE

THE INTERNATIONAL BUREAU OF WEIGHTS AND MEASURES[1]

AS the result of an International Convention held on the 20th May, 1875, an International Bureau of Weights and Measures has been created at Paris with the object of securing an international metric system, and which should take account (1) of all comparisons and verifications of the new prototypes of the metre and the kilogramme; (2) of the conservation of the international prototypes; (3) of periodical comparisons of national standards with the international prototypes, as also of comparisons of thermometric standards; (4) of the comparisons of the new prototypes with the fundamental standards of non-metric weights and measures employed in different countries and in science; (5) of the marking and comparison of geodetical measures; (6) of the comparison of standards and scales of precision, the verification of which may be sought by governments, by learned societies, or even by mechanists and students.

An international committee of weights and measures composed of fourteen members, comprising physicists, mathematicians, surveyors, and astronomers, all of different nationalities, has been intrusted with the supreme direction of the bureau. The president of this committee is General Ibañez, director-general of the Geographical and Statistical Institute of Spain, and its secretary Dr. Hirsch, director of the Observatory of Neuchâtel. It meets once a year for the discharge of its functions at Paris.

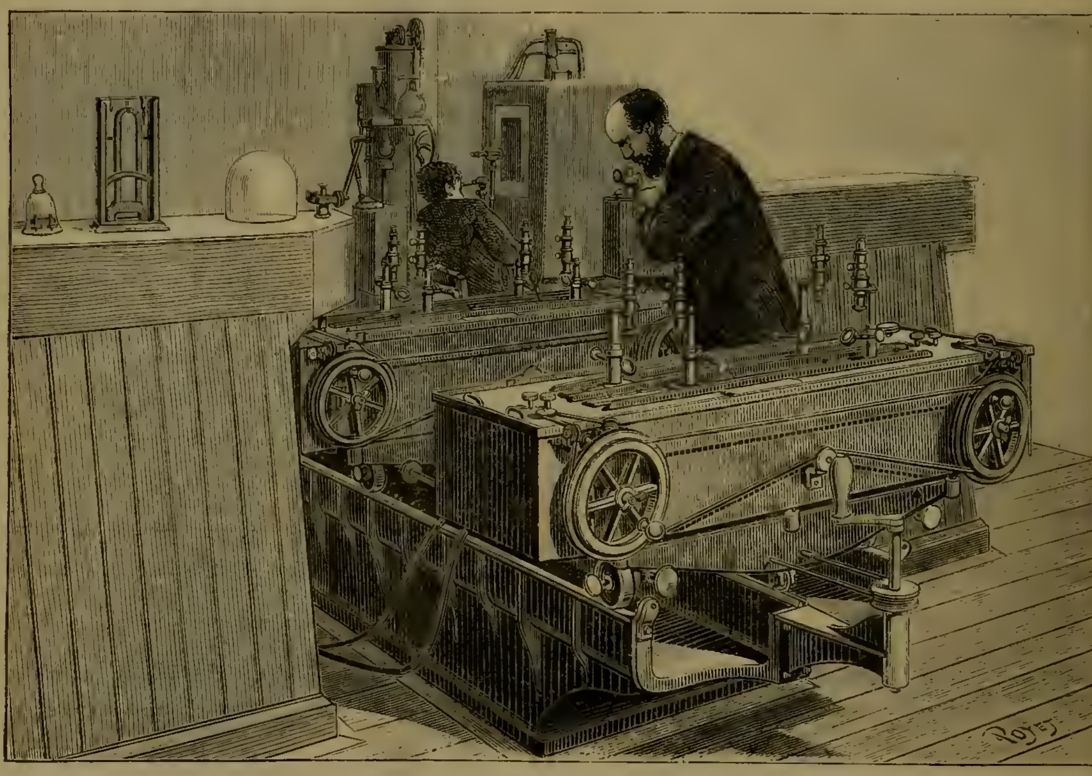


FIG. 1.—"Comparateur" for Measuring Absolute and Relative Dilatations.

Twenty States were represented at the preliminary diplomatic conference of 1875. Of these, seventeen (or nineteen) have signed the international convention which was the result of its deliberations. One alone of these States has not ratified the convention; and consequently the expenses of the foundation and maintenance of the International Bureau of Weights and Measures have hitherto been defrayed by the following sixteen States: Germany, Austria-Hungary, Belgium, the Argentine Confederation, Denmark, Spain, the United States of America, France, Italy, Peru, Portugal, Russia, Sweden, Norway, Switzerland, Turkey, Venezuela. These, representing about 351,000,000 of people, have already contributed over 1,000,000 francs towards the foundation of the International Bureau. The Government of Servia has since given in its adhesion to the convention.

In order to provide for the erection of the necessary structures for the observatory of the International Bureau of Weights and Measures, France made a grant of ground in the park of St. Cloud formerly occupied by the Pavillon de Breteuil, safely removed from all disturbances and surface tremors such as would have been experienced in the centre of a large city amidst the whirl of carriages and the working of machines.

In front of the observatory of the International Bureau are offices set apart for mechanical laboratories. Behind are spacious chambers in which are distributed the various instruments of precision employed in metrological work. These chambers are surrounded by walls of great thickness; they are lighted by skylights so arranged as to prevent solar rays from penetrating: and are environed by a lobby isolating them from the exterior. The object

[1] From *La Nature*.

aimed at by these arrangements is to secure to the utmost degree possible the continuance of a uniform temperature which in certain operations is a condition of success.

The labours of the bureau are naturally divided into two sections; one dealing with standards of length, the other with standards of mass or weight. The first comprises principally the settlement of equations of the various standards, that is of their lengths compared with the prototype which forms the universal basis of departure; the ratio of their expansions; the study of their subdivisions or of their multiples; and particularly of the great rules (*règles*) which serve geodesists for the measurement of terrestrial bases. Similarly, the section of weights is busied with determining the relations of several primary kilogrammes to the kilogramme prototype, with the adjustments of their subdivisions, with the computation of their specific weights, &c. These various labours are divided among a certain number of experts constituting the scientific staff of the bureau. We shall hastily indicate the principal instruments used in both sections, instruments which are the workmanship of the ablest mechanists in the whole of Europe, and which in general show the last limit of perfection attainable in this precise branch of mechanics.

The instruments belonging to the section of linear standards or metres, are called "comparateurs." A comparateur for metres *à traits* is essentially composed of two microscopes solidly fixed and immovable, provided with micrometers under which, by a peculiar mechanism, the two rules it is desired to compare with each other can be successively introduced. The bureau possesses several of these instruments, each adapted to a special purpose, and consequently distinguished by peculiarities of construction.

The first is Brunner's comparateur, designed for comparisons in air of metres *à traits*. The two microscopes are by means of cantilevers riveted on large stone pillars or monoliths resting on massive masonry. The micrometers with which they are furnished follow the arrangements usually observed in the case of astronomical instruments. Each is composed of a kind of little rectangular box, lengthened and flattened, fixed on the body of the microscope below the eyepiece. In this box is a frame capable of being displaced from right to left. On the frame are stretched two spider threads, very fine, parallel and very close to each other, which constitute the parallel spider threads or reticule. The displacement of the frame is effected very slowly by means of a micrometer screw of perfect workmanship, which is worked from the outside by means of an enlarged micrometer-head or drum, the circumference of which is divided into a hundred equal parts. By turning this round the experimenter moves the screw, which in turn moves the frame and displaces the spider threads visible in the field of the microscope. The image of the lines traced on the rule as given by the objective then lies in the plane of these threads. To bisect a line is to make the parallel threads coincide with the image of this line, that is by turning round the drum so as to bring the threads into such a position that the line may appear exactly in the middle of them; the position occupied by the parallel threads will in that case be indicated by the reading of the micrometer-head or drum. Should a second line appear under this microscope in a different position, it will be necessary, in order to bisect it in turn, to displace the threads, that is to make the drum revolve a certain number of divisions. The distance corresponding with one displacement of a division being known, the distance between the first and second lines can then be calculated.

Under the microscopes is the body itself of the comparateur, composed primarily of a strong framework of brass, exceedingly massive and steady, forming on its upper borders a kind of railway on which rolls a heavy carriage, movable at pleasure by a handle which controls a system of cog-wheels. Surmounting this carriage is a long box or metal trough with double walls formed of two cases inclosed one within the other. This box is designed to receive the two rules that are to be compared. These are placed beside each other in the middle of the box on supports of suitable form. The box is provided with various contrivances by means of which the observer, while observing the microscopes, is able to adjust the rules; to lower them or to raise them; to bring them into focus at the two extremities, or to displace them longitudinally or transversely, as may be required. The box is able, moreover, to receive a sufficient number of thermometers, which are observed with the aid of special lenses fitted in the lid which covers the whole and which

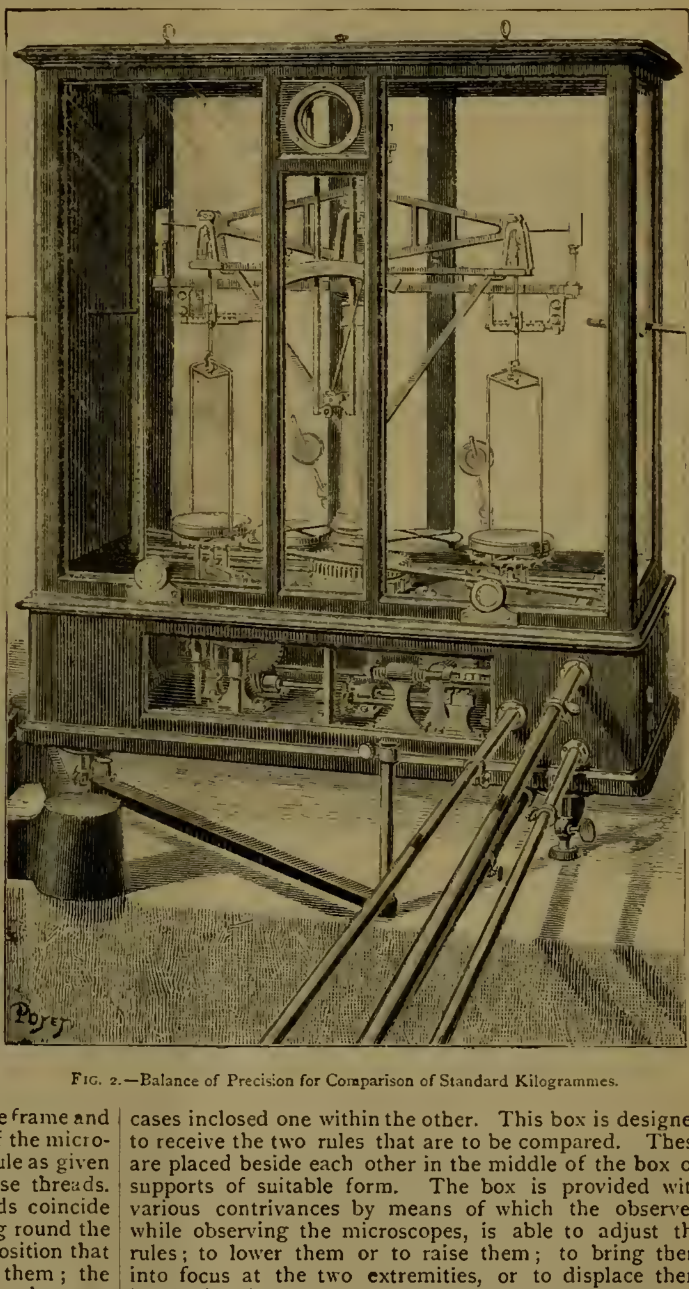


FIG. 2.—Balance of Precision for Comparison of Standard Kilogrammes.

protects the interior of the apparatus from every rapid variation of temperature. By the movement of the carriage the observer brings successively under the microscopes, the two metres the difference between which he wants to ascertain ; he bisects the lines of both, and this operation made at the two extremities furnishes the equation desired between the two rules.

Another of the comparateurs represented in the accompanying diagram (Fig. 1) is designed for the measurement of expansions. As in the preceding comparateur we here also find two microscopes with fixed micrometers and a carriage moving on rails. Here, however, the carriage contains two distinct boxes or troughs ; each at a distance of about one metre from the other. The two rules to be compared are placed one in each of these troughs. They are thus to some extent independent of each other, and may therefore be introduced at different temperatures. To measure the expansion of one rule, you place it in one of the troughs, and in the other trough the other rule called "*de comparaison*." This latter, so long as the process of determination lasts, is maintained at an invariable temperature, while the other is alternately cooled and heated throughout the series of consecutive experiments between sufficiently extended limits. The latter rule then alternately contracts and expands, and in the case of each particular experiment you compare the length the rule has reached at the temperature to which it is then subjected, with the constant length of the "*de comparaison*" rule. One of the great difficulties connected with these measurements is the maintenance for a sufficiently long time of an exactly uniform temperature, particularly when this temperature is notably at variance with the surrounding temperature. To secure this requirement the rules to be compared are immersed in a liquid, and this liquid is heated by means of a constant circulation of water within the double walls of the trough. Indiarubber pipes, as may be seen in the diagram (Fig. 1), are used for this purpose. The water is supplied from a large metal reservoir outside the chamber, being heated by a regulating system that causes it to issue at an invariable temperature. Thence by pipes it is conveyed to the comparateur, traverses the trough in a continuous stream, and is then carried off by waste pipes, conveniently arranged, into a drain. By this means a constant thermal state is maintained, within a few hundredths of a degree, at any point up to forty degrees, for whole hours at a time.

The diagram (Fig. 1) indicates the principal details of the mechanism employed. In front is seen the handle which by means of an endless cord draws away the carriage and allows the rapid substitution of one trough in place of the other under the microscopes. On the sides, the long rods provided with buttons, which the observer finds always under his hand whatever position he may occupy round the instrument, have the power of acting equally on the carriage by means of a cog-wheel placed under it, and of moving it backwards and forwards by a uniform slow movement. On the lids are perceived the heads of the different keys which enable rectifications of all adjustments to be made, as also the lenses by means of which the thermometers are read. The fly-wheels placed in front of the troughs serve by means of cords and pulleys to convey a movement of rapid rotation to the agitators which are placed in the trough, and thus vigorously intermingle the strata of the liquid, and secure uniformity of temperature in all parts of the bath.

With these apparatuses the difference can be determined between two metres at a given temperature with an exactness reaching to some ten-thousandths of a millimetre. In order to obtain such nicety, it is of course necessary that the lines of the metres be traced with sufficient fineness and distinctness to fit them for the magnifying power employed.

The two instruments just mentioned are fitted for the comparison of metres alone. The *comparateur universel*, on the other hand, allows comparisons to be made of any lengths whatever from below a metre up to two metres. The aspect of this new comparateur is entirely different from that of the two preceding. The microscopes which in all cases constitute the essential parts, instead of being fixed, are here mounted on carriages, which can be moved over a kind of bridge placed horizontally between two stone pillars. This bridge is formed by a large block of brass furnished with steel surfaces on its upper edges, which serve as a support and guide to the microscopes in their movements. It is perfectly rectilineal and horizontal When, by moving the carriages, the microscopes have been brought into the position they require to occupy for a given work, they are fixed by tightening a lever with the aid of a knob which controls a screw. Below, as in the preceding comparateurs, is a massive carriage likewise bearing supports on which are arranged the rules needing to be examined. These supports are equally furnished with all the necessary means of adjustment. These latter, again, are worked by a mechanism too complicated to allow so much as an idea of it to be communicated without the help of diagrams. The comparateur contains, besides, a standard rule of two metres, divided along its whole length into centimetres, two supplementary microscopes mounted on a special carriage and designed for marking the subdivision of a metre, various accessory pieces capable of serving for comparison of measures *à bouts*, either one with another or with measures *à traits*,[1] &c. It is entirely inclosed in a large mahogany box. This box is furnished with windows necessary for lighting the various parts, and with the orifices required for the transmission of movements to the interior, &c., and has the appearance of an imposing and elegant piece of furniture.

We have still to mention a comparateur for metres *à bouts* by Steinheil's method; and to add that this beautiful collection will in the course of a few months be completed by the introduction of a geodetical comparateur for rules of four metres, which is actually in process of construction, and the object of which is indicated by its name.

(*To be continued.*)

THE VIENNA INTERNATIONAL ELECTRIC EXHIBITION

FOR two weeks, the arrangement of the machinery being nearly complete, the Exhibition has been open in the evening from 7 till 11. The effect of the illumination of the immense interior of the Rotunda and its annexes by the various incandescent and arc lamps, and of the surrounding places which are lighted by large reflectors, is brilliant. The electric railway of Siemens and Halske between the Rotunda and the Praterstern is already in operation. The theatrical performances at the "Asphaleia" Theatre, which is lighted by 1500 Swan lamps fed by a large Zipernowsky alternating current machine, have also begun this week.

The series of lectures to be held at the theatre during the Exhibition was inaugurated on August 27 by Sir C. W. Siemens, with a lecture "On the Temperature, Light, and Total Radiating Power of the Sun." After a short introductory sketch of the nature of the terrestrial sources of light, the lecturer gave an account of the ratio of the three forms of radiant energy, viz. heat, light, and actinism, as produced by the sun and terrestrial light sources. Then referring to the difference between the statements of various astronomers and physicists relating to solar temperature, he expressed his opinion that the temperature of the sun could not exceed 3000° C., and explained the experimental methods he used for measuring the solar temperature. The second lecture was delivered on Sep-

[1] A metre or other measure *à bouts* is one whose ends exactly coincide with the ends of the material of which it is made ; a measure *à traits* is bounded by lines within the margins of the material on which it is traced.

tember 1, by Dr. Aron of Berlin, "On the Telephone and Microphone." In this lecture the principles were explained on which the construction of the different telephones and microphones is based. There were also mentioned the variations of timbre as produced by these instruments; according to the experiments of Helmholtz the higher tones are transmitted better by the telephone, and therefore the timbre becomes clearer, while by the simple microphone, as Dr. Aron had found, the deeper tones are better transmitted, causing a duller timbre, but this failure is avoidable by using microphones with two coils. The lecturer explained also the principle of a new instrument, invented by himself, called the semaphone. In this instrument the variations of the current in a coil of insulated wire are transmitted by induction to another coil joined to a telephone or microphone. Dr. Aron has made experiments with his semaphone at Berlin, and was able to hear signals, the distance between the two coils being 70 feet. A similar experiment was carried out by Dr. Aron in the course of his lecture, and we could hear the noise made by a Neef's interrupter far from the lecture room, using a Siemens' telephone; the distance between the two coils being 3 feet.

Electric lighting is very well represented at the Exhibition, and a variety of new incandescent and arc lamps is to be seen there. As to the number of lamps exhibited, the first place is taken by the Swan lamps. Nearly 2000 Swan lamps are distributed at the theatre, the splendidly furnished interiors, and other parts of the building, fed by dynamo machines or by Faure-Sellon-Volckmar accumulators. The durability of these lamps is tested by a collection of lamps exhibited by Ganz and Co., used 1720 to 2330 hours. The carbon filaments do not show any damage, only the glass bulb being darkened by a carbon deposit. The exhibition of Edison lamps is not so extensive as it was at previous exhibitions. The Maxim lamps are used for lighting the Oriental pavilion and some of the interiors. The Lane-Fox lamps are also lighting some furnished apartments, and show the applicability of incandescent lamps for street-lighting by lighting the "Ausstellungstrasse." The lamp of C. H. R. Müller has a screw-like curled carbon filament to make the emission of rays uniform in all directions. The U-shaped carbon strap of the Greiner and Friedrich's incandescent lamp is prepared from lamp-black and graphite, coal-tar being used as cement. The coal-tar, at first treated with sulphuric acid, is heated till it becomes an asphalte-like mass, to which lamp-black and graphite are then added, so that a stout paste is formed. By pressing this paste through a little fine hole a thin thread is obtained, which is cut in pieces and dried. If dried, the U-shaped pieces are burned. The carbonised fibres of *Musa textilis* are used for the incandescent lamps of Dr. Puluj. Very interesting is the Bernstein lamp, exhibited by the Bernstein Electric Light Manufacturing Company of Boston. It is claimed by the inventor to have many advantages over the other incandescent lamps. With an electromotive force of 23 volts and a current of 7 amperes, it has an illuminating power of 65 candles; it is stated to be more durable than the other lamps, and more economical, by rendering the light-giving carbon able to expand and contract without being liable to injury and breakage, and therefore capable of withstanding the action of strong currents, so as to avoid the disintegration which takes place in carbon filaments of high resistance. A large number of lamps can be used in series, and long distances can be lighted by means of a thin wire; the lamp is very appropriate for street-lighting. A hollow U-shaped carbon cylinder as big as a lucifer match is used as the light-giving part, having a comparatively large illuminating surface. This carbon cylinder is quite elastic, and its surface resembles knitwork. Though the manufacturing process of the carbon is not yet published, it seems to be very probable that the carbon cylinder is prepared by carbonising a hollow knitted or woven string, a metallic wire being put through during the burning process to support it. The ends of the U-shaped cylinder are connected with pear-like socket pieces of carbon, to which the two conducting wires are attached, entering the thin end of the carbon blocks, secured by means of a reddish cement. Such a lamp, fed by sixteen Faure-Volckmar accumulators, gave, as could be seen at the lecture delivered by Sir William Siemens, a white, dazzling light resembling an arc lamp.

Vienna, September 10

THE EDINBURGH BIOLOGICAL STATION

THE proposal to form a biological station at Granton, which was some time ago brought before the Royal Society in a paper by Mr. Murray of the *Challenger* Commission, has now taken definite shape. A lease of Granton Quarry for fifteen years has just been granted by the Duke of Buccleuch at a nominal rent, and Mr. Alexander Turbyne, salmon fisher, has been appointed keeper of the station, and will enter on his duties next week. Meantime some preliminary experiments have been made, and cages have been put down at the station, and structural work has been commenced in the way of fencing, building of walls, and putting the banks into proper order for further operations.

The proposal for the formation of the station, which it is meant to call "The Edinburgh Marine Station for Scientific Research," had its origin in the resolution of the Committee of the late Fisheries Exhibition in Edinburgh to hand over the surplus funds derived from the Exhibition to the Meteorological Society, to be applied to the purpose of carrying on investigations with respect to fish, with a recommendation to establish a zoological station, and to apply to Government for assistance in the work. The Meteorological Society appointed a sub-committee to consider the best means of applying this money to the purposes for which it was granted. This Committee had many consultations, and set afoot investigations at various ports as to the temperature of the water, habits and food of the fish, &c. They also had their attention carefully directed to the advisability of establishing a zoological station; and the suitableness of the old quarry at Granton for the purpose has been in various ways brought before the public, both at the Royal Society and at the meetings of the Meteorological Society. The scheme for founding a station there first took definite shape on the offer of a gentleman interested in research to build a floating laboratory at the quarry for the purpose of making experiments and investigations. Recently this gentleman was again communicated with, in respect that, after full consideration, it was thought that a floating laboratory, although an essential part of the scheme, was not, perhaps, the first that should be undertaken. In reply to a representation to this effect, the gentleman has written to Mr. Murray, the convener of the Station Committee, expressing his readiness to adopt the alterations proposed, and to give the 1000*l*. for the purpose of founding a zoological station for scientific research at Edinburgh, instead of building a floating laboratory, as originally suggested. He was not surprised to hear, he adds in his letter, that it would cost more than that to carry out the whole of the scheme. It seemed to him that they would require at least 1500*l*., in addition to his 1000*l*., to carry out all their proposals, and they should consider if this additional sum should not be raised before they commenced operations. However, he left the matter in the convener's hands to apply the money as he thought best, inclosing 100*l*. to cover preliminary expenses, and repeating the two conditions of his donation, viz. (1) that the convener should take the general direction of the station for at least three or four years; and (2) that his name was not in the mean-

time to be made public. The resolution which has now been come to is to go on with the undertaking, and the scheme is of a twofold character: (1) to undertake a scientific exploration and description of the Firth of Forth and the adjacent parts of the North Sea; and (2) to establish a marine station for biological investigation and research, where competent scientific men may find laboratories and all the appliances for research free of charge. With respect to the first branch of the scheme, it is meant to take the temperature of the water at fixed points of the Firth, extending from the fresh water of the Firth out to points beyond the Isle of May. The temperatures of the surface water and of the bottom and intermediate waters are to be taken at stated intervals throughout the year. It is part of the same scheme to note the character of the surface fauna and flora regularly throughout the year at these points, and the changes in the specific gravity of the water at the different times of the year and at different parts of the Firth. Observations will also be carefully laid down on the Admiralty charts of the nature of the bottom, and of the deposits, throughout the whole region, and a record of the animals living upon these is also to be attempted, so as to arrive at a complete scientific description of the bottom and its deposits. To this will be added a record of the effects upon the fauna, &c., of the admixture of river and ocean water at different parts, and of circumstances favourable or inimical to life and growth. Under the second branch of the scheme the proposal is to establish at Granton Quarry, and at various places in the Firth, investigations as to the hatching, breeding, and growth of various kinds of fish and marine invertebrates in inclosed spaces, or in cages moored at various points. The central station will be situated at the quarry. Here it is proposed to build, on a high part of the banks surrounding the quarry, a substantial cottage, from which a beautiful view of the whole Firth will be had. The cottage is to be fitted up with laboratories, and will consist of about six rooms, and cost from 400*l.* to 500*l.* On a level piece of ground adjoining the quarry there will be erected an iron cottage and shed for the keeper of the station, and for housing the trawls, dredges, nets, and other instruments required for the proposed investigations. This will cost from 150*l.* to 200*l.* Also, as part of the scheme, there is to be built a floating laboratory—that is to say, a laboratory built on a barge of the description mentioned to the Royal Society, and supplied with all the materials and apparatus requisite for biological investigation. This structure, it is interesting to note, will be so fashioned that it may be taken to any part of the Firth of Forth, and moored in sheltered spots during the summer wherever it may be thought desirable that investigations shall be carried on at any particular spot. This laboratory, it is intended, will give accommodation for three naturalists, with workrooms, and will cost about 800*l.* The station, furthermore, is to be provided with a steam launch fitted for dredging purposes and the making of hydrographic observations. The launch, according to the design, is to be built upon the plan of the steam pinnace that accompanied the *Challenger* during her cruise, but much larger, and will be provided with a separate engine for rolling in the dredges. This again will cost about 800*l.* In addition to these things there will be a small portable house belonging to the station, which may be put up on Inchkeith, Inchmickery, Inchcolm, or the Isle of May, should it be desirable to carry on any observations at these places. This, together with the cages formerly described for inclosing portions of the ocean and water of the quarry, will cost, it is estimated, about 300*l.* more. The fund which was granted by the Committee of the Fisheries Exhibition is to be applied, at the rate of 300*l.* a year for three or four years, to the keeping up of the station and the payment of the annual working expenses, including the salaries of a resident naturalist, an engineer, and a keeper. So that what is now wanted in order to the full equipment of the station is about 1500*l.* to pay for the permanent works which are required before the station can be in complete working order. In the event of this sum being forthcoming at an early date, it is thought that the whole institution would be in working order next spring—probably by March or April.

It is believed by a number of our scientific men that an undertaking of this kind, which will afford the means of making continuous observations into the circumstances which affect marine animals and plants—their food and their enemies—is the true method of getting the information necessary to settle many of the vexed questions with respect to the life histories of our food fishes, both of the salmon and sea fishes. The Firth of Forth yields special facilities for work of this kind. Thus, almost all our food fishes are frequenters of the Firth, and it is known to have a rich fauna, which has at various times been investigated by distinguished naturalists, as by Johnston of Berwick; Parnell, Allman, Forbes, Herdman, and others. A thorough investigation of the kind proposed will lead to great additions to knowledge, and will probably give the information that was wanted as to the evil effects or otherwise of trawling, which is one of the vexed questions at the present day. By directing their efforts to the thorough working out of a somewhat limited area like the Firth of Forth, in its meteorological, hydrographical, and biological aspects, the Committee believe that more rapid progress will be made than by intermittent observations at widely separate points. Such a station will also be a great boon to naturalists who desire to work at any special subject. Naturalists are often deterred from undertaking investigations because of the difficulty of providing themselves with dredges, steam-launch assistance, &c. Here they will have these ready at hand whenever they choose to visit the station. So that, from this point of view, in addition to the purely scientific aspect of an undertaking of this kind, it probably will be found to have a very wide economic bearing. The plans of the floating laboratory and of the other structures to which reference has been made are in the hands of Mr. Murray, from whom persons interested or desirous to aid in the carrying out of the scheme will receive every information they may wish to have.

NOTES

THE Directors of the Ben Nevis Observatory met on Thursday, 6th inst., and out of a list of nineteen applicants elected Mr. R. T. Omond, Edinburgh, Superintendent of the Observatory. Mr. Omond was a distinguished student of Edinburgh University, and for the past six or seven years has been chief assistant of Prof. Tait in conducting an extended series of physical experiments on the influence of pressure on deep-sea thermometers, the maximum density of water under different pressures, and cognate subjects of inquiry. The results of his work have been communicated in the form of papers to the Royal Society of Edinburgh. Mr. Omond's duties began from the above date; and shortly two assistants will be appointed, so that in October next a staff of three observers will have taken up their station at the Observatory, prepared to enter upon the work of the coming winter. The highest section of the bridle-road to the summit of the Ben was finished on Thursday at noon, and the first pair of horses which ever ascended the mountain made the ascent in the afternoon, carrying 2 cwt. each of building material. The building of the permanent Observatory commenced on the following day. A number of horses are employed carrying up material, and the Observatory is expected to be finished early next month. Arrangements are also being made for laying a telegraphic cable from Fort William

to the Observatory, and it is fully expected that the work will be finished by the time the observers take up their residence on the Ben. We understand that the directors have asked Mr. Buchan, secretary of the Scottish Meteorological Society, to visit several of the more important meteorological observatories on the Continent, beginning with that of Hamburg, and including some of the more notable high-level stations, and report on the automatic and other instruments in use there, with a view to a full and satisfactory equipment of the Ben Nevis Observatory next summer. During the coming winter the work will be mostly restricted to eye observations, with the object of collecting information regarding the climate of the Ben, so as to form some guide to the directors in determining the nature of the automatic and other instruments that will be required for making the various observations and conducting the important physical researches which it is proposed to carry out.

THE Lords of the Committee of Council on Education have been informed by the Secretary of State for Foreign Affairs that a note has been received at the Foreign Office from the French Chargé d'Affaires in England stating that the meeting of the Electrical Units Conference at Paris has been postponed till April 2, 1884.

THE president of the American Association at the Minneapolis meeting was Prof. C. A. Young, and as retiring president at the Philadelphia meeting next year it will fall to his lot to give the presidential address. We have already given Principal Dawson's presidential address, and this week we give the address in Section A of Prof. W. A. Rogers on the German survey of the northern heavens. Other important addresses are those of Prof. Rowland, who spoke eloquently on behalf of pure science, Prof. Cope on the evidence of evolution in the history of the extinct mammalia, and Prof. Hitchcock on the early history of the North American Continent. Dr. Folwell, president of the University of Minnesota, pointed out in his address of welcome some of the great triumphs of science in its application to practical purposes: "the further extension of scientific method," he said, "till it shall become the guide of conduct in the everyday life of all men, is now the chief problem in education." So far as reports have reached us, no paper of striking importance was read at the Minneapolis meeting.

THE members of the Swedish Meteorological Expedition at Spitzbergen arrived in Gothenburg on the 6th inst.

AT the general meeting of Tweed Commissioners, last week, it was agreed that 20*l.* should be voted for recommencing investigations regarding the life-history of the various Salmonidæ which frequent the River Tweed. Were similar investigations carried on by the River Conservators in England and by Fishery Boards in Scotland and Ireland, there can be no doubt that information would soon be obtained on many points which are now obscure.

IN the *Comptes Rendus* for September 3 M. Milne-Edwards announces the return to France of the *Talisman*, which had sailed last June to explore the waters of the Atlantic. The expedition has examined the marine fauna along the seaboard of Morocco and the Western Sahara, as well as the waters of the Cape Verde, Canary, and Azores Archipelagoes.

MR. A. HASTINGS WHITE sends us a letter from an Australian correspondent, deploring the wholesale destruction of forests, especially in New South Wales. The correspondent writes:—"I do not know if I have ever mentioned anything about the more than wholesale destruction of the timber going on out here at the present time; but the facts are these. It is a common belief that killing off the timber improves the pastures, and so it does no doubt for a time, but at what a terrible cost. Thousands of acres are killed every year, not even a bush or seedling of timber being left to grow, by cutting a ring round the trees, either into the wood or else by taking a ring of the bark off. The destruction of timber in this way on Crown lands is something terrible; in parts of the country one may travel for miles at a stretch and see nothing but bleached and dead trees, as if a blight had come over the land."

THE Danish ship *Ceres*, having just arrived in Copenhagen from Julianhaab in Greenland, reports that the *Sophia*, with Nordenskjöld's expedition on board, arrived at that place on June 17, having encountered no ice between Iceland and Greenland. After two days' stay there the *Sophia* proceeded to the cryolite quarries at Ivigtuk, where she took in coals. On June 26 the journey was continued to North Greenland. All was well on board.

ADMIRAL MOUCHEZ has asked for the credit required for the publication of the catalogue of stars established by the Paris Observatory for the last twenty-seven years. The number of stars tabulated amounts to 40,000, but the expenses are so heavy that it is doubted whether the required credit will be granted by the Government.

AN International Forestry Exhibition is to be held next year in Edinburgh.

THE International Medical Congress met last week in Amsterdam. The attendance was very large, delegates having arrived from almost every civilised country on the globe. Amongst the representatives of England were Sir Joseph Fayrer and Professors Lewis and De Chaumont of Netley. The Congress was opened by Prof. Stockvis of Amsterdam University, and the Burgomaster of Amsterdam, who welcomed the Congress on its assembly in the Dutch capital. Amongst the honorary presidents of the Congress are Sir Joseph Fayrer, Professors Lewis and De Chaumont, and Dr. Sydney Jones of New South Wales. The inaugural address was delivered by Prof. Stockvis, after which the Congress proceeded to its more special work under different sections.

AN International Society of Electricians has been formed in Paris under the presidency of the Minister of Posts and Telegraphs, its main object being to centralise all information bearing on the progress of electricity, and to promote its spread and development. Information as to the society may be obtained by writing to M. Georges Berger, 99, rue de Grenelle, Paris.

ON Wednesday last week an electric tramcar trial was successfully accomplished in Paris by the French Electrical Power Storage Company. At three o'clock p.m. the vehicle, an ordinary three-horse tramcar, left the Place de la Nation in the far east, and, after traversing the capital through several important thoroughfares, reached the starting point soon after six o'clock. A distance of thirty English miles was thus made in about three hours. There was not the slightest accident. The ease with which the car was turned off one set of tram lines and got on to another across several yards of unmetaled ground is stated to have been admirable. The locomotion is effected by means of Faure accumulators, weighing some fifty hundredweight, which are fixed under the tramcar seats and connected with a Siemens' machine placed under the floor. The machine, which makes twelve hundred revolutions a minute, sets in movement, by means of a pulley, an axle to which are connected the chains which give impulse to the wheels. These wheels revolve sixty times to twelve thousand revolutions of the machine. The speed of the electric tramcar is nine and a third miles an hour on level ground, and five and a half miles on an ascent. The present tram lines are not well adapted for the new locomotion. On the newer lines the movement was sufficiently smooth, but on those that have been laid for some time there was a marked difference, and the actual working force was considerably lower than the indicated horse-power. The estimated cost is one-half that of horse trams.

It is proposed to establish a permanent meteorological observatory for the Bristol Channel. Mr. E. J. Lowe, who for the last forty years has carried on a regular series of meteorological observations at Highfield, near Nottingham, has recently purchased the Shirenewton estate, near Chepstow; and, being convinced of the real importance of establishing an observatory which may be carried on through future years without interruption, he has generously offered to present the whole of his valuable collection of meteorological instruments, together with his books and papers, towards the establishment of such a permanent observatory, for which he also offers to give the site, together with such stone and lime as may be required for the erection of the necessary buildings, provided a sufficient sum can be raised in the district to build the same, and to provide a small endowment towards the maintenance of a limited staff of assistants, who would, in the first instance, be under his gratuitous guidance and supervision. Previous to making this offer publicly known, Mr. Lowe conferred with the Meteorological Department of the Treasury, by whom Mr. Scott, F.R.S. (the director of the Department), was sent down, and his report was in every way most favourable, both as to the great utility and importance of the scheme, and also as to the admirable site which Mr. Lowe proposed to offer.

Prof. Brown Goode, the Commissioner of the United States to the International Fisheries Exhibition, has just received a telegram from Prof. Baird, the United States Commissioner of Fish and Fisheries, to the effect that Mr. Ryder, the embryologist of the Fish Commission, has finally solved the problem of the culture of oysters from artificially impregnated eggs, and that on the 4th inst., at the Government station at Stockton, Maryland, there were many millions of young oysters three quarters of an inch in diameter which had been hatched from eggs artificially impregnated forty-six days before. It may be added that oysters were artificially impregnated in America by Dr. Brooks, of Baltimore, in 1879, but the difficulty hitherto met with in hatching them has been to prevent the young oysters from escaping and being lost immediately after they are hatched, since the spat passes through the meshes of most finely-woven fabrics, such as flannel.

We have before us No. 15, Part II., of the "Encyclopædia of Physical Sciences" (from the publishing house of Eduard, Trewendt, Breslau), which closes Wittstein's Alphabetical Manual of the Pharmaceutical Technology of Botany. As a conclusion to the work are appended three tables: (1) of the German and other popular names of drugs; (2) of the officinal Latin names; (3) of the systematic Latin names of mother-plants. The 16th number contains the continuation of the Alphabetical Manual of Chemistry published by Ladenburg, and among other things gives a very comprehensive and concise work by Tollens on "Analysis," and an important monograph by Weddige on "Aniline." The last number which has reached us of the "Encyclopædia of Physical Sciences" is the 34th, Part I., being at the same time the 50th of the whole series. It brings the Alphabetical Manual of Zoology, Anthropology, and Ethnology a considerable stage forward. The editing of this work from the letter F onwards has been committed to Reichenow in Berlin, who, with the old contributors and a large number of newly added cooperators, such as Sussdorf, Vetter, E. Taschenbergs and Georg Pfeffer, is pushing the work rapidly forwards.

On September 3 the steamer *Nordenskjöld* arrived at Hammerfest with the Dutch Meteorological Expedition saved from the *Varna* on board. The party states that the *Varna* was crushed in the ice on Christmas Eve last, but did not founder until July 24, after which date they were lodged on board the *Dijmphna*. One of the crew died during the winter. The scientific staff are exceedingly well satisfied with the result of their labours, with the exception naturally of the magnetic researches. Although Hovgaard was confident of getting into open water in August, he had decided that if not free by August 15 half the crew, under Lieut. Olsen, should leave the ship and attempt to reach the coast of Siberia at Yálmal, while he, with the other half, would winter on board. All was well on board when the Dutch departed.

The Norwegian geologist, Amund Helland, states that, having measured the following Iceland glaciers, he finds their area in Norwegian square miles to be: Vatnajökull, 150; Langjökull, 26; Hofsjökull, 25; Myrdalsjökull, 18; Drangajökull, 15; Glámujökull, 8; Forfajökull, 2; and Eyriksjökull, 2. By way of comparison he mentions that the Norwegian glacier, the Justedalsbræ, is only 14½ miles. It will thus be seen that the Iceland glaciers are larger than any others in the world, as those of the Alps and the Pyrenees are even smaller than the Norwegian.

The Norwegian zoologist, Prof. Robert Collett, a member of the Norwegian North-Sea Expedition, has written an interesting paper on the beaver in Norway. Formerly, he states, this interesting animal was found in many parts of the country, but now only in two rivers in the south. In 1876 a colony of them appeared near Porsgrund, which, however, disappeared again in 1880. Although he estimates the total number of animals at present in Norway at only about 100, he does not believe they are decreasing.

University College, Bristol, is showing considerable enterprise in extending its curriculum and improving the efficiency of its teaching. In the curriculum of work for the coming session there is an increased extension of laboratory instruction; this is a very pleasing feature. During the past session the chemical laboratory was very largely attended. The physical and electrical laboratory is now in full operation, and valuable apparatus has already been procured, though more is wanted when the funds can be obtained. A biological laboratory has also been commenced. In the ensuing session we see that a geological laboratory will be provided. Special arrangements are also made for the systematic use of the engineering workshops. The success of the engineering department hitherto has been most encouraging; and we are glad to see that the Council have now provided several facilities for the study of architectural drawing, and special arrangements for the practical work of students in this department have been made with various engineers, surveyors, and architects in and near Bristol. The medical school is rapidly growing, and already the necessity for further accommodation has become apparent. Want of funds seems to be the only check to the fuller growth and increased prosperity of the College. We believe, however, that the citizens of Bristol will not allow an important institution which is doing so much good work to feel the need of liberal support.

M. Bertrand read, at the sitting of September 10 of the Paris Academy of Sciences, a report drawn up by the Mayor of Grenoble, assisted by a commission of engineers, contradicting the rumour that the experiments on the transmission of power to a distance by the Marcel-Deprez system had failed. On the contrary, the success was complete. A power of eight horses was conveyed to Grenoble, and the original motive power underwent only a loss of 40 per cent. The force conveyed to Grenoble was utilised not only in pumping water, but in sewing, in moving machinery of every description, &c. The experiments lasted during a lengthened period, and are being continued. We must state that the distance is 14 kilometres, and the wire of copper instead of iron.

Fresh shocks of earthquake were felt at Casamicciola on the 9th and 10th inst.

M. L'Hoste, a French aëronaut, crossed the Channel in a balloon on Sunday; he left the French coast at 5 p.m. on Sunday, and landed at Smeeth, near Ashford, at 11.

The additions to the Zoological Society's Gardens during the past week include a White-fronted Capuchin (*Cebus albifrons*) from South America, presented by Miss A. Tanner; two Common Marmosets (*Hapale jacchus*) from Brazil, presented by Mr. H. H. Forbes Eden; three Mexican Deer (*Cervus mexicanus* ♂ ♀ ♀) from the Island of Santa Cruz, presented by Capt. Edwin Cole; a Getulian Ground Squirrel (*Xerus getulus*) from Morocco, presented by Mr. Geo. D. Cowan; a Grey Ichneumon (*Herpestes griseus*) from India, presented by Capt. W. F. Small; a Common Squirrel (*Sciurus vulgaris*), British, presented by Master C. B. Webster; two Stink-pot Terrapins (*Aromochelys odorata*), a Pennsylvanian Mud Terrapin (*Cinosternon pennsylvanicum*), a Mississippi Alligator (*Alligator mississippiensis*), a Sharp-nosed Crocodile (*Crocodilus acutus*) from Florida, presented by Capt. E. Cole; a Common Chameleon (*Chamæleon vulgaris*) from North Africa, presented by Mr. F. L. B. Payne; a White-fronted Capuchin (*Cebus albifrons*), a Black-faced Spider Monkey (*Ateles ater*), a Pileated Jay (*Cyanocorax pileatus*), a Spotted Tinamou (*Nothura maculosa*) from South America, two Ruddy Finches (*Carpodacus erythrinus*) from Siberia, a Jackdaw (*Corvus monedula*), British, four Eyed Lizards (*Lacerta ocellata*), South European, purchased.

OUR ASTRONOMICAL COLUMN

The Total Solar Eclipse of May 6.—The *Comptes Rendus* of the sitting of the Paris Academy of Sciences on the 3rd inst. contain the reports from the observers sent by the French Government to Caroline Island in the Pacific for the observation of the recent total eclipse of the sun. The party was composed of M. Janssen, M. Trouvelot of the Observatory of Meudon, M. Pasteur, photographer, and an assistant, who were accompanied by Prof. Tacchini, director of the Observatory of the Collegio Romano, and Herr Palisa of the Observatory of Vienna, the discoverer of a large number of minor planets. One of the main objects of the expedition was a search for so-called intra-Mercurial planets, and it is to the observations made in this direction that we shall refer here. Herr Palisa and M. Trouvelot were especially occupied with this work. The former had a telescope of 6 inches aperture, with short focus and large field, equatorially mounted. M. Trouvelot had two telescopes, one of 3 inches aperture, with large field, reticule, and interior circle of position, and one of 6 inches aperture giving a high magnifying power. The 3-inch telescope formed a sweeping instrument with a field of about 4½ degrees diameter, for the exploration of the circumsolar region. Both telescopes were on a parallactic mounting, and in order to secure rapid record of positions and dispense with the readings, which cause the loss of valuable time, M. Janssen had caused what he terms "tracelets de microscope" to be applied to the circles of right ascension and declination. Each of these, placed in the hands of an assistant, allowed of there being made, on the direction of the observer, a fine stroke across the divided circle and its vernier, so that subsequently, with the aid of this very precise indication, the instrument could be replaced in the position of the observation and the necessary readings made at leisure. It was arranged that MM. Palisa and Trouvelot should divide the work, each attending specially to one side of the sun. The Vienna astronomer's instrument, properly, as it seems, a comet-seeker, by Merz, had a magnifying power of 13, giving a field of 3°. With this, on totality taking place, he commenced his search, starting from the sun towards Saturn, at first on the south, and when he did not thus find stars he returned to the sun, and swept more to the north. In this way he recognised *nine* stars, all which are identified in the Bonn *Durchmusterung*. We give the list of stars, correcting two misprints in the *Comptes Rendus* (14°,355 should be 16°,355, and for 20°,542 we should read 20°,543), and appending the positions of the stars for the Bonn epoch 1855·0: thus, with the sun's place reduced to the same epoch, the relative positions of the stars with respect to his centre will be readily seen:—

Bonn Zone and number of star.	Magnitude.	Right Ascension. h. m. s.	Declination. ° ′
16,355	5·7	2 41 13·1	+16 51·5
16,484	6·0	2 31 15·2	16 4·1
19,477	4·2	3 3 20·8	19 10·7
19,578	5·5	3 33 57·8	19 13·9
19,582	6·0	3 35 24·5	19 12·5
20,527	4·5	3 6 34·5	20 30·5
20,543	5·0	3 12 51.0	20 37·3
20,551	5·0	3 14 24·8	20 13·4
20,556	5·8	3 16 4·2	+20 17·4

The result of his search Herr Palisa states to be that, between the limits (1855·0) 2h. 52m. from +14° to +19°, to 3h. 40m. from +16° to +22°, there was no star of the fifth magnitude unmarked in his chart, this, it should be mentioned, being a lithographic chart supplied to him by Prof. Holden, one of the American observing party.

M. Trouvelot's attention was first directed to the study and figure of the corona, but, after the totality had lasted two minutes, he applied himself to explore the region west of the sun. He moved his telescope 10° in declination to the north of the sun's centre, and swept slowly from that point from east to west, to a distance of 15° in right ascension. The first sweep brought out a small whitish star; two other sweeps were made without any result; but in the fourth he saw a bright star of a decided red colour, which he estimated at 4 or 4½ magnitude. Its approximate position was a little to the north, and a little to the west of the sun, but the cause of a more exact determination of position not being made will be best given in M. Trouvelot's own words:—"En voulant amener cet astre dans le champ très restreint de l'oculaire du 6 pouces (0·16m.), afin de chercher à constater s'il montrait traces soit d'un disque, soit d'une phase, il se produisit une certaine confusion parmi les deux aides que j'avais placés aux cercles horaire et de déclinaison pour guider la course des balayages, et bien que l'étoile traversât le champ visuel, il me fut impossible de retenir en place la lunette, et dès lors de reconnaître son caractère et sa position." In the abstract of results of observations appended to the reports of the observers, after reference to Herr Palisa's experiences, we read in the *Comptes Rendus*: "M. Trouvelot arrive à un résultat moins net pour le côté ouest, mais nous savons que cet observateur distingué désire revoir la région où se trouvait le soleil au moment de l'éclipse avant de ce prononcer." It is stated that the photographs, though not yet examined in a complete manner, appear to support the negative result obtained by Herr Palisa as to the existence of an intra-Mercurial planet.

A New Comet.—A Dun Echt circular (No. 78) notifies the discovery of a comet by Mr. Brooks on September 2, which was thus observed by Mr. Wendell at the Harvard College Observatory on the following night:—

	Greenwich M.T. h. m. s.	R.A. h. m s.	Decl. ° ′ ″
September 3 at	16 9 24·5	16 35 15·6	+ 64 49 33

Daily motions in R.A. − 36s., in declination − 12′. It is described as circular; less than 1′ diameter; tenth magnitude; well defined nucleus, and no tail.

THE GERMAN SURVEY OF THE NORTHERN HEAVENS[1]

The illustrious Argelander was accustomed to say in the quaint form of speech which he often employed, "The attainable is often not attained if the range of inquiry is extended too far." In no undertaking is there greater need of a judicious application of this sound maxim than in the systematic determination of the exact positions of all the stars in the visible heavens which fall within the reach of telescopes of moderate power.

The first subject which engaged the attention of the *Astronomische Gesellschaft*, at its formation in 1865, was the proposition to determine accurately the coordinates of all the stars in the northern heavens down to the ninth magnitude. To this association of astronomers (at first national, but since become largely international, in its character and organisation) belongs the credit

[1] An address delivered by Prof. William A. Rogers before the American Association for the Advancement of Science at Minneapolis on August 15, 1883.

of arranging a scheme of observations by which, through the co-operation of astronomers in different parts of the world, it has been possible to accomplish the most important piece of astronomical work of modern times. With a feasible plan of operations, undertaken with entire unity of purpose on the part of the observers to whom the several divisions of the labour were assigned, this great work is now approaching completion. While it is yet too early to speak with confidence concerning the definitive results which the discussion of all the observations is expected to show, we may with profit consider the object sought in the undertaking, the general plan of the work, the difficulties which have been encountered, and the probable bearing which the execution of the present work will have upon the solution of a problem concerning which we now know absolutely nothing with certainty,—a problem of which what we call universal gravitation is only one element, if, indeed, it be an element,—a problem which reaches farther than all others into the mysteries of the universe,—the motion of the solar and the sidereal systems in space.

Our first inquiry will be with respect to the condition of the question of stellar positions at the time when this proposal was made by the *Gesellschaft* in 1865. All the observations which had been made up to this time possess one of two distinct characteristics. A portion of them were made without direct reference to any assumed system of stellar coordinates as a base; but by far the larger part are differential in their character. This remark holds more especially with reference to right ascensions. Nearly all of the observations of the brighter stars made previous to about 1830 were referred to the origin from which stellar coordinates are reckoned by corresponding observations of the sun; but since that date it has been the custom to select a sufficient number of reference stars, symmetrically distributed both in right ascension and declination, and whose coordinates were supposed to be well known. The unequalled Pulkowa observations for the epoch 1845, form, I believe, the only exception to this statement. From the assumed system of primary stars are derived the clock errors and instrumental constants which are employed in the reduction of all the other stars observed. The positions of these secondary stars, therefore, partake of all the errors of the assumed fundamental system, in addition to the direct errors of observation.

The following list comprises the most important of the catalogues which have been independently formed: viz. Bessel's Bradley for 1755, the various catalogues of Maskelyne between 1766 and 1805, Gould's d'Agelet for 1783, Piazzi for 1800, Auwer's Cacciatore for 1805, Bessel for 1815, a few of the earlier catalogues of Pond, Brinkley for 1824, Bessel for 1825, Struve for 1825, Bessel for 1827, Struve for 1830, Argelander for 1830, and Pulkowa for 1845.

An analysis of these catalogues reveals four important facts:—

First, that, a large share of the observations relate to bright stars, at least to stars brighter than the eighth magnitude.

Second, that in a large number of cases the same star is found in different catalogues, but that no rule is discoverable in the selection.

Third, that, with the exception of the Polar catalogues of Fedorenko, Groombridge, Schwerd, and Carrington, the double-star observations of Struve, and the zone observations of Bessel and Argelander, the observations were not arranged with reference to the accomplishment of a definite object.

Fourth, that each catalogue involves a system of errors peculiar to the observers, to the character of the instruments employed, and to the system of primary stars selected, but that thus far there had been no attempt to reduce the results obtained by different observers to a homogeneous system. In estimating the value of these observations it will be necessary to refer to the researches which have been made subsequent to 1865.

The systematic deviations of different catalogues in right ascension *inter se* were noticed at an early date by several astronomers; but the first attempt to determine the law of these variations seems to have been made by Safford in a communication to the *Monthly Notices of the Royal Astronomical Society* in 1861 (xxi. 245), on the positions of the Radcliffe catalogue. I quote the equation derived by Safford, since it appears to be the first published account of a form of investigation almost exclusively followed since that time. It is as follows:—

Diff. of R.A. (Greenw. 12 Year Cat.—Rad.) = – 0·38s. + 0·32s. sin (a + 5h. 32m.). Extending this expression to terms of the second order, it may be put under the form $\Delta = a$ constant + (m sin a + n cos a) + (m' sin 2 a + n' cos 2 a) +, &c.

Safford also seems to have been the first to notice the connection between the observed residuals, and the errors in position of the primary stars employed. He remarks, "In investigating the causes which would give rise to such systematic discrepancies, I was struck with the fact that the same or nearly the same variations were apparent in the assumed places of the time stars for the years since 1845; that, if the correct positions of the time stars had been assumed, the resulting positions would have been free from these small errors." That the relation given by Safford should have been observed at all is the more remarkable since the primary stars upon which the Radcliffe positions depend are nearly the same as those employed at Greenwich. In reality the systematic errors of both catalogues have since been found to be considerably greater than is here indicated, and the deviation pointed out by Safford is in the nature of a second difference. The speaker has shown (*Proc. Amer. Acad.* 1874, 182) that the weight of the errors of the provisional catalogue assumed fell between the first and the third quadrants in the Radcliffe observations for 1841–42, on account of the omission of certain clock stars which were used at Greenwich.

Since the discordances which exist between two catalogues may arise from errors in either one or in both, it is clearly impossible either to determine the nature of the errors or to assign their true cause until a fundamental system has been established which is free both from accidental and from periodic errors,—from accidental errors, since a few abnormal differences may easily invalidate the determination of the errors which are really periodic; from periodic errors, because a relative system can only become an absolute one when one of the elements of which it is composed becomes absolute.

We owe to the researches of Newcomb, published in 1869–70, a homogeneous system of stellar coordinates in right ascension, which are probably as nearly absolute in their character as it is possible to obtain from the data at present available. He determined the absolute right ascensions of thirty-two stars of the first, second, and third magnitudes, and comprised between the limits $-30°$ and $+46°$ declination. A comparison of the places of these stars for a given epoch with the same stars in any catalogue for the same epoch enables us to determine with considerable precision the system of errors inherent in that catalogue. Several circumstances prevent the exact determination of this relation. Among them may be mentioned the fact that Newcomb's system cannot safely be extended far beyond the limits in declination of the stars composing the system, that the stars are not symmetrically distributed in declination, and that the system of errors derived from bright stars is probably not the same as that derived from stars of less magnitude.

To a certain extent all of these objections have been met in the later discussion by Auwers, to which reference will presently be made. The substantial agreement of these two systems, independently determined, furnishes satisfactory evidence that we have at last obtained a foundation system with which it is safe to make comparisons—from which we may draw conclusions with comparative safety. When the catalogues which were formed between 1825 and 1865 are compared with Newcomb's fundamental system, through the medium of these thirty-two stars, the following facts are revealed:—

a. The only catalogues in which there is freedom from both accidental and periodic errors are Argelander's Abo catalogue for 1830 and the Pulkowa catalogue for 1845. One is reminded in this connection of the remark of Pond, that "we can hardly obtain a better test of our power of predicting the future positions of stars than by trying by the same formula how accurately we can interpolate for the past. In a variety of papers which I have submitted to the Royal Society I have endeavoured to show that with us the experiment *entirely* fails."

b. During this interval the constant differences between the earlier catalogues and Newcomb's system vary between +0·17s. for Pond, 1820, and –0·19s. for Pond, 1830; and for later catalogues between +0·07s. for Cambridge, 1860, and +0·02s. for Greenwich, 1860.

c. All the right ascensions determined at English observatories, and especially those which depend upon the positions published by the British *Nautical Almanac*, are too large in the region of five hours, and too small in the region of eighteen hours. The general tendency of the constant part of the deviation from Newcomb's system is to neutralise the periodic errors in the region of five hours, and to augment them in the region of eighteen hours, where, in the case of a few catalogues, the error becomes as great as 0·10s.,—a quantity which can be readily

detected from the observations of two or three evenings with an indifferent instrument, if it relates to a single star.

The right ascensions determined at French observatories exhibit systematic errors which follow nearly the same law as those which characterise English observations.

Distinctively German observations are nearly free from systematic errors. As far as they exist at all, their tendency is to neutralise the errors inherent in distinctively English and French observations.

d. In the case of several catalogues residual errors of considerable magnitude remain after the systematic errors depending upon the right ascensions have been allowed for. These errors are found to be functions of the declination of the stars observed, and without doubt have some connection with the form of the pivots of the instrument with which the observations were made. This statement holds true, especially with respect to the observations at Paris, Melbourne, and Brussels, between 1858 and 1871; and to the Washington observations between 1858 and 1861.

e. The systematic errors which exist in observations previous to 1865 follow the same law and have nearly the same magnitude as the errors of the same class which are inherent in the national ephemerides of the country in which they were made.

The British *Nautical Almanac* and the *Connaissance des Temps* are largely responsible for the perpetuation of this class of errors. For a few years before and after 1860 the ephemerides of the *Nautical Almanac* were based upon the observations of Pond, which contain large periodic errors. It is found that the errors of this system have been transferred without sensible diminution to every catalogue in which the observations depend upon *Nautical Almanac* clock stars. At English observatories it has been the custom to correct the positions of the fundamental stars by the observations of each successive year; but this has produced no sensible effect on the diminution of the periodic errors, which belong to the fundamental system. The periodic errors of the *American Ephemeris* follow nearly the same law as the errors of the *Nautical Almanac*, but their magnitude is somewhat reduced. The error of equinox is also less.

Wolfer's *Tab. Reg.*, upon which the *Berliner Jahrbuch* is based, has no well defined systematic errors, and the correction for equinox is nearly the same in amount as in the *American Ephemeris*, but with the opposite sign. The accidental errors seem to be rather larger than in the system of the *American Ephemeris.*

f. A general estimate may be formed of the relative magnitudes of the errors of secondary catalogues by comparing the average error for each star of the primary catalogue. The numbers given below represent the average deviation for each star, expressed in hundredths of seconds, after the various catalogues have been reduced to a common equinox:—

		Average error for each star.
Argelander	1830	1·1
Pulkowa	1845	1·1
Greenwich	1845	2·0
Greenwich	1860	2·0
D'Agelet (Gould)	1783	2·2
Cape of Good Hope (Henderson)	1833	2·2
Greenwich	1850	2·2
Greenwich	1871	2·2
Paris	1867	2·4
Washington	1846–52	2·5
Struve	1830	2·5
Cape of Good Hope	1856	2·8
Radcliffe	1860	3·1
Greenwich	1840	3·1
Bessel	1825	3·2
Pond	1830	3·7
Gillis	1840	3·8
Madras (Taylor)	1830	3·9
Cape of Good Hope (Fallows)	1830	3·9
Radcliffe	1845	4·5
Armagh	1840	5·0
Piazzi	1800	5·3
Bessel's Bradley	1755	7·9
Lalande	1800	13·2
Lacaille	1750	24·9

It is obvious from these relations that previous to about 1825 the magnitude of the accidental errors of observation, combined with the errors of reduction, prevent any definite conclusions with respect to the periodic errors inherent in these early observations. It is probable, also, that early observations of stars of the eighth and ninth magnitudes are subject to a class of errors peculiar to themselves, the nature of which is now well nigh impossible to determine.

The systematic errors in declination which belong to the various secondary catalogues named are even more marked than those in right ascension. The experience of Pond in 1833 is the experience of every astronomer who has attempted to compare observations of the same star made at different times, under different circumstances, with different instruments, and by different observers. He says: "With all these precautions, we do not find by comparing the present observations with those of Bradley made eighty years ago under the same roof, and computed by the same table of refractions, that we can obtain by interpolation any intermediate catalogue which shall agree with the observations within the probable limits of error."

We owe to the investigations of Auwers (*Astron. Nachr.*, Nos. 1532-1536), the first definite system of declinations which is measurably absolute in its character. Yet the deviations of this system from that derived by the same author, but from much additional data in Publication xiv. of the *Gesellschaft*, is no less than 1·2s. The present difference outstanding between the Pulkowa and the Greenwich systems at 10° south declination is 1·7s.

Within the past five years the labours of Auwers, of Safford, of Boss, and of Newcomb, have resulted in the establishment of a mean system of declinations from which accidental errors may be considered to be eliminated in the case of a large number of stars; but the different systems still differ systematically *inter se* by quantities which are considerably greater than the probable error of any single position.

When the discussion of the question of a uniform determination of all the stars in the northern heavens to the ninth magnitude was taken up by the *Gesellschaft* at its session in Leipzig in 1865, Argelander, who was then president of the Society, appears to have been the only astronomer who had a clear apprehension of the difficulties of the problem. He alone had detected the class of errors whose existence subsequent investigations have definitely established. He alone had found a well-considered plan by which these errors might be eliminated, as far as possible, from future observations.

Argelander, however, always claimed for Bessel the first definite proposal of the proposition under consideration (see *Astron. Nachr.*, i. 257). It was in pursuance of this plan that the zones between $-15°$ and $+15°$ in declination were observed. These zones were to form the groundwork of the Berlin charts; and Argelander, in the execution of the Bonner *Durchmusterung*, simply carried out the second part of Bessel's recommendation.

With the exception of the observations of Cooper at Markree Observatory, and the charts of Chacornac, these two great works —the second being a continuation of the first, under a better and more feasible plan—are the only ones in existence which give us any knowledge of the general structure of the stellar system.

The observations of stars to the ninth magnitude, found in the catalogues of Bessel, Lalande, and Piazzi, form the groundwork of these charts. The coordinates in right ascension and declination of the stars found in these authorities were first reduced to the epoch 1800; the resulting right ascension being given to seconds of time, and the declination to tenths of minutes of arc. With these places as points of reference, all other stars were filled in, down to the ninth magnitude, by observations with equatorial instruments. The work was divided into zones of one hour each. Bremiker undertook five zones; Argelander and Schmidt, two; Wolfers, three; and Harding, two. The remaining zones were undertaken by different astronomers in widely separated localities.

The work seems to have been performed with somewhat unequal thoroughness, some zones containing nearly all the stars to the ninth magnitude, while in others a large number of stars having this limit in magnitude are wanting.

The *Durchmusterung*, undertaken by Argelander at Bonn, was a far more serious and well-considered undertaking. This unequalled work consists in the approximate determination of the coordinates of 324,198 stars situated between $-2°$ and $+90°$ declination. It includes stars to the 9·5 magnitude, the coordinates being given to tenths of seconds of time, and the declinations to tenths of minutes of arc.

The first definite proposal of this work undertaken by the *Gesellschaft*, however, appears to have been made by Bruhns. In

the course of a report upon the operations of the Leipzig Observatory, he stated that in his view the time had come for undertaking a uniform system of determinations of the places of stars to the ninth magnitude in the northern hemisphere by means of meridian circles; but he proposed at the same time that the positions of stars fainter than the ninth magnitude should be determined by means of differential observations with equatorial instruments. After explaining certain plans and arrangements relating particularly to his own observatory, he introduced the following resolution:—

"The *Astronomische Gesellschaft* regards it as needful that all the stars to the ninth magnitude occurring in the *Durchmusterung* should be observed with meridian circles, and commissions the Council to arrange for the execution of the work."

This proposal occasioned a long and somewhat animated discussion, in which Argelander, Hirsch, Bruhns, Förster, Schönfeld, and Struve took part.

Argelander declared himself surprised at this proposal, which called for the rapid realisation of a plan of organisation which he had been considering for years with the greatest care, the difficulties of which he had maturely considered, and the execution of which still demanded the most careful deliberation and preparation. One of the necessary preliminary steps was a plan which he had already prepared, published, and presented to the Society in an informal way, which provided for contemporaneous and corresponding observations of the brighter stars. As president of the Society he felt unequal to undertaking the charge which the acceptance of the resolution proposed would involve, as this procedure seemed to him premature without previous preparation. He would admit, however, that every call to action of this kind tended to stimulate enthusiasm, and should therefore be encouraged, but he felt obliged to ask the Society not to require from him the immediate execution of the plan, but to intrust the serious consideration of it and the preparation for it to his zealous friends in the Council.

Upon the motion of Struve, the Society, by a rising vote, expressed its confidence in the assurance of the president that he would bring forward his plan at the proper time, as soon as the means for its execution could be assured.

At the meeting held at Bonn in 1867 Argelander again brought up the subject in a communication which appears to have been an exhaustive discussion of the whole problem. This paper is not printed in the *Proceedings of the Gesellschaft*, but at its conclusion a committee was appointed to take definite action with respect to the recommendations which it contained. The committee reported at the same session, and their report, which is published in the place of the paper presented by Argelander, is probably identical in substance with it. The plan proposed and adopted was finally published in the form of a programme, in which the details of the work are arranged with considerable minuteness. As this programme has been widely distributed, it seems unnecessary to give anything more than a general abstract of it. Since it differs in a few minor points from the first report of the committee at the Bonn meeting, the essential features of this report will be given instead of an abstract of the programme itself.

They are as follows:—

a. The limits in declination of the proposed series of observations are $-2°$ and $+80°$. The first limit was chosen on account of the lack of suitable fundamental stars south of the equator. It is probable, also, that Argelander had a suspicion of the fact, since proven, that the uncertainty with respect to the systematic errors of southern stars is, of necessity, considerably greater than for northern stars, and that on this account it would be better to defer this part of the work until further investigations in this direction could be made.

The limit $+80°$ was chosen because the repetition of Carrington's observations between 81° and 90° was considered superfluous, and Hamburg had already undertaken the extension of Carrington's observations from 81° to 80°.

b. Within these limits, all stars in the *Durchmusterung* to the ninth magnitude, and, in addition, all stars which have been more exactly observed by Lalande, by Bessel at Königsberg, and by Argelander at Bonn, are to be observed.

c. The observations are to be differential. The clock errors are not to be found from the fundamental stars usually chosen for this purpose, and the equator point corrections are not to be derived from observations at upper and lower culminations, but these elements are to be derived from a series of 500 or 600 stars, distributed as uniformly as possible over the northern heavens. The exact coordinates of these stars are to be determined at Pulkowa, thus securing the unity necessary in order to connect in one system the observations of different zones.

d. Every star is to be observed twice. If the two observations differ by a quantity greater than ought to be expected, a third observation will be necessary.

e. In order to facilitate the work it will be desirable to use only three or four transit threads and only one or two microscopes. In order to facilitate the reductions to apparent place the working-list of stars should be comprised within narrow limits.

f. Before the commencement and after the close of each zone, two or three fundamental stars are to be observed upon the same threads and with the same microscopes as were used in the zone observations. When the seeing is not good, and when for any other cause it seems desirable, one or more fundamental stars may be observed in the course of the zone. The number and selection of the stars will depend upon the character of the instrument employed. If it remains steady for several hours and has no strongly marked flexure or division errors, or if these errors have been sharply determined, the fundamental stars may be situated ten degrees or fifteen degrees away from the zone limits. However, there must remain many things for which no general rule can be given, and which must be left to the judgment of the observer, aided by an accurate knowledge of his instrument.

g. With a Repsold or a Martin instrument one microscope will be sufficient, if its position with respect to the whole four can be determined. It will be sufficient if the change in position during the observations can be interpolated to 0·2s.

h. It will be desirable to divide beforehand the zones into such time intervals that the observations can be easily made.

i. Zones exceeding one and a half or at the most two hours are not advisable, first, because the zero points will be too far apart, and, second, because a longer duration will involve too much fatigue physically and mentally.

At the conclusion of this report all the astronomers present who were willing to take part in this work were requested to communicate with the Council, stating the regions of the heavens which they preferred to select for observation.

At this meeting, Berlin, Bonn, Helsingfors, Leipzig, and Mannheim signified their intention to share in the work. Leyden also expressed its intention of taking part as soon as the work already undertaken should be completed.

When the stars to be observed had been selected from the *Durchmusterung*, it was found that the number would not vary much from 100,000, requiring rather more than 200,000 observations. Preparations for the work of observation were immediately commenced, and, by the time of the next report in 1869, considerable progress had been made.

In the report for this year the provisional places of a catalogue of 539 fundamental stars were published. This catalogue is composed of two parts. The list of *Haupt-sterne* consists of 336 stars to the fourth magnitude, observed at Pulkowa by Wagner with the large transit instrument, and by Gyldén with the Ertel vertical circle. The list of *zu za t-sterne* consists of 203 stars fainter than the fourth magnitude. As the details of the work in the formation of the provisional places of the stars of this list are not given in the report, it is not quite clear upon what authority they rest. The work assigned to the Pulkowa observatory by the Zone Commission was the exact determination of the places of the stars of this list. The observations were undertaken by Gromadski with the Repsold meridian circle. In accordance with the plan adopted each star was observed eight times—four times in each position of the instrument. The observations were differential with respect to the *Haupt-sterne*.

The results were published by Struve in 1876, and the places there given were used in the first reduction of the Harvard College observations for 1874-75, and perhaps in some other cases.

About this time a change seems to have been made in the original plan with respect to the formation of the final catalogue of fundamental stars, of which I have been unable to find a clear account. The original intention was to make the positions depend entirely upon the observations at Pulkowa. The Zone Commission established by the *Gesellschaft*, however, committed the formation of this catalogue to Auwers; and it is to him that we owe the most complete and the most perfect catalogue of fundamental stars yet published. The Pulkowa system for 1865 was adopted as the basis; but, in order to obtain greater

freedom from accidental errors for individual stars, the final catalogue was obtained by combining with the Pulkowa series the Greenwich observations from 1836 to 1876, the Harvard College observations for 1871–72, the Leipzig observations in declination only, between 1865 and 1870, and the Leyden observations in declination between 1864 and 1870. Before this combination was made, however, these observations were all reduced to the Pulkowa system.

The following observatories have taken part in the zone observations:—

Observatories.	Limits of zones in declination.	Observatories.	Limits of zones in declination.
Nicolaieff ...	− 2° to + 1°	Lund	+35° to +40°
Albany	+ 1 ,, + 5	Bonn	+40 ,, +50
Leipzig	+ 4 ,, +10	Harvard College	+50 ,, +55
Leipzig	+10 ,, +15	Helsingfors ...	+55 ,, +60
Berlin	+15 ,, +25	Christiania ...	+65 ,, +70
Cambridge (Eng.)	+25 ,, +30	Dorpat	+70 ,, +75
Leyden	+30 ,, +35	Kasan	+75 ,, +80

The zone between −2° and +1° was originally undertaken at Palermo, that between +1° and +4° at Neuchâtel, that between +4° and +10° at Mannheim, and that between +35° and +40° at Chicago.

In the latter case the great fire at Chicago crippled the resources of the observatory to such an extent that Safford was compelled to relinquish the work, which was at that time quite far advanced.

Attention was called at an early date to the importance of continuing the survey of the northern heavens beyond the southern limit fixed by Argelander. The preparation necessary for the execution of this work consisted in the extension of the *Durchmusterung* to the tropic of Capricorn. This was undertaken by Schönfeld at Leipzig.

In the report to the *Gesellschaft* at the meeting held at Stockholm in 1877, he has given an account of this work, in which he stated that it was sufficiently near completion to invite the consideration of the question of the meridian circle determinations of the places of stars to the ninth magnitude. The lack of southern fundamental stars whose positions were well determined was still a hindrance to the immediate commencement of the work. Relatively more stars of this class are required than in the northern observations, in order to eliminate the inequalities due to refraction. Schönfeld stated that, while the burden of the determination of the places of these southern fundamental stars must rest mainly upon southern observations, it seemed necessary to connect them with the Pulkowa system by a connecting link (*Mittelnlied*), through observations at some observatory well situated for this purpose. At this meeting Sande Bakhuysen, of Leyden, gave notice of intention to take part in this work. Gyldén urged the importance of securing the co-operation of Melbourne, and Peters suggested the advantage of securing Washington as an additional "mean term" (V.J.S. 1877, p. 265).

The next reference to this work is contained in the *Gesellschaft* for 1881 (V.J.S. xv. p. 270). A list of 303 southern stars is here given whose exact places were at that time being determined at Leyden and at the Cape of Good Hope. This list was selected by Schönfeld and Sande Bakhuysen, in a way to meet the requirements referred to in previous discussions.

A final catalogue of eighty-three southern fundamental stars by Auwers appears in this number of the *Gesellschaft*. The places depend upon the same authorities as for the northern stars, with the addition of the Cape of Good Hope catalogue for 1860, Williamstown, Melbourne for 1870, and Harvard College (Safford) for 1864. For stars not observed at Pulkowa, the general catalogue of Yarnall (1858-1861), and the Washington observations, with the new meridian circle between 1872 and 1875, were employed. As in the case of the northern stars, these observations are all reduced to the Pulkowa system for 1865. It is understood that the coordinates of the list of 303 stars are to depend upon this extension of the general system of Publication xiv. to the limits required by the southern *Durchmusterung* of Schönfeld.

It would be surprising if all the conditions of success were fulfilled in the first execution of a work having the magnitude and involving the difficulties of the scheme of observations undertaken under the auspices of the *Gesellschaft*. The extent of the discordances which are to be expected between the results obtained by different observers can only be ascertained when the observations by which the different zones are to be connected have been reduced. Each observer extended the working list of his own zone 10′ north and south; and it is expected that a sufficient number of observations of this kind have been made to determine the systematic relations existing between the coordinates of each zone with those of its neighbour.

It is probable, however, that the experience of Gill will be repeated on a larger scale. In 1871 he solicited the cooperation of astronomers in the determination of the coordinates of twenty-eight stars, which he desired to employ in the reduction of his heliometer observations of the planet Mars for the purpose of obtaining the solar parallax. The results obtained at twelve observatories of the first class are published in vol. xxxix. p. 99, of the *Monthly Notices of the Royal Astronomical Society*. Notwithstanding the fact that the final values obtained at each observatory depend upon several observations, the average difference between the least and the greatest results, obtained by different observers for each star, is 0·24s. in right ascension, and 2·3″ in declination. In four cases the difference in right ascension exceeds 30s., and in four cases the difference in declination exceeds 3·0″.

Even after the results are reduced to a homogeneous system, the following outstanding deviations from a mean system are found:—

Authority.	Δ α (s.)	Δ δ (″)	Authority.	Δ α (s.)	Δ δ (″)
Königsberg	+·005	−0·71	Leyden ...	−·053	−0·19
Melbourne	+·026	−0·49	Paris	+·055	+0·01
Pulkowa ...	+·005	+0·36	Washington	−·120	+0·78
Leipzig ...	+·049	+0·40	Harvard Coll.	−·072	+0·09
Greenwich ...	+·009	−0·56	Cordova ...	−·032	−0·20
Berlin ...	+·044	+0·67	Oxford ...	+·076	+0·21

The observations of a second list of twelve stars, one-half of the number being comparatively bright, and the remaining half faint, showed no marked improvement, either with respect to the magnitude of errors which could be classed as accidental, or in regard to the systematic deviations from a mean system.

This discussion revealed one source of discordance which will doubtless affect the zone observations: viz. the difference between the right ascensions determined by the eye-and-ear method and those determined with the aid of the chronograph.

The programme of the *Gesellschaft* makes no provision for the elimination of errors which depend upon the magnitude of the stars observed; but special observations have been undertaken at several observatories for the purpose of defining the relation between the results for stars of different magnitudes. At Harvard College Observatory, the direct effect of a reduction of the magnitude has been ascertained by reducing the aperture of the telescope by means of diaphragms. Besides this, the observations have been arranged in such a manner that an error depending upon the magnitude can be derived from an investigation of the observations upon two successive nights.

At Leyden, at Albany, and perhaps at other observatories, the effect of magnitude has been determined by observations through wire gauze. But notwithstanding all the precautions which have been taken in the observations, and which may be taken in the reductions, it will undoubtedly be found that the final results obtained will involve errors which cannot be entirely eliminated.

In the experience of the writer two other sources of error have been detected. It has been found that there is a well defined equation between the observations, which is a function of the amount, and the character of the illumination of the field of the telescope. It has also been found that observations made under very unfavourable atmospheric conditions differ systematically from those made under favourable conditions. When the seeing was noted as very bad, it is found that the observed right ascensions are about ·08s. too great, and that the observed declinations are about 0·8″ too great.

There are doubtless other sources of error which the discussion of the observations will bring to light. The effect of the discovery of these and other errors will probably be to hasten the repetition of the zone observations under a more perfect scheme, framed in such a manner as to cover all the deficiencies which experience has revealed or may yet reveal. One would not probably go far astray in naming the year 1900 as the mean epoch of the new survey. If the observations are again repeated in 1950, sufficient data will then have been accumulated for at least an approximate determination of the laws of sidereal motion.

What is the present state of our knowledge upon this subject? It can be safely said that it is very limited. First of all it cannot be affirmed that there is a sidereal system in the sense in which we speak of the solar system. In the case of the solar system we have a central sun about which the planets and their satellites revolve in obedience to laws which are satisfied by the hypothesis of universal gravitation. Do the same laws pervade the interstellar spaces? Is the law of gravitation indeed universal? What physical connection exists between the solar system and the unnumbered and innumerable stars which form the galaxy of the heavens? Do these stars form a system which has its own laws of relative rest and motion, or is the solar system a part of the stupendous whole? Does the solar system receive its laws from the sidereal system, or has Kepler indeed pierced the depths of the universe in the discovery of the laws which gave him immortality? Are we to take the alternative stated by Ball,—either that our sidereal system is not an entirely isolated object, or its bodies must be vastly more numerous or more massive than even our most liberal intepretation of observations would seem to warrant? Are we to conclude, for example, that stars like 1830 Groombridge and α Centauri, "after having travelled from an infinitely great distance on one side of the heavens, are now passing through our system for the first and only time, and that after leaving our system they will retreat again into the depths of space to a distance which, for anything we can tell, may be practically regarded as infinite?" Can we assert with Newcomb, that in all probability the stars do not form a stable system in the sense in which we say that the solar system is stable,—that the stars of this system do not revolve around definite attractive centres? Admitting that the solar system is moving through space, can we at the present moment even determine whether that motion is rectilinear, or curved, to say nothing of the laws which govern that motion. How much of truth is there in the conjectures of Wright, Kant, Lambert, and Mitchel, or even in the more serious conclusions of Mädler that the Alcyone of the Pleiades is the central sun about which the solar system revolves?

These are questions which, if solved at all, must be solved by a critical study of observations of precision accumulated at widely separated epochs of time. The first step in the solution has been taken in the systematic survey of the northern heavens undertaken by the *Gesellschaft*, and in the survey of the southern heavens at Cordova by Dr. Gould. The year 1875 is the epoch about which are grouped the data which, combined with similar data for an epoch not earlier than 1950, will go far towards clearing up the doubts which now rest upon the question of the direction and the amount of the solar motion in space; and it cannot be doubted that our knowledge of the laws which connect the sidereal with the solar system will be largely increased through this investigation. The basis of this knowledge must be the observed proper motions of a selected list of stars, so exactly determined that the residual mean error shall not affect the results derived; or, failing in this, of groups of stars symmetrically distributed over the visible heavens, sufficient in number to effect an elimination of the accidental errors of observation without disturbing the equilibrium of the general system.

For an investigation of this kind, a complete system of zone observations, at widely separated intervals, will afford the necessary data, if the following conditions are fulfilled.

First, the proper motions must be derived by a method which does not involve an exact knowledge of the constants of precession. In every investigation with which I am acquainted the derived proper motions are functions of this element.

Second, the general system of proper motions derived must be free from systematic errors. Errors of this class may be introduced either through the periodic errors inherent in the system of fundamental stars employed in the reduction of the zone observations, or in a change in the constants of precession. It is in this respect that the utmost precaution will be required. If from any cause errors of even small magnitude are introduced into the general system of proper motion at any point, the effect of these errors upon the values of the coordinates at any future epoch will be directly proportional to the interval elapsed. We can, therefore, compute the exact amount of the accumulated error for any given time.

When this test is applied to the fundamental stellar systems independently determined by Auwers, Safford, Boss, and Newcomb, we find the following deviations *inter se* at the end of a century:—

	Maximum mean deviation in a century. $\Delta\alpha$	$\Delta\delta$		Maximum systematic deviation in a century.	
Auwers *minus* Safford ...	$-0{\cdot}22$s.	$+0{\cdot}2''$	...	$0{\cdot}23$s.	$1{\cdot}1'$
Auwers *minus* Boss ...	—	$+0{\cdot}8$	...	—	$2{\cdot}1$
Auwers *minus* Newcomb	$-0{\cdot}09$	$+0{\cdot}8$	...	$0{\cdot}06$	$2{\cdot}2$

It is the common impression that both the direction and the amount of the motion of the solar system in space are now well established. The conclusions of Struve upon this point are stated in such explicit language that it is not surprising that this impression exists. He says, "The motion of the solar system in space is directed to a point in the celestial sphere situated on the right line which joins the two stars measured from π and ω Herculis. The velocity of this motion is such that the sun, with the whole *cortège* of bodies depending on him, advances annually in the direction indicated, through a space equal to 154,000,000 miles.

It must be admitted that there is a general agreement in the assignment by different investigators of the coordinates of the solar apex. This will be seen from the following tabular values:—

Authorities.		Right Ascension.		Declination.
		° ′		° ′
Herschel, 1783		257 00	...	+25 00
Prevost, 1783		230 00	...	+25 00
Klugel, 1789		260 00	...	+27 00
Herschel, 1805		245 52	...	+49 38
Argelander, 1837		257 49	...	+28 50
Lundahl, 1837		252 24	...	+14 26
Struve, 1837		261 22	...	+37 36
Galloway, 1837		260 01	...	+34 23
Mädler, 1837		261 38	...	+39 54
Airy, 1837	{	256 54	...	+34 29
		261 29	...	+26 44
Dunkin	{	261 14	...	+32 55
		263 44	..	+25 00

In estimating the value which should be attached to these results, several considerations must be taken into account.

(*a*) All of the results except those of Galloway depend practically upon the same authorities at one epoch, viz. upon Bradley.

(*b*) The deviations *inter se* probably result in a large measure from the systematic errors inherent in one or both of the fundamental systems from which the proper motions were derived. For example, Lundahl employed Pond as one of his authorities, and it is in Pond's catalogue that the most decided periodic errors exist.

(*c*) Biot in 1812, Bessel in 1818, and Airy in 1860, reached the conclusion that the *certainty* of the movement of the solar system towards a given point in the heavens could not be affirmed.

(*d*) The problem is indirect. In the case of a member of the solar system, exact data will determine the exact position in orbit at a given time; but here we have neither exact data nor can we employ trigonometrical methods in the solution. We simply find that the observed proper motions are probably somewhat better reconciled under the hypothesis of an assumed position of the apex of the solar motion. The method of investigation employed by Safford, who has of late years given much attention to this subject, consists in assuming a system of coordinates for the pole of the solar motion, from which is determined the direction each star would have if its own proper motions were zero. Comparing this direction with the observed direction as indicated by the observed proper motion, equations of condition are formed from which a correction is found to the assumed position of the apex, by the methods of least squares.

It must always be kept in mind that the quantities with which we must deal in this investigation are exceedingly minute, and that the accidental errors of observation are at any time liable to lead to illusory results. The weak link in the chain of Mädler's reasoning is to be found here. I think we can assume $0{\cdot}20''$ as the limit of precision in the absolute determination of the coordinates of any star, however great the number of observations upon which it depends. Beyond this limit it is impossible to go, in the present state of instrumental astronomy.

It is safe to say that there is not a single star in the heavens whose coordinates are known with certainty within this limit. Do not misunderstand me. Doubtless there are many stars in which

the error will at some future time be found to fall within this limit. The law of probabilities requires this, if the maximum limit falls within 1″. But who is prepared to select a particular star and say that the absolute position of this star in space cannot be more than 0·2″ in error?

e. At present an arbitrary hypothesis is necessary in the discussion of the problem. Airy assumed that the relative distances of the stars are proportional to their magnitudes; and he found slightly different results according to different modes of treatment. Safford assumed that the distances are, at least approximately, in inverse proportion to the magnitude of the proper motions. The general result of his investigations up to this point is that there is some hope of using the solar motion as a base to advance our knowledge of stellar distances. Later investigations have been made by De Ball, but the details have not yet come to hand. It is understood, however, that his results coincide in a general way with those previously obtained.

It is clear from this brief review that we have here a field of investigation worthy of the highest powers of the astronomer. The first step has been taken in the survey of the heavens carried on under the auspices of the *Gesellschaft*. It remains for the astronomers of the present generation to solve the difficulties which now environ the problem, and prepare the way for a more perfect scheme of observation in the next century.

INDIAN METEOROLOGY[1]

III.

THE next paper we shall notice is No. IX., by Fred. Chambers, on "The Winds of Kurrachee." The station dealt with is not only a representative one of the Arabian sea current, but is remarkable for exhibiting the highest average monthly wind velocity of any place in India. The observations used were furnished by a Beckley's anemograph for 1873, 1874, and 1875.[2]

In discussing the annual variation, Mr. Chambers adopts a plan which has been followed out with much success by his brother in his great work on the meteorology of the Bombay Presidency, viz. its separation into *normal* and *abnormal* north and east components.

It is thence found that the former are closely related to the corresponding barometric variations, and represent that part of the grand monsoon system which affects Kurrachee, while the latter are found to be connected with a system of local convection currents, due to (relatively) local temperature variations. These latter, though subordinate to the former in point of magnitude, are still sufficiently large to mask the true nature of the regular monsoon currents which obey the barometric law. This is more especially the case in Bengal, where, as it appears both from evidence furnished in this paper and elsewhere, the activity of the monsoon currents is far less than on the west coast of India, while the absolute efficiency of the *local* variations is about the same.[3]

Another important result deduced, is that the causes which produce the abnormal variations in the wind and pressure components, are similar to those which produce the annual variations. Thus, when the barometer rises abnormally a tenth of an inch, it is accompanied by an abnormal wind of 4·4 miles per hour from N. 55° E., while a similar rise in the barometer from summer to winter gives rise to a wind of 4·7 miles per hour from N. 57° E.

This principle, which, though *a priori* probable, has not hitherto been supported by direct evidence, is without doubt destined to play an important part in the meteorology of the future, and to form one of the few channels by which we may hope to arrive at a correct knowledge of the effects of the suspected intrinsic variation of solar radiation on terrestrial meteorology. Thus Mr. Chambers says: "If the sun's heat is itself subject to fluctuations, either periodical or irregular, corresponding meteorological effects similar to those which are produced by the sun's change of position must result;" and he adds: "The relation at Kurrachee appears to be one of the kind that would be anticipated on the supposition of the sun's heat being variable, and in itself affords a reason for suspecting, if it does not tend to prove, such variability."

In discussing the diurnal variations, Mr. Chambers divides the winds into two great classes, *convection* or ordinary currents, in which the air moves from relatively cool to relatively warm regions, and *anti-convection* currents, or "winds of elastic expansion" as Blanford calls them, which blow outwards from regions of high temperature. Each of these classes is again divisible into two sub classes, (1) general and (2) local.[1]

If each of these systems is possible, as Mr. Chambers infer, the resultant variation is evidently a very complex one, and the main difficulty in discussing it, evidently consists in being able to adequately separate each component in turn from the rest. For this purpose Mr. Chambers employs Bessel's formula, and though he admits that the components derived by this method, do not necessarily represent physically distinct variations, its use in this case, as well as in others throughout this work, is attended with such favourable results, as to constitute a plea in favour of its more general adoption by English meteorologists.

To follow all the details of the investigation would be beyond our scope. It may therefore be briefly noted that the greater part of the variation of the north component, is due to the alternate land and sea breeze (convection currents), while a portion at any rate of the variation of the east component, is due to local anti-convection currents which prevail only in the drier months. Further, the direction of the local anti-convection currents varies with the varying position of the centre of maximum temperature range in the peninsula, while that of the coast convection currents is nearly constant.

By an ingenious plan for eliminating the variations due to coast convection currents, and by choosing the months so as to reduce the local anti-convection currents to a minimum, the existence is further proved of a system of general anti-convection currents, which, it may be remarked, were first noticed by Mr. Laughton in 1871, consisting of a double diurnal oscillation of the east component, which in the case of Kurrachee reaches its maxima at 10 a.m. and 9 p.m. and its minima at 4 p.m. and 2 a.m. respectively. These general anti-convection currents have been likewise proved by Mr. Chambers to exist at Calcutta, Belgaum, Bermuda, and Falmouth, *i.e.* in places where the ordinary convection currents differ completely both in character and intensity.

A comparison of the rainfall with the wind at the end of this paper leads to a conclusion similar to that drawn by Mr. Blanford, viz., that rain seldom falls as long as the summer monsoon continues to blow steadily, and Mr. Chambers hence infers, that a strong, damp wind from the seaward, is not the only condition required to produce rain. If this rule is only meant to apply to the place where the wind prevails, it is doubtless correct; but it seems open to misinterpretation if taken in a more general sense, since the laws of cyclonic systems and experience, both tell us that the reason why there is little rain on the coast when the sea wind is blowing strongly, is because the area of lowest pressure towards which the wind is spirally blowing is situated in the interior of the country, and that when there is *least* rain on the coast there is probably *most* inland.

Paper X. "Some Results of the Meteorological Observations taken at Allahabad during the Ten Years 1870–79," by S. A. Hill.—This paper, which represents the most complete discussion of the climatic elements at a single station in the interior of India that has ever been published, contains much that is valuable and highly suggestive to the physical meteorologist. To the climatologist it is especially interesting, owing to the inland as well as tropical position of the station. In May and June, Allahabad is one of the hottest places in India, the maximum temperature in the shade often rising above 115° Fahr., while in that terribly hot year, 1878, the temperature actually rose up to 119°·8 on June 19.

Nearly all the elements are discussed by the aid of Bessel's formula, and as it is a paper which cannot readily be reviewed in detail, we propose merely noticing one or two of the most salient conclusions deduced by the author.

One remarkable feature that comes out from the discussion of the diurnal barometric oscillation, is its "continental" character. Like Yarkand and other typically continental stations, the fall of the night tide is very small, and the ratio of the amplitude of the semi-diurnal to the diurnal component, is not only smaller than

[1] Continued from p. 430.

[2] The small elevation of the anemograph (only 15·6 feet above the ground) is open to some objection, but this is a good deal compensated for by its unusually free exposure.

[3] The resultant ranges of the wind variations obeying the barometric law are as follows:—

Kurrachee	26·6
Bombay	20·5
Calcutta	6·2

[1] These latter are dealt with in detail in those papers of Mr. Chambers's which have already been alluded to.

that at marine stations like Bombay, but reaches its minimum value in the hottest part of the year, when the ratio at the latter stations is rising towards its maximum.

When discussing the vapour tension, Prof. Hill remarks that, "while the diurnal variations of vapour tension and atmospheric pressure are connected with each other in so far as they are both effects of the diurnal inequality of temperature, it is doubtful whether there is any other connection between them except in an indirect way. At a dry station like Allahabad, where the range of the inequality of vapour tension is less than one-fourth of the range of pressure, it could never be supposed that the observed variation of the barometer is *caused* by the variation of the quantity of aqueous vapour in the air."

In explaining the afternoon minimum of vapour tension which is so distinctly marked at Allahabad during the dry hot months, a suggestion of Mr. Blanford's is noticed referring it to the semi-diurnal interchange between the lower and upper currents (which is supposed by Dr. Köppen to account for the diurnal increase in the velocity of the wind), supplemented perhaps by diffusion. The occurrence of a maximum of cloud nearly simultaneously, lends countenance to this view.

The clouds and rain are found to manifest similar diurnal variations, reaching their maxima nearly (1) when the temperature is lowest; and (2) when the vapour in the air reaches a maximum, either by diffusion from below or intermixture with the lower strata.

The heaviest fall recorded in one day during the ten years of observation was 15·48 inches between July 29 and 30, 1875, which has only been approached in the plains by the rainfall at Purneah in Bengal on September 13, 1879, and the rainfalls on September 17 and 18, 1880, when the disastrous land lip at Naini Tal took place. These abnormal falls are found to be due to the passage of small cyclones (secondaries, as they are generally termed in European weather bureaux) which strike the land on the coast of Orissa, and move northwards along a line separating the westerly winds of Southern India from the easterly winds of the northern plain—the axis as it were of the entire monsoon system.[1] The occurrence of the fall so far west as Naini Tal, together with its exceptional character in 1880, appears to the writer to have been due to the preponderance in that year of the eastern over the western monsoon system.

In regard to wind, Allahabad conforms to the general rule deduced by Dr. Hann for stations near sea-level in every part of the world, viz. that the velocity of the wind in every season is greatest about the hottest hour of the day.

The double diurnal rotation of the wind, exhibits a peculiarity which is of considerable interest in relation to Mr. Chambers's hypothesis of the connection between it and the diurnal variation of the barometric pressure. It is that in the dry hot months, the loop in the diagram representing the nocturnal variation is almost invisible, while in the rainy season it is much more pronounced, in correspondence with the nocturnal barometric tide which undergoes similar changes. In the marine climate of Bermuda, as Mr. Chambers has shown, both the nocturnal wind and barometer variations are nearly equal to those which occur during the day, and, in proportion as the climate of Allahabad becomes moister and therefore more maritime, so the variations in these elements appear to approximate in character to those at marine stations.

Paper XII. "The Meteorology of the North-West Himalaya," by S. A. Hill.—In this paper, which was originally compiled for a gazeteer and afterwards expanded, the author gives one of the most lucid and exhaustive accounts of the meteorology of a single district that we have ever had the good fortune to meet with. Not only is the region, one of peculiar interest, owing to the extraordinary facilities it presents for the observation of atmospheric changes, in vertical as well as horizontal range, but the manner in which the data are discussed is so eminently exhaustive, and withal attractive, that it virtually forms an almost complete epitome in miniature of meteorological science. In a preliminary description of the climate, and while noticing the great heat of the Punjab and North-West Provinces as compared with regions further south, Prof. Hill alludes to the investigations of Poisson, Meech, and Wiener, as showing that the total heating effect of the sun is a function of *the time during which he is above the horizon of a place*, as well as his altitude. The region where, according to their calculations, most heat falls from May 7 to August 7, lies about latitude 41°, and it is to this circumstance, together with the dryness of the air and absence of cloud, that Prof. Hill ascribes the excessively high temperature of June and July in the extreme north of the Punjab, and in the plains of Yarkand and Kashgar still farther north.

In fact (and this is a point which we think has been a good deal overlooked by climatologists) the annual range of temperature, is not merely dependent on the sea distance of a place, but also on its latitude; the further it is from the equator, *cæteris paribus*, the greater the amount of heat that falls in the summer months. On the other hand, since the summer season diminishes in length as the latitude increases, the region where the effect upon the temperature reaches its *maximum for the longest period* is probably about lat. 25½°, where the greatest amount of heat falls from equinox to equinox.

With respect to the other factor, humidity, the North-Western Himalaya are found to differ very markedly from what may be termed the South-Eastern Himalaya. Thus Darjeeling, though higher, has very nearly the same temperature in January as Simla, Chakrata, or Mussoorie, where the winter rains are more prevalent; while in May and June, owing to its coming in for a much more copious share of the summer monsoon, it is seven or eight degrees cooler.[1]

The vertical variation in the annual and diurnal ranges of temperature is found to be dependent chiefly on differences in the relative humidity of the air, the ranges being greater at the surface than at 6000 feet, where the lower cloud strata prevail, and greater again at the most elevated stations, where the radiation is excessive.

Another important element—the vertical decrement of temperature—is found to vary considerably in amount and rate at different seasons, being on the whole greater in the summer than in the winter up to 6000 feet. Above this height, especially in the inner ranges, the temperature diminishes very slowly, partly owing to the greater latitude, and partly to the absence of *cloud*, and it is to these circumstances, quite as much as to the small amount of precipitation, that the well-known fact of the snow-line being higher on the northern than on the southern side of the Himalaya, must be attributed. After working out the decrements in detail by the help of the method of least squares, Prof. Hill finds that on the mean of the year the temperature diminishes on the mean latitude 32°, at the rate of 2°·8 per 1000 feet, or 1° in every 357 feet of ascent. In the Eastern Himalaya, it is more rapid, being 1° for 320 feet.

It is interesting to observe, as Prof. Hill says, that, assuming the rate of decrement to be uniform over the southern slope of the North-West Himalaya, "a mean temperature of 50° Fahr., equal to that of London, would be attained at a height of 9600 feet, and the annual range of temperature would probably differ little from that observed in England. The hill sanitaria, at heights of 6000 to 7000 feet, possess climates comparable as regards temperature to those of Nice, Mentone, and other health resorts on the Riviera," only they appear to be somewhat superior to these in having a much smaller annual temperature range.

Prof. Hill calculates the height of the perpetual snow-line on the south slope of the North-West Himalaya to be 17,800 feet, which is a good deal higher than the measurements given hitherto. It seems probable, however, that a good many of these, by mistaking glaciers for snow, erred in making it too low. On the inner ranges bordering on Thibet, for reasons already noticed, the snow-line is about 2000 feet higher.

The diurnal variation of pressure—that hitherto unsolved problem for meteorologists—is discussed, though briefly, yet in a masterly manner, and the analogy of the mountain type by vertical exchange of air between their summits and the valleys, to the coast type caused by lateral exchange of air between the sea and the land, is noticed in connection with the corresponding system of mountain winds.

It is evident, from a perusal of this as well as other facts in connection with the diurnal range, that a complete explanation of it can only be attained by discussing data embracing a wide area, in order to eliminate all such local variations of the normal type.

The annual variation of pressure, is also very well described and explained, and we cordially commend it to the perusal of teachers of physical geography out of text-books, in which the old

[1] Mascart, in his "Météorologie appliquée à la Prévision du Temps," has noticed a similar tendency in European storms to move "vers la région des vents faibles."

[1] This explains why Darjeeling is such a healthy place for children, to whom a high summer temperature is so fatal.

stock notions of how the monsoons are caused in India are so prevalent. The explanations of many of the text-books are in fact a libel on the intelligence of both teacher and pupil.

It is usually said that the air is heated over the land expands, and rises (presumably in a courant ascendant). The air from the sea then rushes in to supply the tendency to vacuum, and this constitutes the monsoon.

The true state of the case is, however, quite different. The way in which the air is removed from North India is not by ascending, but by lateral currents which constitute the "hot winds." The vertical expansion by which a larger portion of the whole atmosphere is lifted above the level of the hill-stations would indeed rather tend to *raise* than *lower* the isobaric planes towards the north, and as no true "courant ascendant" can exist until the air is rendered moist by the monsoon rains, the lateral winds are the only means by which the isobaric planes are caused to slope northwards prior to their arrival.

The annual variation of pressure at the level of Leh, which is 11,538 feet above sea-level, shows us that at a still higher elevation the phases of annual barometric variation are exactly contrary to those which occur on the plains, the minimum occurring in mid-winter, and the maximum in midsummer.

It is further shown by Prof. Hill that the peculiar double oscillation at the hill-stations, which in correspondence with their position is intermediate in character to those in the two extreme cases, is due to exactly the same causes as the single oscillation on the plains—a fact which will prove of much utility in further research on this complicated question.

The winds for the most part correspond to the barometric variations. The constant south-westerly direction of the wind at the elevation of the hill-stations is, the result of two independent circumstances, viz. (1) the small depth of the winter (north-east) monsoon, above which the south-west anti-monsoon blows, and (2) the great height to which the summer (south-west) monsoon reaches. Above the latter monsoon it is not known how the wind blows, but in accordance with cyclonic laws it should be *north-west*. Perhaps future research will verify this inference.

The discussion of the humidity observations, leads to results which corroborate some previously obtained from somewhat meagre data by General Strachey. On the assumption that Hann's empirical formula with the value of the constant as given by Hill is correct, viz.—

$$\log p = \log P - \frac{h}{23058},$$ [1]

it is found that "at an elevation of 23,000 feet, or about the average height of the snowy peaks, the quantity of vapour in the air is only one-tenth of that at sea-level. *The extreme dryness of Thibet and Ladakh is thus easily accounted for.*"

The relative humidity depending on the temperature, obeys quite different laws, and undergoes variations very similar to those in the amount of cloud.

The average height at which cloud would be formed in the rainy season, is calculated by Prof. Hill to be about 4000 feet, and it is interesting to note that this elevation agrees with that of the zone on which the greatest amount of rain falls in the Himalaya, the exact height of which is found to be 4240 feet above sea-level. Above this height the rainfall decreases rapidly owing to the exhaustion of vapour, but in the case of the Himalaya this decrease is rendered more prominent owing to the outer ranges cutting off the supply of vapour to those more in the interior by promoting abnormal precipitation in their own vicinity.

E. DOUGLAS ARCHIBALD

[1] Where p P are the vapour tensions at the given elevation and sea level respectively, and h is the height in feet.

MULTIPLEX CAMERA BACK

THE great advance in tourist photography by reason of the production of the more sensitive and rapid gelatine dry plates now used in such large numbers has led to continued improvements in the construction of portable photographic apparatus.

Considerable difficulty has always been experienced in carrying a sufficient supply of sensitive plates for a day's tour.

To meet this want not only are large numbers of double backs carried but the changing box has also been devised. The latter consists of a cabinet arranged to carry twelve sensitive plates and a specially constructed dark back for the camera. When a plate is required to be changed, the dark back is attached to the changing cabinet, and by the action of springs and shutters a sensitive plate is transferred from the cabinet to the dark back, which is then removed and exposed in the camera as desired. The changing box is complicated and expensive, besides adding another piece of apparatus to the tourist's luggage. The greatest difficulty, however, arises from the very merits of the gelatine plates themselves.

They are so sensitive that the utmost care is required to keep every trace of light from the plate, and double backs that appear perfect to the eye, yet by the action of the sensitive plates themselves are found to be imperfect. It is obvious that the multiplication of double backs and the shutters forming part of them, adds to the liability of access of light and consequent fogging of plates. With the use of a changing box the same trouble is experienced, with occasionally further difficulties, caused by variations in thickness or sizes of sensitive plates, the latter sometimes refusing to pass from the changing box to the back or *vice versâ*, very often causing loss of time, temper, and plate as well.

We give illustration and description of an improved apparatus that, by its simplicity of action, appears to obviate the

difficulties before mentioned, and to possess merits of its own that will insure the success desired by the inventors. The apparatus combines in one cabinet the dark back and the changing box, and is the invention of Messrs. J. H. Hare and H. J. Dale.

The woodcut (which shows part of the outer cabinet cut away to give a view of the interior construction) will immediately explain its action.

The cabinet or multiplex back is made large enough to contain thirteen plates in two tiers, the lower tier containing seven and the upper tier six plates. The plates are secured in holders or carriers, with a thin metal back to each to prevent the light passing through the plate which may be exposed to those behind it. In the front of the cabinet is the usual sliding shutter, which draws up half way for exposure of the front plate of the lower tier.

At the back of the cabinet is a shutter which can be entirely removed when required to refill the back with plates. In the front shutter a small window of non-actinic glass is provided, through which the number of the sensitive plate ready to be exposed can be seen. In the back shutter two quick-running three-thread screws are provided, the lower one to bring the plates of the lower tier up to focus, and the

upper screw to tighten the upper tier of plates to prevent damage during travelling. The multiplex back fits into the camera in the usual manner. When the plate has been exposed, the shutter is closed, back removed from camera, and both screws at back unscrewed. Then the back is gently turned over; the first half turn causes the front plate of lower tier (just exposed) to pass into the upper tier, and then the second half-turn causes the back plate of upper tier to pass to the back of lower tier, while the second plate of lower tier has now come to the front, and is ready for exposure.

If any particular plate is required to be exposed, repeat the operation of revolving the box until the number of that particular plate is seen through the window in the shutter. An ivory tablet is provided on the side of the cabinet to register the numbers of the plates as exposed.

SOCIETIES AND ACADEMIES

Paris

Academy of Sciences, September 3.—M. Blanchard, president, in the chair.—The sitting was chiefly occupied with the reading of the report on the French mission to the Pacific to observe the total eclipse of the sun on May 6, 1883. The report was prepared and read by M. J. Janssen, head of the mission, to which, besides M. Trouvelot, of the Meudon Observatory, and M. Pasteur, photographer, were also attached MM. Tacchini, director of the Roman Observatory, and Palisa, of the Vienna Observatory. The station selected was Caroline Island, in 10° S. and 152° 20′ W., about 200 leagues north of Tahiti, a point lying very nearly within the zone of total obscuration. In the report are summed up the results of all the observations, which aimed especially at the solution of certain questions touching the constitution of the sun and the existence of the so-called intra-Mercurial planets. As regards contact the commencement of total obscuration was determined at 23h. 31m. 51·8s. mean time at Caroline Island; end of same 23h. 37m. 15·8s., leaving a difference of 5m. 24·1s. as the actual duration of totality according to M. Trouvelot. M. Tacchini gave 5m. 23s., or a difference of slightly over one second, which was considered as so far satisfactory. M. Tacchini also made some remarkable observations, especially touching a certain analogy between the constitution of the spectrum of certain parts of the corona and that of comets. In his attempt to ascertain whether the light of the corona contains any large proportion of solar light, M. Janssen succeeded beyond his expectations. The complete Fraunhofer spectrum seen by him shows that, apart from what may be due to diffraction, there exists in the corona, and especially in certain parts of it, an enormous mass of reflected light. And as the coronal atmosphere is known to be extremely attenuated, such an abundance of reflected solar light can be explained only by the presence in these regions of cosmic matter in the form of solid corpuscules. The photographs of the corona yielded several interesting phenomena, which are reserved for future study. For the present it will suffice to remark that these photographs show a more extended corona than that obtained from telescopic observation. The phenomenon also appeared limited and fixed during the period of total obscuration. A photometric measurement of the luminous intensity of the corona, which M. Janssen had prepared by means of photography, showed that in Caroline the luminosity of the corona was greater than that of the full moon. This is the first time that a precise calculation has been made of this phenomenon. On May 13 the mission re-embarked on board the *Éclaireur*, and on the home voyage visited Hawaii during the volcanic disturbances in the crater of Kilauea. M. Janssen took this opportunity of making a spectrum analysis of the flames emitted by the molten lavas, and was able to determine the presence of sodium, hydrogen, and carburetted combinations.—On the antiseptic frigidity of sores, by M. Gosselin.—Note by M. J. Delauney on the indications some years ago formulated by him on the probable epochs of great earthquakes. In a note inserted in the *Comptes Rendus* for November 17, 1879, the author considered it probable that the influence of Jupiter and Saturn on seismic disturbances is due to the passage of these planets through meteoric bodies situated in the mean longitudes of 135° and 265°. In the approximate table of future earthquakes accompanying the note, the year 1883 was not mentioned. But in another note inserted in *La Nature* for October 23, 1880, a fresh calculation of probable epochs of seismic agitation, brought down to the year 1920, mention is made of the date 1883-85, when disturbances might be expected owing to the transit of Jupiter through the August meteors.—Observations of the new planet (234) made at the Paris Observatory (equatorial of the west tower), by M. G. Bigourdan.—On the affinities of the eocene floras of England and the west of France, by M. L. Crié.—Fresh remarks on the *Phylloglossum Drummondii* (Kunze), by M. C. Eg. Bertrand.—On a process for extracting alcohol by means of lemon juice, by M. Levat.—On the fermentation of bread-stuffs, by M. G. Chicandard.

Vienna

Imperial Academy of Sciences, July 12.—D. Stur, on the morphology and systematics of culmian and carbon fauna.—H. Jahn, electrolytic studies (preliminary note).—A. Adamkiewicz, on the theory of brain pressure and on the pathology of brain compression (part ii.)—Th. von Oppolzer, communication on a series of observations (just completed) for the absolute determination of gravity at Vienna.—W. Fossek, on a derivative of isobutyraldehyde analogous to hydrobenzoin.—On the preparation of isobutyraldehyde free from acetone, by the same.—H. Molisch, researches on hydrotropism.

July 19.—C. von Ettingshausen, on the Tertiary flora of Japan.—F. Brauer, on two parasites of the June beetle (*Rhizostrogus solstitialis*): 1, *Hirmoneura obscura*, Mg.; 2, *Phorostoma lata*, Egg.—B. Mandelstamm, studies on innervation and atrophy of the laryngeal muscles.—T. Korteweg, on the question whether the variations in the length and height of the singular periods of frequency of sun-spots were produced by the interference of two periods of unequal but invariable length and height.—V. Hausmanninger, new observations on the impact of cylindrical caoutchouc rods.—M. Loewit, on the formation of white and red blood-corpuscles.—L. von Barth and H. Weidel, on the oxidation of morphine.—G. Goldschmidt, on papaverine.—J. Habermann, on some basic sulphates.—On arbutin, by the same.—M. Hoenig and E. Zatzek, on the direct estimation of carbonic acid in presence of sulphides, sulphites, and thiosulphates.—On the action of permanganate of potassium on some sulphur compounds, by the same.—A. Waage, on the action of ammonia on propionaldehyde.—E. Lippmann and F. Fleissner, contribution to a knowledge of azylines.

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THURSDAY, SEPTEMBER 20, 1883

SCIENCE WORTHIES

XXII.—ARTHUR CAYLEY

IT is natural that the public in general should wish to know something of the life and work of one whom the British Association for the Advancement of Science has honoured by placing him this year at its head, an honour indeed which could not much longer have been withheld, considering the foremost place which our new President occupies among English mathematicians. But when asked to tell the story I am tempted to exclaim with the needy knifegrinder—

"Story, God bless you, there is none to tell, Sir."

The quiet life of a student is not likely to be rich in sensational incidents, and of the nature of the work done by a labourer in the field of pure mathematics it is not possible to give more than a vague idea to the outside world. Some slight sketch I must attempt to give, and in doing so I must express my obligations to Mr. J. W. L. Glaisher, without the help of whose greater knowledge of Cambridge matters and of the recent progress of mathematics I could not have undertaken this task.

Arthur Cayley was born August 16, 1821. His father, a grandson of Cornelius Cayley—who was Recorder of Kingston-on-Hull from 1725 to 1771—was settled at St. Petersburg as partner in the firm of Russian merchants—Thornton, Melville, and Cayley. It was during a short visit of his parents to England that their second son, Arthur, was born at Richmond, Surrey. An elder brother had died in infancy; a younger brother has since become well known as an Italian scholar and a translator of Dante. In 1829 the family returned permanently to England, and after a while fixed their residence at Blackheath. At a very early age Arthur gave the usual indication by which mathematical ability is wont first to show itself, namely, great liking and aptitude f r arithmetical calculations. A lady, who was one of his first instructors, has told that he used to ask for sums in Long Division to do while the other little boys were at play. After four years' teaching at a private school at Blackheath he was sent at the age of fourteen to King's College School, London, the principal of which (Hugh Rose), being struck by the indications of mathematical genius which he gave, prevailed on his father to abandon his intention of bringing the boy up to his own business and induced him to send him instead to Cambridge, where he entered Trinity College at the rather unusually early age of seventeen. At his college examinations Cayley was first by an enormous interval; but it was fortunate for him that the wares in which he dealt were those which fetched the highest price; for, if classics had been given the preference over mathematics instead of *vice versâ*, he had in his class at Trinity College two most formidable competitors, namely, Mr. Munro, the well known scholar and editor of Lucretius, and Mr. Justice Denman, who afterwards came out as Senior Classic at the same time that Cayley came out as Senior Wrangler and first Smith's Prizeman.

This was in 1842. In University as in other harvests, there sometimes comes a run of unusually good years, and this certainly appears to have been the case at the period in question. The Senior Wrangler in 1840 was Leslie Ellis, in 1841 Stokes, in 1842 Cayley, in 1843 Adams; the last three of whom have, for now over twenty years, given lustre to the Cambridge mathematical school, of which they have formed part of the working staff. I do not know whether Cayley's success at the Tripos Examination was as little a surprise to himself as it was to others. Stories were current in Cambridge at the time of the equanimity with which he received the news of his success. The best authenticated one is that he was on the top of the coach on a night journey from London to Cambridge when the tripos list was put into his hands; he quietly put it into his pocket, resigning himself very contentedly to the necessity of waiting till the morning light for a knowledge of its contents. Cayley's name cannot be added to the list of those who have combined distinction in the boats or on the cricket field with high University honours. He was, however, an active pedestrian, and was a member of the Alpine Club in its comparatively early days.

While still an undergraduate, Cayley commenced his career of mathematical publications by a paper in the *Cambridge Mathematical Journal* for 1841. This periodical had been founded a little time before by Leslie Ellis, who has been just mentioned, in conjunction with his friend, Mr. Gregory, who thereby rendered a service to English mathematics that it would be difficult to estimate. One who devotes himself to original mathematical research must make up his mind to forego the pecuniary rewards which attend other forms of successful literary labour. The public which he addresses is so limited that, instead of expecting to be paid for what he writes, he has to think how he can give it to the world without too severe pecuniary loss. If it were not for the help given by learned societies and by mathematical periodicals, every mathematician who was not rich would be forced to keep his discoveries to himself, and on such terms few would have spirit to persevere in research. At the time of which I speak mathematical periodicals open to young students scarcely existed, so that to young mathematicians doubtful of the value of their own speculations, and whose modesty would hardly permit them to ask for publication from the Royal Society, an immense stimulus was given by the foundation of the periodical just mentioned, the *Cambridge Mathematical Journal*, afterwards continued under the names of the *Cambridge and Dublin Mathematical Journal* and the *Quarterly Journal of Mathematics*. This journal roused the energies of the younger members of the University by making known to them that others of no higher standing than themselves were engaged in original research and by promising them the means of publishing whatever they might discover; and certainly it is no small thing that it can boast to have given Cayley his first opportunity of coming before the world.

His prodigious activity however could not long be content with a single outlet, and there were few organs of mathematical publication at home or abroad which did not receive communications from him. If his memoirs were now collected, they would form a mass exhibiting a spectacle of enormous literary industry. It appears,

however, not to have been until 1852 that he addressed a memoir to the Royal Society, of which he was elected a Fellow in the same year.

His mathematical activity during this period was the more surprising, as he was able to devote to these studies only a limited portion of his time. He had been elected a Fellow of Trinity College in 1842; but as he was not willing to take Holy Orders, this was but a temporary provision, for he could only hold his Fellowship for seven years after his Master's degree. It became necessary for him therefore to look out for some profession more remunerative than mathematics, and very soon after taking his Master's degree he became a pupil of the eminent conveyancer, Mr. Christie. It is said that when offering himself as a pupil he modestly suppressed all mention of his antecedents, and that Mr. Christie was much surprised to find out on cross-examining him that he had to do with a Senior Wrangler and Fellow of Trinity. However this may be, he soon became Mr. Christie's favourite pupil, as indeed was not wonderful in the case of one who possessed a very clear head, immense capacity for work, and the power of throwing his whole mind into the work on which he was at the time engaged. After he was called to the bar he never had occasion to look elsewhere for business, for Mr. Christie was always glad to supply him with as much conveyancing work as he was willing to undertake. I have been told that some of his drafts were made to serve as models for students. But nothing that her wealthy rival had to offer could seduce Cayley into unfaithfulness to his first love, Mathematics. For Mathematics he always jealously reserved a due portion of time free from the encroachments of his business relations with Law, and it was during the time of his legal practice that some of his most brilliant mathematical discoveries were made. At last he obtained release from the embarrassment of a divided allegiance. By placing Lady Sadler's trusts on a new footing and founding the Sadlerian Professorship, his University was able to invite him to return, and he gladly accepted what was at the time a very modest provision, but which would enable him to give his whole time to the pursuits most congenial to him. Some time after his return to Cambridge his pecuniary position was improved. His College, which on his return had speedily made him an honorary Fellow, after a time reelected him to a foundation Fellowship, necessarily a very rare distinction, since the reelection of an ex-Fellow involves the exclusion of the claims of a younger candidate. Later still, in the course of University legislation about Professorships, the position of the Sadlerian Professorship was improved. But these things could not have been foreseen at the time that Cayley accepted the office.

It was in 1863 that, after fourteen years of chamber life in Lincoln's Inn, he married and settled permanently in Cambridge. He never would own to any regret when his friends spoke to him of the prospects of professional advancement which he sacrificed by not remaining at the bar. He knew what mode of life would best promote his own happiness, and he had strength of mind to follow it without troubling his head about the riches or honours a different course might bring. His mathematical work gave him pleasure which he never found in law; and in his hatred of unnecessary words he was once wicked enough to say that the object of law was to say a thing in the greatest number of words, and of mathematics to say it in the fewest. But, jesting apart, the University had no reason to regret the legal training and knowledge which he had acquired during his absence from it. It has much added to his usefulness as a member of the Council of the Senate, where his opinion has carried the greatest weight, and it has enabled him to be particularly useful both to his College and to the University in the drafting of new statutes and in the necessary preliminary deliberations. At the last contested Parliamentary election Cayley presided at one of the three polling places, and gave universal satisfaction, hearing patiently the arguments on both sides on all disputed points, and then promptly making a decision in a few words in such a way as to inspire general confidence.

But after all it is as a mathematical professor that Cayley is eminently "the right man in the right place." No one could be better fitted to discharge the duties prescribed for the Sadlerian Professor, "to explain and teach the principles of pure mathematics, and to apply himself to the advancement of the science." It is seldom that one man so well combines the two qualifications here indicated, viz. power to teach what is known already, and ability to extend the boundaries of knowledge. It constantly happens that men of great originality of genius find it irksome to study what has been done by others. And now every department of science has so enlarged its borders that it has not only become impossible for one man to master the whole circle of the sciences; but even a single department, such as pure mathematics, includes under it so great a variety of subjects that most men are content to be specialists, and, devoting themselves to their favourite topic, are satisfied with a very superficial knowledge of other branches. Cayley is quite as distinguished for the amount and universality of his reading as for his power of original work, and may fairly count as the most learned mathematician of the present day. I suppose that, if all European mathematicians could be subjected to a tripos competition, no matter who might come out first on the "problem" papers, Cayley would be far ahead in the "book work." And his tastes are so catholic that no form of mathematics comes amiss to him. I remember how we in Dublin were struck by his proficiency in pure geometry, a subject then much cultivated with us, but which we had been accustomed to look on as too little esteemed at Cambridge.

This wideness of knowledge has made Cayley invaluable as a mathematical referee. To several scientific societies (the Royal Society, the Mathematical Society, the Royal Astronomical Society, the Cambridge Philosophical Society) he has long been a principal adviser as to the merits of mathematical papers presented for publication, no one being more willing to take the trouble of examining such papers, or being better able to pronounce how much of their contents is new or important. And no one could be more ready and obliging with his advice to private students who have desired to interest him in their investigations, and to be assured by him that no unscrupulous predecessor has plagiarised their discoveries. Repeatedly have foreign mathematicians expressed their surprise at the rapidity with which he has dealt with such inquiries, an answer commonly coming by return of post, probably

giving a new proof of some of the results, or pointing out that some of them were capable of greater generalisation. By his services in this way he has made himself so widely popular that if European mathematicians had to elect themselves a head I could not name any one likely to have a larger number of votes.

With respect to Cayley as an original inquirer, his special merit has in my opinion been truly seized by Mr. Glaisher, who has described him as the greatest living master of algebra. While, as I have said, no part of mathematics comes amiss to him, he is always happiest when he can translate his theorems into pure algebra and show that a proposed result is but the expression of an algebraical fact. In this respect he differed from H. J. Smith, by whose recent loss English mathematics has so terribly suffered, who was entirely arithmetical in his thoughts and work.

Mathematicians, like chess-players, may be divided into the book-learned and the original, the highest amount of excellence being attained by those who combine great knowledge of books with the power to strike into new paths of their own. Of this I have spoken already. But there is another division of chess-players, the solid and the brilliant, some being full of ingenious devices which, however, will not bear a careful examination; others being quite free from mistake but wooden in their style. Cayley combines the excellences of the two kinds in a very high degree, though his merits in the one respect appear to me to be more marked than in the other. Men weak in power of calculation have often exhibited beautiful exercises of ingenuity in their attempts to arrive at results by some shorter process. Such a master of algebra in all its forms as Cayley was not to be dismayed by any amount of calculation, and he therefore has been able to trample down many a difficulty which an inferior in this respect might have evaded by some ingenious oblique method.

As Cayley is not afraid of hard work himself, so it is necessary for the readers of his papers not to be easily discouraged by formidable calculations. But in my opinion it is not this so much that makes Cayley's papers difficult to read as the fact that he usually proceeds by the synthetic, not the analytic, method. It usually happens that a mathematical inquirer begins by proposing to himself some comparatively simple question. By the time he has found the answer to it, the subject opens on him; the first question suggests others, the theorem first discovered is found to admit of wide generalisations, and perhaps it may be found that these could have been arrived at in quite another way. When the time comes for the inquirer to publish his results to the world, the most attractive course is to take his readers by exactly the same road he has travelled himself, beginning with the simple problem which first attracted attention, and leading on step by step to the highest results arrived at. Cayley on the contrary usually begins by trying to establish at once the highest generalisation he has reached, writing down equations and proceeding to make calculations as to the good of which he has not taken his readers into his confidence. The consequence is that few master his papers but those who have found a clue to them by some previous work in the same direction.

I fancy that the difficulty of Cayley's papers is to be accounted for by his having had comparatively little experience in teaching mathematics until rather late in life, and then only to students of the highest order. He lectured for a few years at Trinity after taking his degree, but I dare say that he did wisely in going to the bar instead of making a livelihood by mathematical teaching at Cambridge, for one who loved mathematics so much for its own sake, would hardly sympathise with the many whose only object in coming to him would be to learn how they could successfully get through an examination. On his return to Cambridge he possibly would have extended his influence more widely if he had taken what may seem the lazier course of giving the same series of lectures year after year. But Cayley preferred to give his classes his latest and highest work, and each year has taken for his subject that of the memoir on which he was for the time engaged. The result has been that he has been brought little in contact with any but the most advanced students, who alone could profit by such instruction, nor even they, indeed, unless they were as high-minded as himself, and were content to spend a great amount of time and labour on work that could not "pay" at the great University examination.

As I have spoken of Cayley's lectures I ought not to omit to mention the honour done him by the heads of the Johns Hopkins University of Baltimore, Maryland, an institution which numbers among its professors, as head of its mathematical department, Cayley's distinguished friend and fellow worker, Sylvester. They invited Cayley to go over to lecture at Baltimore in the winter session of 1882. He accepted a proposal in every way so flattering, and lectured at Baltimore in the months of January to May, 1882, returning to England in June. His subject was Elliptic and Abelian functions, and his lectures, in which he considered from an algebraic point of view the geometrical theories of Clebsch and Gordan, were given for publication to the *American Journal of Mathematics*, and are likely to form a classic memoir on the subject.

As I have said so much of Cayley's mathematical labours, it will probably be expected that I should speak a little less vaguely, and endeavour to explain more particularly the nature and progress of his discoveries; yet it is not easy to make the history of discovery in the higher branches of pure mathematics readable even for so select a class as the subscribers to NATURE. It requires but a small stock of technical knowledge to enable a reader to follow with interest a history of mechanical inventions, or of discoveries admitting of useful practical applications, or of the skilled organisation of labour; but what is to be said of the work done by a solitary student in his closet, the result of which will not so much as cheapen one yard of calico?

It would be out of place if I were to take trouble here to show that pure mathematics have after all added much to the material wealth of the world. My subject is the life of a great artist who has had courage to despise the allurements of avarice or ambition, and has found more happiness from a life devoted to the contemplation of beauty and truth than if he had striven to make himself richer, or otherwise push himself on in the world. We do not classify painters according to the numbers capable of appreciating their respective productions. On the contrary, we can understand that it is often the lowest

tyle of art which will attract round it the largest circle of admirers. So the fact that it is a very limited circle which is capable of appreciating the beauty of the work done by a great mathematician should not prevent men from understanding that it is like the work done by a poet or a painter, work done entirely for its own sake, and capable of affording lively pleasure both to the worker himself and his admirers, without any thought of material benefit to be derived from it.

But in point of fact mathematics stand midway between the arts which minister to man's sense of beauty and those which supply his material comforts. The name "pure mathematics" suggests that there is such a thing as "applied mathematics," and it is well known that the mathematician furnishes the instruments employed by cultivators of sciences whose practical utility is beyond dispute. If the mathematician did no more than manufacture such instruments precisely as the demand arose for them, his might count as one of the arts which are valued only for their practical utility. But actually the invention of the mathematical instruments usually comes first, and the use to be made of them is found out afterwards. The stock example of the kind is the debt which physical astronomy owes to the labours of the early geometers on the theory of conic sections, a theory cultivated without any suspicion that it could be turned to practical account. Yet it was because Newton was in his day the greatest master of this as of every other branch of pure mathematics that he was able to bring all the motions of the heavenly bodies under the dominion of mathematical calculation, and to convert the moon into a timepiece by which the mariner can ascertain his position on the seas. With the advance of physical science greater refinement and power in the mathematical instruments of investigation have become necessary; but pure mathematicians have ever outrun the demands of the practical workers, for instrument-making has delights of its own. The late Lord Rosse I have no doubt found more pleasure in devising the innumerable ingenious and beautiful contrivances necessary for the manufacture of his huge telescope than he ever did from observing with it after it was made. It is impossible for any one now to say what advantages future investigators will derive from the perfection to which the mathematical instruments have been brought by the labours of such men as Cayley, who have invented mathematical steam hammers by which ponderous masses of formulæ can be manipulated with ease and calculations made simple which in former times were looked on as impracticable.

There is hardly anything that comes under the head of pure mathematics at which Cayley has not worked, but it will be enough if I try to say something as to that by which his name is likely to be best remembered—his creation of an entirely new branch of mathematics by his discovery of the theory of invariants, which has given quite a new aspect to several departments of mathematics. It has introduced such a host of new ideas, and consequently of new words, that a Senior Wrangler of forty years ago, who had not kept pace with modern investigations, would find, on taking up a book of the present day on geometry or algebra, that he could not read it without a glossary, and must go to school again to learn what the writer was speaking of. It would be out of place if I were to enter into a very long technical exposition here, but it is possible, without assuming in the reader more than a moderate knowledge of analytic geometry, to make him at least understand what the word "invariant" means. Suppose that we have written down the general equation of a curve of any degree, and also have found the relation that must subsist between the coefficients in order that the curve should assume some special form. For simplicity I suppose the equation to be of the second degree, and I take the well known relation between the coefficients which is satisfied when the curve represented reduces itself to two right lines. Now imagine the equation to be transformed to any new coordinates whatever, this can make no change in the form of the curve represented. If the relation in question were satisfied by the coefficients of the original equation, it must also be satisfied by the coefficients of the transformed equation. But by actually performing the transformation we can express these new coefficients in terms of the old ones and of the constants introduced in the process of transformation. The expression will be complicated enough, and that of the relation of which I am speaking still more so. But since the relation must vanish whenever the corresponding relation expressed in terms of the old coefficients vanishes, the one must contain the other as a factor. The remaining factor, it will be seen on examination, contains nothing but the constants introduced by transformation. All this can be verified by actual work; but the result which I have stated can be foreseen without any calculation.

The principle which I have described has proved to be very fertile in applications. The late Dr. Boole made, in 1841, some interesting use of a simple case of the same principle. But it was Cayley who set himself the problem to determine *a priori* what functions of the coefficients of a given equation possess this property of *invariance*, viz., that when the equation is linearly transformed the same function of the new coefficients is equal to the given function multiplied by a quantity independent of the coefficients. The result of his investigations was to bring to light a number of important functions (some of them involving the variables as well as the coefficients) whose relations to the given equation are unaffected by linear transformation. And the effect has been that the knowledge which mathematicians now possess of the structure of algebraic forms is as different from what it was before Cayley's time as the knowledge of the human body possessed by one who has dissected it and knows its internal structure is different from that of one who has only seen it from the outside.

In an age when the work of mathematical research is so actively carried on, whenever one worker finds a nugget there is an immediate rush to the spot of other searchers. In the present case Cayley's friend Sylvester was one of the first on the spot, and both being resident in London were able by frequent oral communication to stimulate each other's ideas. As I am not relating the history of mathematical science, I need not name the foreign mathematicians who rapidly came in to labour in the same field; but it is agreed on all hands that it was Cayley who both discovered the "diggins" and got out some of the biggest nuggets. It is not always the case that the history of a mathematical discovery has not to

tell of some contests for priority. All pure mathematics consists in the drawing out of ideas latent in admitted principles, and it is a curious fact how men will fail to draw the consequences which to another will appear irresistibly suggested by something they have themselves asserted, and consequently how near they will come to the brink of a discovery without actually making it. And controversies as to mathematical priority naturally arise because it seems so cruel to the man who has taken all the steps except the very last, that another should step in and get the credit of the discovery, when it seems to him that he himself had done all the difficult part of the work and the other only drawn an inference so simple that no credit should be given to any one for making it. If no controversy of the kind has arisen in the present case, perhaps the cause is not exclusively the indisputable character of Cayley's claims, but something is also due to the moral nature of the man. His motto has always been "esse quam videri," and I do not know any one to whom it would be more repulsive to engage in a personal contest by claiming for himself a particle of honour or of money more than was spontaneously conceded. He would be apt to take for his model the patriarch Isaac, who, when the Philistines claimed a well which he had dug, went on and dug another, and when they claimed that too, went on and dug a third.

The place of a more minute account of his mathematical discoveries may be supplied by a mention of the wide recognition which his labours have received. He was given the honorary degrees of D.C.L. Oxford, 1864, LL.D. Dublin, 1865, and was elected Fellow or Correspondent of the following Societies:—Philosophical Society, Manchester, 1859; French Institute, 1863; Royal Societies, Edinburgh and Berlin, 1865; Boston, 1866; appointed a Member of the Board of Visitors, Greenwich Observatory, 1866; Milan, 1868; St. Petersburg and Göttingen, 1871; Royal Irish Academy, 1873; Upsala, Leyden, and Rome, 1875; Hungary, 1881; Sweden, 1882. I should add that the Royal Society awarded him a Royal Medal in 1859, and last year (1882) the Copley Medal; the latter a distinction seldom conferred on a pure mathematician.

Though his principal interests are mathematical, they are far from being exclusively so. He is a good linguist, and, as was said of Moltke, there are few European languages in which he does not know how to hold his tongue. He is chairman of the Association for Promoting the Higher Education of Women. When seats in the University Council are contested, his name always appears on both the rival lists. By all who know him he is as much respected as a high-minded man as he is admired as a mathematician. GEORGE SALMON

BENTHAM AND HOOKER'S "GENERA PLANTARUM"

Genera Plantarum ad exemplaria imprimis in herbariis Kewensibus servata definita. By G. Bentham and J. D. Hooker. 3 vols. (London, 1862-1883.)

THE completion of the "Genera Plantarum" of Messrs. Bentham and Hooker, an event long impatiently desired by all botanists, has been recently effected by the publication of the second and concluding part of the third volume. This great work has required more than five-and-twenty years of assiduous labour, during which the authors have devoted themselves to their formidable task with untiring perseverance, and with a degree of unity both in the plan and the execution of the work which would have been impossible but for their constant daily intercourse, and their relations of intimate personal friendship.

Before undertaking the publication of the "Genera" its authors had already given to the world important works which had placed them in the foremost rank as botanists, and both were familiarly acquainted with the scientific wealth accumulated in the museums and gardens at Kew. Mr. Bentham, whose botanical collections were united to those of the Royal Herbarium as long as thirty-six years ago, had already in connection with his various works and memoirs had occasion to study nearly the entire vegetable kingdom; while Sir Joseph Hooker, in addition to an equally wide range of study, had the inestimable advantage of having during his extensive travels been able to observe in the living state numerous species of many genera characteristic of the tropical and antarctic regions, and of having fixed their analytical characters by sketches and diagrams of singular elegance and accuracy.

With a rare amount of abnegation of personal feeling the authors of this work were content to let it go forth under their joint names, without in any way indicating the separate share contributed by each of them, desiring, as it would appear, that it should be regarded as the collective result of their joint labours—the product of two minds working harmoniously for a common object. Only very recently, under the pressure of urgent requests from many different quarters, Mr. Bentham consented, in a short note communicated to the Linnean Society,[1] to explain in a summary way the share contributed by each of the authors. This is of so much interest to botanists that the present writer does not hesitate to give here the substance of Mr. Bentham's note.

The *Polypetalæ*, which fill the first volume, were pretty equally divided. While Mr. Bentham was engaged on the earlier orders, Sir J. Hooker undertook the *Cruciferæ*, *Capparideæ*, and *Resedaceæ;* and to his share also fell most of the numerous families of the *Disciflorœ,* while Mr. Bentham elaborated the remaining families of the *Thalamifloræ*, along with the *Lineæ*, *Humiriaceæ*, *Geraniaceæ*, and *Olacineæ*. Of the group of the *Calycifloræ* it was natural that Mr. Bentham should undertake the *Leguminosæ*, which he had already illustrated by a series of important memoirs, and to him also fell the *Myrtaceæ*, *Umbelliferæ*, and *Araliaceæ*. The remaining families of this group, including the *Rosaceæ*, *Saxifrageæ*, *Melastomaceæ*, and *Cucurbitaceæ*, besides many others less important, were assigned to Sir J. Hooker.

The first portion of the second volume is almost entirely occupied by the two great families of *Rubiaceæ* and *Compositæ*. To the former of these Sir J. Hooker devoted two years of constant study which involved very numerous dissections of a difficult nature, and he also elaborated the *Caprifoliaceæ*. During the same period Mr. Bentham was mainly occupied with the vast family of *Compositæ*, comprising nearly 800 genera, and not much

[1] "On the joint and separate work of the authors of Bentham and Hooker's 'Genera Plantarum.'" *Journal of the Linnean Society*—Botany, vol. xx. pp. 304-308.

fewer than 10,000 species. To assign definite generic characters to a series of forms so closely allied was an undertaking which, in spite of the previous labours of many eminent botanists, required the most careful examination of an almost overwhelming mass of materials, along with the severest critical acumen. The second portion of the second volume includes the great mass of the Gamopetalous families. At this period the pressure of official duties, and those devolving upon him as President of the Royal Society, prevented Sir J. Hooker from devoting much of his time to the laborious tasks of critical systematic botany; and to this portion of the work he contributed only the allied families of the *Vacciniaceæ*, *Ericaceæ*, and *Epacrideæ*, in addition to the *Myrsineæ*, *Primulaceæ*, and a part of the *Sapotaceæ*. On Mr. Bentham devolved all the remaining families of this vast group; and to show the prodigious amount of labour accomplished by this remarkable man, it is sufficient to say that, along with minor families, these included the *Apocyneæ*, *Asclepiadeæ*, *Gentianeæ*, *Boragineæ*, *Convolvulaceæ*, *Solaneæ*, *Scrophularineæ*, *Gesneriaceæ*, *Bignoniaceæ*, *Verbenaceæ*, and, finally, the *Labiatæ*. Some additional years might have been requisite for such an undertaking if his classical monographs on the two great families *Scrophularineæ* and *Labiatæ* had not supplied Mr. Bentham with the materials for his subsequent work.

The first part of the third volume is occupied by the *Monochlamydeæ* and the Gymnosperms. To this part the group of the *Curviembryeæ*, including the important families *Amarantaceæ* and *Chenopodiaceæ*, was contributed by Sir J. Hooker, who further undertook the *Nepenthaceæ*, *Cytinaceæ*, and *Balanophoreæ*. The materials for the latter were ready to hand, being for the most part contained in the remarkable monographs long since published by himself. The remaining families of *Monochlamydeæ* were elaborated by Mr. Bentham. Amongst the more important must be mentioned the *Laurineæ*, *Proteaceæ*, *Thymelæaceæ*, and *Santalaceæ*. But it was especially the great families *Euphorbiaceæ* and *Urticaceæ* which, in spite of recent monographs, demanded a vast amount of minute examination and careful revision of all existing sources of information. The Gymnosperms had originally been undertaken by Sir J. Hooker, who possesses so wide an acquaintance with these plants in the living state; but the pressure of other occupations again interfered, and this group was also executed by Mr. Bentham, doubtful questions here as well as throughout the entire work being reserved for discussion between the joint authors.

The second part of the third volume, which concludes the work, contains all the families of Monocotyledonous plants. The examination and revision of the vast store of existing materials appeared to the authors such a formidable task that, in the doubt whether they should be able to complete it, they resolved to attack in the first instance the most difficult families, Sir J. Hooker undertaking the Palms, and Mr. Bentham the *Orchideæ*. As is well known, the study of these families offers peculiar difficulties. In the former the great size of all the parts, as well as their texture, usually makes it impossible to preserve herbarium specimens available for study, and much restricts the supply of materials to be found even in the best-furnished museums. Notwithstanding his very extensive previous knowledge of this family, and the exceptional resources available at Kew, Sir J. Hooker found the task to involve a much greater expenditure of time and labour than he had anticipated, chiefly owing to the necessity for a very extensive correspondence with botanists in various parts of the world who were able to supply special information or materials not otherwise obtainable. Along with other special difficulties, the study of the vast family of the *Orchideæ* is hampered by the unsatisfactory condition of a great proportion of the specimens sent to Europe from countries whose climate makes their preparation and preservation almost unmanageable. It is not surprising that Mr. Bentham found more than a year of unbroken persistent labour no more than sufficient for this family, and that he subsequently required an equally long period in dealing with the *Gramineæ*.

In treating the remaining Monocotyledonous families, the task of the authors was in many cases lightened, though not by any means replaced, by the work of various recent monographers. Sir J. Hooker disposed of the group of *Nudifloræ* (*Aroideæ* and allied families) and that of the *Apocarpeæ*, including the *Triurideæ*, *Alismaceæ*, and *Najadeæ*. To Mr. Bentham fell the heavy task of completing the work by the examination of the numerous remaining families of Monocotyledons, among which may be specified the *Bromeliaceæ*, *Irideæ*, *Amaryllideæ*, *Liliaceæ*, *Commelynaceæ*, *Pandaneæ*, *Restiaceæ*, and *Cyperaceæ*. It is a surprising proof of exceptional mental and bodily activity that in dealing with this portion of the work, and in studying natural families where the floral parts are too often lost or obliterated in dried specimens, and therefore demand the most delicate and careful dissection, Mr. Bentham, in spite of his advanced age, revised and defined in the course of three years more than 1200 genera.

Throughout the progress of the work, as well as in determining its original plan and arrangement, every important question was decided after joint consideration and discussion. In this way the limits and characters of the larger groups, the descriptions of the natural families, their subdivision into suborders and tribes, and the arrangement of genera, were settled by mutual interchange of views. Almost invariably the work of each author was read and criticised by the other before it was sent to press, and the proofs were regularly corrected by both, so as to eliminate as far as possible any chance of divergence of opinion; and, finally, they were fortunate enough to obtain the help of a highly competent friend, the Rev. M. J. Berkeley, who undertook the revision of the Latin text with a view to secure the desirable uniformity of style and diction.

The descriptive characters of the families, or natural orders, are drawn up with the same care as those of the separate genera; they are clear, exactly comparable, and the affinities of families, as well as the exceptional and abnormal forms which they not seldom present, are specially noted. The approximate number of known species belonging to each family, as well as to each separate genus, is stated throughout the work, and the geographical distribution of each genus, as well as of the larger groups, has been recorded as fully as the present state of our knowledge makes it possible. Finally, very

full references to the works in which each genus has been first described or best illustrated, with similar references to the authorities for synonyms, add further to the value of the work as a guide to the student of systematic botany.

The descriptive characters of the genera have been throughout verified or established after the previous examination of numerous specimens, and as a rule it may be said that for the purpose of this work the whole of the vast collections in the Royal Herbarium at Kew were passed in review, and especial attention given to the aberrant forms presented by many large genera. In the comparatively few cases where the authors were unable to refer to and examine specimens of a genus enumerated, they are careful to cite the author on whose authority it has been admitted. Genera that appear to the authors to have been founded on insufficient characters, or on an erroneous view of the structural facts, are in some cases reduced to the rank of subgenera or sections of the typical genus, in others simply recorded as synonyms at the conclusion of the description of the genus to which they are referred. There remains a further category of generic names given by authors who, either from ignorance of the science or incomplete materials, have failed to make it possible to identify them at the present day. These are enumerated as *Genera dubia* at the end of the synoptic table of the genera of each family. In short, it may be truly said that the authors have neglected nothing that could make their work useful and practical, as well as a complete storehouse of the present condition of our knowledge of this branch of natural science.

Of the many different points of view in which this great work may be regarded, the most interesting, perhaps, to the scientific naturalist is the consideration that we have here the results of a complete reconsideration of the whole subject of the classification of the flowering plants by two men of remarkable intellectual power, possessing an extent of knowledge and a command of materials far surpassing anything possible to the authors of preceding works of similar scope. In one or two brief sentences of a note already cited, Mr. Bentham has assigned the amply sufficient reasons which induced the authors to maintain in its main features the arrangement of the natural orders established by the elder De Candolle. Every attempt to set forth in a linear series the complex relations which connect together as in a network the various groups of the vegetable kingdom is necessarily incomplete and defective. It is a fortunate circumstance that the authors of the "Genera" have added the weight of their authority to the judgment of those botanists who hold that no one of the various arrangements which have been proposed during the last half century, and more or less extensively adopted in some parts of Europe, possesses advantages which can compensate the serious practical inconvenience of having systematic works of reference arranged after a variety of discordant systems. The Candollean arrangement has therefore been deliberately maintained in this work, with a few not unimportant modifications; but in the arrangement and grouping of the genera into tribes and subtribes there has been ample space for the exercise of the highest faculties of the philosophical naturalist. It is evident throughout the work that every question as it has arisen has received fresh consideration, and in many important families the classification adopted is altogether new. It is remarkable that, even in regard to families previously elaborated by Mr. Bentham, he has not hesitated to introduce important changes suggested by further consideration and study. It is of course impossible to say that the final results of future discovery and research may not lead to further modifications in botanical classification; but for the present generation this will remain as the best result of the comprehensive survey of the whole field of our knowledge.

The number of genera described in the present work, taking into account the addenda, is 7565, while the number described by Endlicher in his "Genera Plantarum," with the supplements, is 7202. These figures give some measure of the progress of botanical discovery during the last thirty years, and at the same time some indications of the amount of labour involved in the collection and examination of the materials scattered throughout the numerous general works and monographs published during that period, and especially throughout hundreds of volumes of scientific periodicals which are now annually produced in every part of the world. The increase in the number of known genera is in truth much greater than the figures above cited would indicate, inasmuch as the tendency of Bentham and Hooker is to unite under the same generic designation plants which do not appear to present sufficient differences of structure, and they have not hesitated to suppress numerous genera that have been admitted in preceding systematic works of authority. Those who may not be disposed to acquiesce in these conclusions may easily continue to regard as genera the subgenera and sections whose distinctive characters are throughout the work subjoined to the descriptions of the respective genera.

It follows from the preceding remarks that for practical use in classing large botanical collections the present work is an indispensable guide. The present writer, who has enjoyed the advantage of daily, almost hourly, reference to its pages, feels that he is merely discharging a debt of gratitude in endeavouring to express his sense of the scientific value of a work which has become a classic from the day of its publication. A work which, at a given period, summarises the entire field of knowledge in one department of science, marks an epoch in its progress, and becomes the starting-point for further advance towards wider knowledge. Such is the work to which Mr. Bentham and Sir Joseph Hooker have devoted a full quarter of a century, and as such, notwithstanding the importance of their other works, it must remain their chief title to enduring fame.

ERN. COSSON

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications.*

[*The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to insure the appearance even of communications containing interesting and novel facts.*]

The Red Spot upon Jupiter

THE red spot on Jupiter has really disappeared. I have observed the planet again after conjunction. The region in which

the red spot formerly was is now very white; it passed over the central meridian of the planet this morning at 4h. 36m. (M.T. at Palermo), which gives for this place the Jovicentric longitude 63°, plainly corresponding to the longitude that Mr. Marth assigned to the red spot at present, if visible. This proves that the neighbourhood of the red spot had followed the particular motion of the spot itself. This place is well characterised by the permanent depression in the great reddish band of the planet. A. Riccò

Royal Observatory, Palermo, September 10

"Elevation and Subsidence"

Mr. O. Fisher has been so good as to offer a reply to my "remark with a query," his answer being (allowing for an obvious printer's error) that it is "an open question whether the melting temperature of rocky matter is, or is not, raised by pressure."

I cannot for a moment pretend to the same familiarity with the results either of experiment or of calculation as is doubtless possessed by Mr. Fisher. I only claim to speak as representing the class whose knowledge on these subjects is essentially second-hand; but, speaking as such, I think that Mr. Fisher's reply will not generally be regarded as satisfactory. I should, therefore, like to repeat my question with a little extension:—

1. Do not the "rigidity" calculations incontestably show that the earth is extremely rigid, *i.e.* solid? Are not, therefore, all theories which disregard this result (such as that the nucleus may be above its own critical temperature) put out of count?

2. Are not the phenomena of metamorphic and hypogene rocks on too large a scale to be accounted for by heat of merely local origin, whether produced by chemical or mechanical action, such as has been suggested in connection with volcanoes?

3. Do not all reasonable views of the origin of the earth, *i.e.* any form of the nebular hypothesis, point to the same conclusion as (2), viz. that the earth's heat is the residuum of a much greater amount formerly possessed, and not yet entirely lost by radiation?

4. Does not (3), taken in connection with the known laws of conduction, involve a continuous increase of temperature, whether rapid or slow, as we descend below the surface?

5. Although we may have no *direct* evidence as to the "temperature at depths bearing considerable ratios to the radius," is there not ample evidence that at comparatively insignificant depths the temperature is such as would melt not only "rocky matter," but far more refractory substances, if there were no counteracting influence? Even allowing a very slow increase, provided the increase is always positive, as 4 points out, should we not sooner or later almost certainly reach the melting temperature of the most refractory substances with which we are acquainted?

6. Can we then escape the conclusion, either that the nucleus consists of matter of a totally different kind from anything with which we are familiar, or that pressure raises its melting temperature? But does not every fact bearing on the question discredit the former hypothesis?

7. Should we not then accept the view that pressure does raise the melting-point of nucleus stuff, at least as a working hypothesis, only to be overthrown by direct evidence to the contrary, if direct evidence on the subject is ever forthcoming?

Trinity College, Cambridge F. Young

In a paper I read before a full meeting of the Geological Association on March 2 last, of which a brief notice is given in Nature, vol. xxvii. p. 523, I discussed the probability of subsidence of land, in certain cases, being due to *loading* by local accumulations of terrestrial matter acting upon a deflectible crust supported upon a viscous interior. The greatest effects, I imagined, from this cause, were due to local accumulations of ice past and present, particularly about the poles of the earth; but that secondary and important effects were due to the weight of accumulations of solid mineral matter from denudations being carried by oceanic currents and winds, from coral deposition, and the reaction of volcanic outflows. One illustration I proposed was that the sinking of the coast of Greenland was probably due to the weight of inland accumulation of ice, which proposition I thought was original, but Mr. Gardner (Nature, vol. xxviii. p. 324) says—"It has often been supposed that the sinking of the coast of Greenland is similarly due to its icecap." I should feel obliged if Mr. Gardner would point out references where this has been proposed, as I thought I had read the literature of the subject, and I fear that this part of my paper is less original than I assumed. W. F. Stanley

That there is a connection between sedimentation and subsidence on the one hand and between denudation and elevation on the other is a fact now admitted by most geologists. The real question to be answered, however, is:—Are these directly connected as cause and effect? or are they simply concomitant effects of the same cause? If the first be true, we should expect cause and effect to vary together, that is, that subsidence should keep an even pace with sedimentation. That this has not been exceptionally the case is proved by the sections of the carboniferous system in the central valley of Scotland, where the facts point to a continuous subsidence, accompanied by a very irregular sedimentation, with the result that now subsidence gained on sedimentation, now sedimentation on subsidence. Again, once the process commenced—and it is not very evident how on an originally even surface it could have commenced at all—we should expect it to be continuous. Sedimentation causes subsidence, subsidence gives rise to fresh sedimentation, and that again to renewed subsidence, and so on and on. Consequently we should expect that when once an area of sedimentation and subsidence was formed, it would continue an area of sedimentation and subsidence through all geological time.

It appears rather, I think, that the connection between them arises from their being concomitant effects of lateral pressure in the earth's crust (for notwithstanding the Rev. O. Fisher's masterly exposition of the inadequacy of this cause to produce the observed inequalities of the earth's surface, I still believe that, with the exception of the ocean basins, which must be otherwise accounted for, it is quite competent to account for the facts). We may suppose the action to take place so:—

A certain portion of the earth's crust is first thickened and strengthened by volcanic outburst or other accumulation on the surface. This part, when the tangential thrust comes, offers, by reason of its increased weight and thickness, a greater resistance to the elevating force than the parts around, and as a consequence these are raised around the thickened part, while it is at the same time depressed in a corresponding degree; in other words it becomes the centre of a syncline, while the strata around are raised into anticlines. Depression naturally leads to sedimentation, and this still more thickens the part, and enables it to offer greater resistance to the tangential thrust, with the result that it continues to be depressed as the strata around are elevated. The converse is also true. Denudation means the thinning and consequent weakening of the crust, and hence when the thrust comes the denuded part is the more likely to be elevated into the anticline.

This theory provides for the cessation of the phenomena, since the tension of the crust is after a time relieved. It also accounts for the fact that strata around volcanoes and volcanic necks, as also along the base of mountain chains, so frequently appear to dip below them. The rate of subsidence, too, would vary with the intensity of the exciting force, though the consequent sedimentation need not vary with it in the same absolute degree.

Perth, September 3 William Mackie

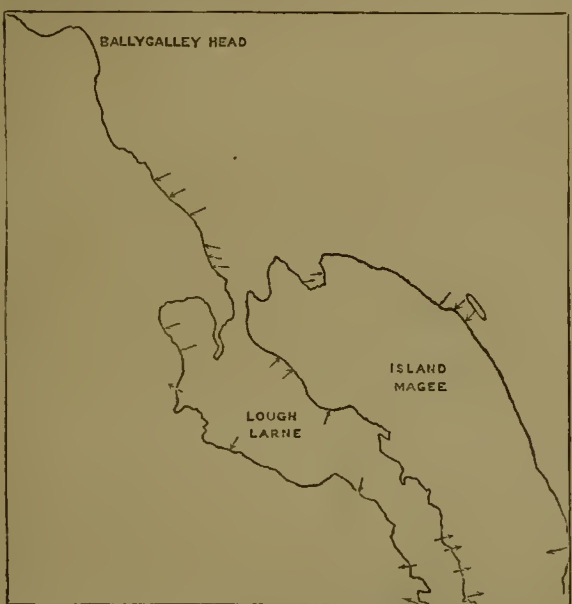
My article on elevation and subsidence has provoked considerable and, on the whole, friendly criticism, a so far satisfactory result, though but few points have been raised requiring reply. Dr. Ricketts objects, and very properly, that I have not alluded to his many writings on the subject; and to this I can only plead want of space, that I have not entered at all into its already voluminous bibliography, and that my article was written and in type before his recent contributions to the *Geological Magazine* had appeared. Beyond this I had sufficiently indicated that there were many observers in the field, and every geologist must be aware that the subject has for a long while past excited attention not only in England but in France and America.

The fundamental error in my article is pointed out by the Rev. Mr. Fisher and by Mr. Young, and the assumption that inert pressure induces heat must be abandoned. As I had read the "Physics of the Earth's Crust," I expected that this would be challenged, but I let it stand, as the fallacy has been shared by a large number of geologists, comprising some of the most distinguished, and has even escaped the correction of physicists. But this rectification, while very important, by no means affects the results, and on the contrary facilitates an appreciation of the

causes of movements of the earth's crust; for if the fluid or viscous layer is chiefly due to internal heat and the relaxation of pressure near the surface, it may exist much nearer to our feet than could otherwise be admitted.

One of the gravest difficulties that the theory that added weight produces subsidence by acting on a fluid layer has had to contend with has been the great depth at which this fluid layer has had to be placed. It has always seemed to me next to impossible that liquid lava could well up from any such depths as those assigned to the viscous layer, or that a solid crust of so great a thickness should be sensitive to, as it is now shown to be, and rise and fall under, barometric changes. In acknowledging Mr. Fisher's letter and thanking him, I feel I am ungrateful in questioning that part of his work which interposes barriers which would break up the continuity of the viscous layer; I allude to his theory of "the roots of mountains." There does seem to me to be little fact in support of so startling a proposition, and I think the existence of volcanic vents, scattered through and in the midst of some of the highest chains, renders its acceptance difficult.

Mr. Murray restates his theory of the formation of coral atolls and reefs in the clearest manner, but I do not see that he explains any fact left unexplained by Darwin, or exposes any flaw in Darwin's reasoning. These masses of coral may have been continuously forming throughout even successive geological



Sheet 21. Geological Survey of Ireland, Antrim Coast, facing north-east.

periods, and their thickness is perhaps not exceptionally remarkable relative to that of slowly deposited oceanic sediments. There is no evidence that atolls are mere incrustations of volcanic craters, and it seems to me difficult to imagine so great a number of craters at the same level so completely masked. There are volcanic isles in abundance outside coral areas, but none I think, or few, of the form of a coral atoll. After all, Mr. Murray only shows that a second explanation is possible, though I still prefer the first.

I regret, being from home, that I am unable to answer Mr. Stanley. I may have alluded to the sinking of Greenland myself, and if I did not it was because the illustration was too familiar and self-evident. The sinking on the Greenland coast is not, I have understood, universal.

I still think it would render a service to science if readers of NATURE residing on sea-coasts would furnish authentic examples of elevation or subsidence or of waste. The magnificent Antrim coast, which I have recently visited, furnishes examples of subsidence among most unyielding rocks. The cliffs on the mainland are capped with basalt and dip inland, yet the basalt reappears in the Skerries out to sea with the same dip and at a much lower level. The same correspondence in stratification is seen between the mainland and Rathlin, but also with a great difference in elevation. The dip inland in all cases on this coast should bring up much older rocks out to sea, unless we are prepared to admit a fault running parallel to the coast, and following its sinuosities, and at right angles to the general lines of faulting.

The way in which all the strata forming the cliffs along the Antrim coast dip inland is very remarkable. The accompanying tracing from the Geological Survey Map is of a particularly indented coast-line, and the arrows show that the dip is everywhere away from the sea, irrespective of any general strike. In fact the general strike must often be the reverse of that shown on the coast for the same strata crop out at much higher levels on the hills farther inland. I recollect that most cliffs that I have examined, particularly in Hampshire, dip away from the sea. It would appear that the removal of weight along a cliff line causes a local elevation, which gives a cant inward, whilst subsidence takes place under sediment farther out to sea. This seems to explain the observed facts connected with marine denudation; but I must take a future opportunity of entering more thoroughly into this part of the question.

Glasgow, September 12 J. STARKIE GARDNER

"Zoology at the Fisheries Exhibition"

LETTERS have been published in NATURE of August 9 and 16 (pp. 334 and 366) by Mr. Bryce-Wright of Regent Street and Prof. Honeyman of Canada, calling in question the accuracy of statements made in an article in NATURE (vol. xxviii. p. 289) which were condemnatory of exhibits for which these two gentlemen are respectively responsible. It is natural that they should seek to remove the unfavourable impression which the statements in question were intended to convey: they seem, however, to have been unacquainted with the complete character of the information upon which the statements were based. Mr. Bryce-Wright states that it is not the fact that some of the corals exhibited in Lady Brassey's case belong to him. Nevertheless it is the fact that when the jury of Class V. asked Mr. Bryce-Wright to point out the corals entered in the official catalogue under his name, No. "813*b*," he informed them that the corals so entered were in the same case with Lady Brassey's corals, and formed part of that collection. It is also the fact that in the opinion of experts the names attached by Mr. Bryce-Wright to many of these corals are incorrect; and as to his assertion that these specimens have been compared with those in the British Museum and with those obtained during the *Challenger* Expedition, it is a fact that neither the one series nor the other has been accessible for such purposes for some considerable time, and I have reason to believe that no qualified zoologist has made a comparison of the corals exhibited by Lady Brassey and Mr. Bryce-Wright with any collection at all.

The letter of Prof. Honeyman in reference to the naming and state of preservation of the Collection in the Canadian Department, for which he is responsible, is misleading. The discreditable state of that collection, to which a passing allusion only was made in NATURE, has been remedied in one or two instances since the visit of the jury of Class V. Should there be any doubt as to the justice of the opinion expressed in the article in NATURE, I would simply ask Prof. Honeyman whether he would have any objection to allowing the matter to be decided by reference to the report of the jury of Class V., of which he was a member. I should be surprised (and so I think would he) were the report of that jury, when published, found to be at variance with the opinion expressed in the article in NATURE. Prof. Honeyman's statement that the specimen of *Cryptochiton Stelleri* is properly exhibited in a convenient glass jar and labelled inside and out, is calculated to mislead. When first exhibited it was not labelled with any name; subsequently it was labelled with the name of a genus of Holothurians, "Psolus." After the visit of the jury of Class V., probably as the result of information imparted by some of the eminent zoologists who served on that jury, it was labelled with its proper name. Without citing details, I shall simply state that there are (or were when the article in NATURE was written) far more serious blunders in the identification of specimens and worse instances of bad preservation in the Canadian collection of Invertebrata than those to which special allusion has been made.

THE WRITER OF THE ARTICLE

A Complete Solar Rainbow

MR. D. MORRIS, in his account of this rainbow (p. 436) appears to have fallen into a mistake in stating that its inner dia-

meter—taken by Capt. Winchester, R.N.R.—was 43° 08′. It should be, I think, "inner semidiameter." The first circumsolar bow has a semidiameter of 41° 37′. That is almost necessarily invisible. The second circumsolar bow has a semidiameter of 43° 52′, and is rarely visible. I have no doubt that was the bow witnessed on board the *Norham Castle* on August 16

Athenæum Club, September 7 C. M. INGLEBY

Flint Flakes Replaced

As this subject has been more than once adverted to in NATURE, the following recent instances of placing flint flakes on to their original position may be interesting:—

Whilst examining the relics from Cowper's Camp, Epping Forest, in Mr. Raphael Meldola's house last month, I looked over a small number of flakes collected from one spot in the

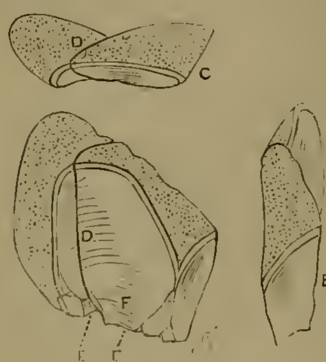


FIG. 1.

rampart of the camp, with remains of burnt wood and late Celtic pottery. I immediately saw that several of the flakes had been struck from the same block of flint, and after a short examination I managed to replace two as illustrated, one-half real size, in Fig. 1. The front of the two conjoined flakes is shown in the lefthand bottom figure, the side at B, the top at C, and the line of junction at D D. Behind E E are two cones of percussion, one belonging to each flake, and at F is the depression into which the cone of the missing frontal flake at one time fitted. The fractured part of the flint is deep chocolate brown, and lustrous, and the bark of the flint is dull ochreous; the flakes are undoubtedly artificial, and as old as the rampart of the camp, not less than two thousand years. This example, with other relics, will be placed in the Guildhall Museum.

Greater interest attaches to the replacing of Palæolithic flakes, as these are enormously older than Neolithic, and the chances are so very much against lighting on a perfectly undisturbed Palæolithic position.

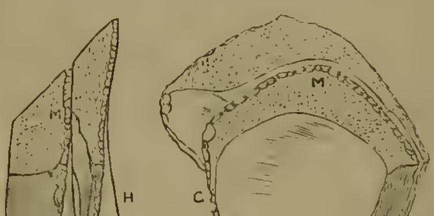


FIG. 2.

At Fig. 2 is illustrated (one-half actual size) two Palæolithic flakes from the "Palæolithic floor" at Stoke Newington Common, found and replaced by me. The front of the conjoined flakes is shown at G and the side at H. I found the lower flake two days before, and some distance from where I found the upper one; but as I have a method of placing newly found sharp flakes on a table, arranged temporarily in accordance with their colour and markings, I speedily saw that the upper flake would fit on to the lower one. Each flake has a cone of percussion, as shown at J K, and the upper flake has a well-marked depression at L, corresponding with the missing flake, which, if it had been found, would have fitted on to the front of the two conjoined examples. Both flakes are sharp and slightly stained with the ochreous river sand which overlaid them. Both (especially the upper one) show unmistakable signs of having been used as scrapers, the upper curved edge (and that edge only) being worn away by use. The worn upper edge of the superimposed flake at M M is distinctly shown in the illustration. A small intermediate piece belonging to the position at N I did not find. Both are naturally mottled in a peculiar manner, and the pattern and colour of the mottling exactly agree.

WORTHINGTON G. SMITH

NOTES ON THE POST-GLACIAL GEOLOGY OF THE COUNTRY AROUND SOUTHPORT

SINCE the writer carried out the geological survey of the western coast of Lancashire in 1868 he has constantly been asked, "Is there any geology to be studied at Southport? Is not the country a sandy expanse fringing peat-mosses of ceaseless monotony?" The meeting of the British Association this week at Southport renders this a fitting time to reply to these questions; for, strange as it may appear, in these apparently unpromising surroundings exists a record of the complete sequence of events from the commencement of the Glacial episode down to the present time. The sand dunes, rising to 50 and even 80 feet in height, that form so prominent a feature between Liverpool and Southport, rest upon a wedge-shaped mass of sand blown from the coast by westerly winds over the thick peat-mosses that intervene between the coast and the rising ground about Ormskirk; the surface of the Glacial beds, with the overlying deposits, dip steadily towards the sea, and fragments of peat are frequently trawled up by the fishermen.

Beneath the sand dunes on the sea coast the peat is seen cropping out, and at the base of the peat occur the roots of forest trees embedded in clay beneath, while trunks of trees lie scattered in many directions, but generally with their heads lying to the north-east, as if they had been blown over by a gale from the south-west. The bases of the trunks are left standing in the places where they grew; all appear to have been broken off at a uniform level, and it is most probable that through the drainage being obstructed water surrounded the trees, which gradually became rotten at the point of contact of the air and the water, and thus the way was prepared for the effects of storms and hurricanes. Sections of these beds near High Town, at the mouth of the Alt, will be found of great interest. Sections also occur on the coast at Dunkirk, near Crossens. At the Palace Hotel, Kirkdale, a boring was put down in 1867, that proved the sand to be 78 feet in thickness, resting on 18 inches of peat, which occurs at about 90 feet beneath high-water mark. When the land stood this amount above its present level, the coast would range in a straight north and south line from St. Bees Head to the mouth of the Clywd at Rhyl, but there is no reason to suppose that this amount represents the subsequent submergence since the era of the peat in Lancashire and North Wales. It is far more probable that when the trees flourished, found at the bottom of the peat fringing these coasts, this coast nearly coincided with the present twenty-fathom line, which passes from Anglesea round the Isle of Man; in that island the same sequence of post-glacial deposits is found, and the Irish elk alike occurs in the grey slags beneath peat.

At the mouth of the Ribble very interesting sections occur at Freckleton and Dow Brook; the latter is crossed by a Roman road, and has upon it a "Roman bath," only ten feet above the present high-water mark, proving the elevation of this coast has not been great since Roman times. The same fact is brought out by the interesting find of Roman coins near Rossal landmark, near Fleetwood, which were found in a salt-marsh clay lying on the peat beds, at about eight feet below the

surface, or at about high-water mark, the coins having been apparently lost by the Romans scrambling over the soft slippery mud. This discovery proves the thick peat beds to be of older date than the Romans; this is also borne out by the very remarkable sections along the north coast of Wirral, especially near Leasowe, which have afforded the fine collection of antiquities preserved in the Liverpool Free Museum; the silty beds over the peat yield Roman coins of Nero, Antoninus Pius, and Marcus Aurelius, while in the peat beds beneath occur flint implements of the Neolithic type. When the peat beds of Western Lancashire are followed into the valleys of the large rivers that traverse the country, they are found to pass insensibly into a peaty seam occurring at the base of the alluvium of the lowest plain of these rivers. This is well seen in the valley of the Ribble at Preston; it is more than a mile in width, and 180 feet in depth; it is excavated entirely in the Glacial deposits, down to the rocky floor, which lies somewhat below high-water mark, and nearer the sea slopes down considerably beneath it. On the slopes of the valley lie terraces of old alluvium, marking successive stages in the process of denudation, commenced since the deposition of the Upper Boulder Clay, as the bottom of the valley is the ordinary alluvial plain, made of silt, resting on a peaty bed, with trunks of trees lying on rough river gravel, the latter marking a period of great fluviatile denudation, when the land was at least as high, if not higher, above the sea as it is at present. To this era belong the marine beds lying beneath the peat I have called the *Presall shingle*, occurring east of Fleetwood, and the *Shirdley Hill sands* near Southport, which mark the position of old sea-beaches and old sand dunes respectively.

From these facts it appears that the excavation of the Western Lancashire river valleys was entirely carried out since the Glacial episode, that they had reached their present depth when Neolithic man inhabited the northwest of England, and that since that era much land has been destroyed, now covered by the Irish Sea, but since Roman times there has been but little change.

C. E. DE RANCE

THE BRITISH ASSOCIATION

THE Southport meeting promises to be one of the most successful since the Association met in Liverpool twelve years ago. According to the latest statistics it is expected that in attendance it may even rival the York meeting, when over 2500 people gathered to celebrate the jubilee of the Association. From the information we have already published it will have been seen that Southport has shown the greatest zeal in preparing to give a generous reception to the representatives of British science; and if only the weather be propitious, there can be little doubt that the meeting will be a success. Both the papers to be read and the reports to be presented are expected this year to suggest some specially interesting subjects for discussion.

Last night Sir C. W. Siemens resigned the presidential chair to Prof. Cayley, who then delivered the opening address.

INAUGURAL ADDRESS BY ARTHUR CAYLEY, M.A., D.C.L., LL.D., F.R.S., SADLERIAN PROFESSOR OF PURE MATHEMATICS IN THE UNIVERSITY OF CAMBRIDGE, PRESIDENT.

SINCE our last meeting we have been deprived of three of our most distinguished members. The loss by the death of Prof. Henry John Stephen Smith is a very grievous one to those who knew and admired and loved him, to his University, and to mathematical science, which he cultivated with such ardour and success. I need hardly recall that the branch of mathematics to which he had specially devoted himself was that most interesting and difficult one, the Theory of Numbers. The immense range of this subject, connected with and ramifying into so many others, is nowhere so well seen as in the series of reports on the progress thereof, brought up unfortunately only to the year 1865, contributed by him to the Reports of the Association; but it will still better appear when to these are united (as will be done in the collected works in course of publication by the Clarendon Press) his other mathematical writings, many of them containing his own further developments of theories referred to in the reports. There have been recently or are being published many such collected editions—Abel, Cauchy, Clifford, Gauss, Green, Jacobi, Lagrange, Maxwell, Riemann, Steiner. Among these the works of Henry Smith will occupy a worthy position.

More recently, General Sir Edward Sabine, K.C.B., for twenty-one years general secretary of the Association, and a trustee, president of the meeting at Belfast in the year 1852, and for many years treasurer and afterwards president of the Royal Society, has been taken from us at an age exceeding the ordinary age of man. Born October, 1788, he entered the Royal Artillery in 1803, and commanded batteries at the siege of Fort Erie in 1814; made magnetic and other observations in Ross and Parry's North Polar exploration in 1818–19, and in a series of other voyages. He contributed to the Association reports on Magnetic Forces in 1836-7-8, and about forty papers to the *Philosophical Transactions;* originated the system of Magnetic Observatories, and otherwise signally promoted the science of Terrestrial Magnetism.

There is yet a very great loss: another late president and trustee of the Association, one who has done for it so much, and has so often attended the meetings, whose presence among us at this meeting we might have hoped for—the president of the Royal Society, William Spottiswoode. It is unnecessary to say anything of his various merits: the place of his burial, the crowd of sorrowing friends who were present in the Abbey, bear witness to the esteem in which he was held.

I take the opportunity of mentioning the completion of a work promoted by the Association: the determination by Mr. James Glaisher of the least factors of the missing three out of the first nine million numbers: the volume containing the sixth million is now published.

I wish to speak to you to-night upon Mathematics. I am quite aware of the difficulty arising from the abstract nature of my subject; and if, as I fear, many or some of you, recalling the Presidential Addresses at former meetings—for instance, the *résumé* and survey which we had at York of the progress, during the half century of the lifetime of the Association, of a whole circle of sciences—Biology, Palæontology, Geology, Astronomy, Chemistry—so much more familiar to you, and in which there was so much to tell of the fairy-tales of science; or at Southampton, the discourse of my friend who has in such kind terms introduced me to you, on the wondrous practical applications of science to electric lighting, telegraphy, the St. Gothard Tunnel, and the Suez Canal, gun-cotton, and a host of other purposes, and with the grand concluding speculation on the conservation of solar energy: if, I say, recalling these or any earlier addresses, you should wish that you were now about to have, from a different president, a discourse on a different subject, I can very well sympathise with you in the feeling.

But, be this as it may, I think it is more respectful to you that I should speak to you upon and do my best to interest you in the subject which has occupied me, and in which I am myself most interested. And in another point of view, I think it is right that the Address of a President should be on his own subject, and that different subjects should be thus brought in turn before the meetings. So much the worse, it may be, for a particular meeting; but the meeting is the individual, which on evolution principles must be sacrificed for the development of the race.

Mathematics connect themselves on the one side with common life and the physical sciences; on the other side with philosophy, in regard to our notions of space and time; and in the questions which have arisen as to the universality and necessity of the truths of mathematics, and the foundation of our knowledge of them. I would remark here that the connection (if it exists) of arithmetic and algebra with the notion of time is far less obvious than that of geometry with the notion of space.

As to the former side, I am not making before you a defence of mathematics, but if I were I should desire to do it—in such manner as in the "Republic" Socrates was required to defend justice, quite irrespectively of the worldly advantages which may accompany a life of virtue and justice, and to show that, independently of all these, justice was a thing desirable in itself and for its own sake—*not* by speaking to you of the utility of mathematics in any of the questions of common life or of physi-

cal science. Still less would I speak of this utility before, I trust, a friendly audience, interested or willing to appreciate an interest in mathematics in itself and for its own sake. I would, on the contrary, rather consider the obligations of mathematics to these different subjects as the sources of mathematical theories now as remote from them, and in as different a region of thought —for instance, geometry from the measurement of land, or the Theory of Numbers from arithmetic—as a river at its mouth is from its mountain source.

On the other side the general opinion has been and is that it is indeed by experience that we arrive at the truths of mathematics, but that experience is not their proper foundation: the mind itself contributes something. This is involved in the Platonic theory of reminiscence; looking at two things, trees or stones or anything else, which seem to us more or less equal, we arrive at the idea of equality: but we must have had this idea of equality before the time when first seeing the two things we were led to regard them as coming up more or less perfectly to this idea of equality; and the like as regards our idea of the beautiful, and in other cases.

The same view is expressed in the answer of Leibnitz, the *nisi intellectus ipse*, to the scholastic dictum, *nihil in intellectu quod non prius in sensu*: there is nothing in the intellect which was not first in sensation, except (said Leibnitz) the intellect itself. And so again in the "Critick of Pure Reason," Kant's view is that, while there is no doubt but that all our cognition begins with experience, we are nevertheless in possession of cognitions *a priori*, independent, not of this or that experience, but absolutely so of all experience, and in particular that the axioms of mathematics furnish an example of such cognitions *a priori*. Kant holds further that space is no empirical conception which has been derived from external experiences, but that in order that sensations may be referred to something external, the representation of space must already lie at the foundation; and that the external experience is itself first only possible by this representation of space. And in like manner time is no empirical conception which can be deduced from an experience, but it is a necessary representation lying at the foundation of all intuitions.

And so in regard to mathematics, Sir W. R. Hamilton, in an introductory lecture on astronomy (1836), observes: "These purely mathematical sciences of algebra and geometry are sciences of the pure reason, deriving no weight and no assistance from experiment, and isolated or at least isolable from all outward and accidental phenomena. The idea of order, with its subordinate ideas of number and figure, we must not indeed call innate ideas, if that phrase be defined to imply that all men must possess them with equal clearness and fulness: they are, however, ideas which seem to be so far born with us that the possession of them in any conceivable degree is only the development of our original powers, the unfolding of our proper humanity."

The general question of the ideas of space and time, the axioms and definitions of geometry, the axioms relating to number, and the nature of mathematical reasoning, are fully and ably discussed in Whewell's "Philosophy of the Inductive Sciences" (1840), which may be regarded as containing an exposition of the whole theory.

But it is maintained by John Stuart Mill that the truths of mathematics, in particular those of geometry, rest on experience; and, as regards geometry, the same view is on very different grounds maintained by the mathematician Riemann.

It is not so easy as at first sight it appears to make out how far the views taken by Mill in his "System of Logic Ratiocinative and Inductive" (ninth edition, 1879) are absolutely contradictory to those which have been spoken of; they profess to be so; there are most definite assertions (supported by argument), for instance, p. 263:—"It remains to inquire what is the ground of our belief in axioms, what is the evidence on which they rest. I answer, they are experimental truths, generalisations from experience. The proposition 'Two straight lines cannot inclose a space,' or, in other words, two straight lines which have once met cannot meet again, is an induction from the evidence of our senses." But I cannot help considering a previous argument (p. 259) as very materially modifying this absolute contradiction. After inquiring "Why are mathematics by almost all philosophers . . . considered to be independent of the evidence of experience and observation, and characterised as systems of necessary truth?" Mill proceeds (I quote the whole passage) as follows:—"The answer I conceive to be that this character of necessity ascribed to the truths of mathematics, and even (with some reservations to be hereafter made) the peculiar certainty ascribed to them, is a delusion, in order to sustain which it is necessary to suppose that those truths relate to and express the properties of purely imaginary objects. It is acknowledged that the conclusions of geometry are derived partly at least from the so-called definitions, and that these definitions are assumed to be correct representations, as far as they go, of the objects with which geometry is conversant. Now we have pointed out that from a definition as such no proposition, unless it be one concerning the meaning of a word, can ever follow, and that what apparently follows from a definition follows in reality from an implied assumption that there exists a real thing conformable thereto. This assumption in the case of the definitions of geometry is not strictly true: there exist no real things exactly conformable to the definitions. There exist no real points without magnitude, no lines without breadth, nor perfectly straight, no circles with all their radii exactly equal, nor squares with all their angles perfectly right. It will be said that the assumption does not extend to the actual but only to the possible existence of such things. I answer that according to every test we have of possibility they are not even possible. Their existence, so far as we can form any judgment, would seem to be inconsistent with the physical constitution of our planet at least, if not of the universal [*sic*]. To get rid of this difficulty, and at the same time to save the credit of the supposed system of necessary truths, it is customary to say that the points, lines, circles, and squares which are the subjects of geometry, exist in our conceptions merely, and are parts of our minds: which minds, by working on their own materials, construct an *a priori* science, the evidence of which is purely mental and has nothing to do with outward experience. By howsoever high authority this doctrine has been sanctioned, it appears to me psychologically incorrect. The points, lines, and squares which any one has in his mind, are (as I apprehend) simply copies of the points, lines, and squares which he has known in his experience. Our idea of a point I apprehend to be simply our idea of the *minimum visibile*, the small portion of surface which we can see. We can reason about a line as if it had no breadth, because we have a power which we can exercise over the operations of our minds: the power, when a perception is present to our senses or a conception to our intellects, of *attending* to a part only of that perception or conception instead of the whole. But we cannot *conceive* a line without breadth: we can form no mental picture of such a line: all the lines which we have in our mind are lines possessing breadth. If any one doubt this, we may refer him to his own experience. I much question if any one who fancies that he can conceive of a mathematical line thinks so from the evidence of his own consciousness. I suspect it is rather because he supposes that unless such a perception be possible, mathematics could not exist as a science: a supposition which there will be no difficulty in showing to be groundless."

I think it may be at once conceded that the truths of geometry are truths precisely because they relate to and express the properties of what Mill calls "purely imaginary objects"; that these objects do not exist in Mill's sense, that they do not exist in nature, may also be granted; that they are "not even possible," if this means not possible in an existing nature, may also be granted. That we cannot "conceive" them depends on the meaning which we attach to the word conceive. I would myself say that the purely imaginary objects are the only realities, the ὄντως ὄντα, in regard to which the corresponding physical objects are as the shadows in the cave; and it is only by means of them that we are able to deny the existence of a corresponding physical object; if there is no conception of straightness, then it is meaningless to deny the existence of a perfectly straight line.

But at any rate the objects of geometrical truth are the so-called imaginary objects of Mill, and the truths of geometry are only true, and *a fortiori* are only necessarily true, in regard to these so-called imaginary objects; and these objects, points, lines, circles, &c., in the mathematical sense of the terms, have a likeness to and are represented more or less imperfectly, and from a geometer's point of view no matter how imperfectly, by corresponding physical points, lines, circles, &c. I shall have to return to geometry, and will then speak of Riemann, but I will first refer to another passage of the "Logic."

Speaking of the truths of arithmetic Mill says (p. 297) that even here there is one hypothetical element: "In all propositions concerning numbers a condition is implied without which none of them would be true, and that condition is an assumption which may be false. The condition is that $1 = 1$: that all the numbers are numbers of the same or of equal units." Here at least the assumption may be absolutely true; one shilling = one

shilling in purchasing power, although they may not be absolutely of the same weight and fineness: but it is hardly necessary; one coin + one coin = two coins, even if the one be a shilling and the other a half-crown. In fact, whatever difficulty be raisable as to geometry, it seems to me that no similar difficulty applies to arithmetic; mathematician or not, we have each of us, in its most abstract form, the idea of a number; we can each of us appreciate the truth of a proposition in regard to numbers; and we cannot but see that a truth in regard to numbers is something different in kind from an experimental truth generalised from experience. Compare, for instance, the proposition that the sun, having already risen so many times, will rise to-morrow, and the next day, and the day after that, and so on; and the proposition that even and odd numbers succeed each other alternately *ad infinitum*: the latter at least seems to have the characters of universality and necessity. Or, again, suppose a proposition observed to hold good for a long series of numbers, one thousand numbers, two thousand numbers, as the case may be: this is not only no proof, but it is absolutely no evidence, that the proposition is a true proposition, holding good for all numbers whatever; there are in the Theory of Numbers very remarkable instances of propositions observed to hold good for very long series of numbers and which are nevertheless untrue.

I pass in review certain mathematical theories.

In arithmetic and algebra, or say in analysis, the numbers or magnitudes which we represent by symbols are in the first instance ordinary (that is, positive) numbers or magnitudes. We have also in analysis and in analytical geometry *negative* magnitudes; there has been in regard to these plenty of philosophical discussion, and I might refer to Kant's paper, "Ueber die negativen Grössen in die Weltweisheit" (1763), but the notion of a negative magnitude has become quite a familiar one, and has extended itself into common phraseology. I may remark that it is used in a very refined manner in bookkeeping by double entry.

But it is far otherwise with the notion which is really the fundamental one (and I cannot too strongly emphasise the assertion) underlying and pervading the whole of modern analysis and geometry, that of imaginary magnitude in analysis and of imaginary space (or space as a *locus in quo* of imaginary points and figures) in geometry: I use in each case the word imaginary as including real. This has not been, so far as I am aware, a subject of philosophical discussion or inquiry. As regards the older metaphysical writers, this would be quite accounted for by saying that they knew nothing, and were not bound to know anything, about it; but at present, and considering the prominent position which the notion occupies—say even that the conclusion were that the notion belongs to mere technical mathematics, or has reference to nonentities in regard to which no science is possible, still it seems to me that (as a subject of philosophical discussion) the notion ought not to be thus ignored; it should at least be shown that there is a right to ignore it.

Although in logical order I should perhaps now speak of the notion just referred to, it will be convenient to speak first of some other quasi-geometrical notions; those of more-than-three-dimensional space, and of non-Euclidian two- and three-dimensional space, and also of the generalised notion of distance. It is in connection with these that Riemann considered that our notion of space is founded on experience, or rather that it is only by experience that we know that our space is Euclidian space.

It is well known that Euclid's twelfth axiom, even in Playfair's form of it, has been considered as needing demonstration; and that Lobatschewsky constructed a perfectly consistent theory wherein this axiom was assumed not to hold good, or say a system of non-Euclidian plane geometry. There is a like system of non-Euclidian solid geometry. My own view is that Euclid's twelfth axiom in Playfair's form of it does not need demonstration, but is part of our notion of space, of the physical space of our experience—the space, that is, which we become acquainted with by experience, but which is the representation lying at the foundation of all external experience. Riemann's view before referred to may I think be said to be that, having *in intellectu* a more general notion of space (in fact a notion of non-Euclidian space), we learn by experience that space (the physical space of our experience) is, if not exactly, at least to the highest degree of approximation, Euclidian space.

But, suppose the physical space of our experience to be thus only approximately Euclidian space, what is the consequence which follows? *Not* that the propositions of geometry are only approximately true, but that they remain absolutely true in regard to that Euclidian space which has been so long regarded as being the physical space of our experience.

It is interesting to consider two different ways in which, without any modification at all of our notion of space, we can arrive at a system of non-Euclidian (plane or two-dimensional) geometry; and the doing so will, I think, throw some light on the whole question.

First, imagine the earth a perfectly smooth sphere; understand by a plane the surface of the earth, and by a line the apparently straight line (in fact an arc of great circle) drawn on the surface; what experience would in the first instance teach would be Euclidian geometry; there would be intersecting lines which produced a few miles or so would seem to go on diverging, and apparently parallel lines which would exhibit no tendency to approach each other; and the inhabitants might very well conceive that they had by experience established the axiom that two straight lines cannot inclose a space, and the axiom as to parallel lines. A more extended experience and more accurate measurements would teach them that the axioms were each of them false; and that any two lines if produced far enough each way would meet in two points: they would in fact arrive at a spherical geometry, accurately representing the properties of the two-dimensional space of their experience. But their original Euclidian geometry would not the less be a true system; only it would apply to an ideal space, not the space of their experience.

Secondly, consider an ordinary, indefinitely extended plane; and let us modify only the notion of distance. We measure distance, say, by a yard measure or a foot rule, anything which is short enough to make the fractions of it of no consequence (in mathematical language by an infinitesimal element of length); imagine, then, the length of this rule constantly changing (as it might do by an alteration of temperature), but under the condition that its actual length shall depend only on its situation on the plane and on its direction: viz., if for a given situation and direction it has a certain length, then whenever it comes back to the same situation and direction it must have the same length. The distance along a given straight or curved line between any two points could then be measured in the ordinary manner with this rule, and would have a perfectly determinate value; it could be measured over and over again, and would always be the same; but of course it would be the distance, not in the ordinary acceptation of the term, but in quite a different acceptation. Or in a somewhat different way: if the rate of progress from a given point in a given direction be conceived as depending only on the configuration of the ground, and the distance along a given path between any two points thereof be measured by the time required for traversing it, then in this way also the distance would have a perfectly determinate value; but it would be a distance, not in the ordinary acceptation of the term, but in quite a different acceptation. And corresponding to the new notion of distance, we should have a new, non-Euclidian system of plane geometry; all theorems involving the notion of distance would be altered.

We may proceed further. Suppose that as the rule moves away from a fixed central point of the plane it becomes shorter and shorter; if this shortening takes place with sufficient rapidity, it may very well be that a distance which in the ordinary sense of the word is finite will in the new sense be infinite; no number of repetitions of the length of the ever-shortening rule will be sufficient to cover it There will be surrounding the central point a certain finite area such that (in the new acceptation of the term distance) each point of the boundary thereof will be at an infinite distance from the central point; the points outside this area you cannot by any means arrive at with your rule; they will form a *terra incognita*, or rather an unknowable land: in mathematical language, an imaginary or impossible space: and the plane space of the theory will be that within the finite area—that is, it will be finite instead of infinite.

We thus with a proper law of shortening arrive at a system of non-Euclidian geometry which is essentially that of Lobatschewsky. But in so obtaining it we put out of sight its relation to spherical geometry: the three geometries (spherical, Euclidian, and Lobatschewsky's) should be regarded as members of a system: viz., they are the geometries of a plane (two-dimensional) space of constant positive curvature, zero-curvature, and constant negative curvature respectively; or, again, they are the plane geometries corresponding to three different notions of distance;

in this point of view they are Klein's elliptic, parabolic, and hyperbolic geometries respectively.

Next as regards solid geometry: we can by a modification of the notion of distance (such as has just been explained in regard to Lobatschewsky's system) pass from our present system to a non-Euclidian system; for the other mode of passing to a non-Euclidian system it would be necessary to regard our space as a flat three-dimensional space existing in a space of four dimensions (*i.e.* as the analogue of a plane existing in ordinary space); and to substitute for such flat three-dimensional space a curved three-dimensional space, say of constant positive or negative curvature. In regarding the physical space of our experience as possibly non-Euclidian, Riemann's idea seems to be that of modifying the notion of distance, not that of treating it as a locus in four-dimensional space.

I have just come to speak of four-dimensional space. What meaning do we attach to it? or can we attach to it any meaning? It may be at once admitted that we cannot conceive of a fourth dimension of space; that space as we conceive of it, and the physical space of our experience, are alike three-dimensional; but we can, I think, conceive of space as being two- or even one-dimensional; we can imagine rational beings living in a one-dimensional space (a line) or in a two-dimensional space (a surface), and conceiving of space accordingly, and to whom, therefore, a two-dimensional space, or (as the case may be) a three-dimensional space, would be as inconceivable as a four-dimensional space is to us. And very curious speculative questions arise. Suppose the one-dimensional space a right line, and that it afterwards becomes a curved line: would there be any indication of the change? Or, if originally a curved line, would there be anything to suggest to them that it was not a right line? Probably not, for a one-dimensional geometry hardly exists. But let the space be two-dimensional, and imagine it originally a plane, and afterwards bent (converted, that is, into some form of developable surface) or converted into a curved surface; or imagine it originally a developable or curved surface. In the former case there should be an indication of the change, for the geometry originally applicable to the space of their experience (our own Euclidian geometry) would cease to be applicable; but the change could not be apprehended by them as a bending or deformation of the plane, for this would imply the notion of a three-dimensional space in which this bending or deformation could take place. In the latter case their geometry would be that appropriate to the developable or curved surface which is their space: viz. this would be their Euclidian geometry: would they ever have arrived at our own more simple system? But take the case where the two-dimensional space is a plane, and imagine the beings of such a space familiar with our own Euclidian plane geometry; if, a third dimension being still inconceivable by them, they were by their geometry or otherwise led to the notion of it, there would be nothing to prevent them from forming a science such as our own science of three-dimensional geometry.

Evidently all the foregoing questions present themselves in regard to ourselves, and to three-dimensional space as we conceive of it, and as the physical space of our experience. And I need hardly say that the first step is the difficulty, and that granting a fourth dimension we may assume as many more dimensions as we please. But whatever answer be given to them, we have, as a branch of mathematics, potentially, if not actually, an analytical geometry of n-dimensional space. I shall have to speak again upon this.

Coming now to the fundamental notion already referred to, that of imaginary magnitude in analysis and imaginary space in geometry: I connect this with two great discoveries in mathematics made in the first half of the seventeenth century, Harriot's representation of an equation in the form $f(x) = 0$, and the consequent notion of the roots of an equation as derived from the linear factors of $f(x)$ (Harriot 1560-1621: his "Algebra," published after his death, has the date 1631), and Descartes' method of coordinates, as given in the "Géometrie," forming a short supplement to his "Traité de la Méthode, &c." (Leyden, 1637).

I show how by these we are led analytically to the notion of imaginary points in geometry; for instance, we arrive at the theorem that a straight line and circle in the same plane intersect *always* in two points, real or imaginary. The conclusion as to the two points of intersection cannot be contradicted by experience: take a sheet of paper and draw on it the straight line and circle, and try. But you might say, or at least be strongly tempted to say, that it is meaningless. The question of course arises, What is the meaning of an imaginary point? and, further, In what manner can the notion be arrived at geometrically?

There is a well known construction in perspective for drawing lines through the intersection of two lines which are so nearly parallel as not to meet within the limits of the sheet of paper. You have two given lines which do not meet, and you draw a third line, which, when the lines are all of them produced, is found to pass through the intersection of the given lines. If instead of lines we have two circular arcs not meeting each other, then we can, by means of these arcs, construct a line; and if on completing the circles it is found that the circles intersect each other in two real points, then it will be found that the line passes through these two points: if the circles appear not to intersect, then the line will appear not to intersect either of the circles. But the geometrical construction being in each case the same, we say that in the second case also the line passes through the two intersections of the circles.

Of course it may be said in reply that the conclusion is a very natural one, provided we assume the existence of imaginary points; and that, this assumption not being made, then, if the circles do not intersect, it is meaningless to assert that the line passes through their points of intersection. The difficulty is not got over by the analytical method before referred to, for this introduces difficulties of its own: is there in a plane a point the coordinates of which have given imaginary values? As a matter of fact, we do consider in plane geometry imaginary points introduced into the theory analytically or geometrically as above.

The like considerations apply to solid geometry, and we thus arrive at the notion of imaginary space as a *locus in quo* of imaginary points and figures.

I have used the word imaginary rather than complex, and I repeat that the word has been used as including real. But, this once understood, the word becomes in many cases superfluous, and the use of it would even be misleading. Thus, "a problem has so many solutions:" this means so many imaginary (including real) solutions. But if it were said that the problem had "so many imaginary solutions," the word "imaginary" would here be understood to be used in opposition to real. I give this explanation the better to point out how wide the application of the notion of the imaginary is, viz. (unless expressly or by implication excluded) it is a notion implied and presupposed in all the conclusions of modern analysis and geometry. It is, as I have said, the fundamental notion underlying and pervading the whole of these branches of mathematical science.

I consider the question of the geometrical representation of an imaginary variable. We represent the imaginary variable $x + iy$ by means of a point in a plane, the coordinates of which are (x, y). This idea, due to Gauss, dates from about the year 1831. We thus picture to ourselves the succession of values of the imaginary variable $x + iy$ by means of the motion of the representative point: for instance, the succession of values corresponding to the motion of the point along a closed curve to its original position. The value $X + iY$ of the function can of course be represented by means of a point (taken for greater convenience in a different plane), the coordinates of which are X, Y.

We may consider in general two points, moving each in its own plane, so that the position of one of them determines the position of the other, and consequently the motion of the one determines the motion of the other: for instance, the two points may be the tracing-point and the pencil of a pentagraph. You may with the first point draw any figure you please, there will be a corresponding figure drawn by the second point: for a good pentagraph a copy on a different scale (it may be); for a badly-adjusted pentagraph, a distorted copy; but the one figure will always be a sort of copy of the first, so that to each point of the one figure there will correspond a point in the other figure.

In the case above referred to, where one point represents the value $x + iy$ of the imaginary variable and the other the value $X + iY$ of some function $\phi(x + iy)$ of that variable, there is a remarkable relation between the two figures: this is the relation of orthomorphic projection, the same which presents itself between a portion of the earth's surface and the representation thereof by a map on the stereographic projection or on Mercator's projection—viz., any indefinitely small area of the one figure is represented in the other figure by an indefinitely small area of the same shape. There will possibly be for different parts of the figure great variations of scale, but the shape will be unaltered; if for the one area the boundary is a circle, then

for the other area the boundary will be a circle; if for one it is an equilateral triangle, then for the other it will be an equilateral triangle.

I have been speaking of an imaginary variable $(x + iy)$, and of a function $\phi(x + iy) = X + iY$ of that variable, but the theory may equally well be stated in regard to a plane curve: in fact the $x + iy$ and the $X + iY$ are two imaginary variables connected by an equation; say their values are u and v, connected by an equation $F(u, v) = 0$; then, regarding u, v as the coordinates of a point *in plano*, this will be a point on the curve represented by the equation. The curve, in the widest sense of the expression, is the whole series of points, real or imaginary, the coordinates of which satisfy the equation, and these are exhibited by the foregoing corresponding figures in two planes; but in the ordinary sense the curve is the series of real points, with coordinates u, v, which satisfy the equation.

In geometry it is the curve, whether defined by means of its equation, or in any other manner, which is the subject for contemplation and study. But we also use the curve as a representation of its equation—that is, of the relation existing between two magnitudes x, y, which are taken as the coordinates of a point on the curve. Such employment of a curve for all sorts of purposes—the fluctuations of the barometer, the Cambridge boat races, or the Funds—is familiar to most of you. It is in like manner convenient in analysis, for exhibiting the relations between any three magnitudes x, y, z, to regard them as the coordinates of a point in space; and, on the like ground, we should at least wish to regard any four or more magnitudes as the coordinates of a point in space of a corresponding number of dimensions. Starting with the hypothesis of such a space, and of points therein each determined by means of its coordinates, it is found possible to establish a system of n-dimensional geometry analogous in every respect to our two- and three-dimensional geometries, and to a very considerable extent serving to exhibit the relations of the variables.

It is to be borne in mind that the space, whatever its dimensionality may be, must always be regarded as an imaginary or complex space such as the two- or three-dimensional space of ordinary geometry; the advantages of the representation would otherwise altogether fail to be obtained.

I omit some further developments in regard to geometry; and all that I have written as to the connection of mathematics with the notion of time.

I said that I would speak to you, not of the utility of the mathematics in any of the questions of common life or of physical science, but rather of the obligations of mathematics to these different subjects. The consideration which thus presents itself is in a great measure that of the history of the development of the different branches of mathematical science in connection with the older physical sciences, astronomy and mechanics: the mathematical theory is in the first instance suggested by some question of common life or of physical science, is pursued and studied quite independently thereof, and perhaps after a long interval comes in contact with it, or with quite a different question. Geometry and algebra must, I think, be considered as each of them originating in connection with objects or questions of common life—geometry, notwithstanding its name, hardly in the measurement of land, but rather from the contemplation of such forms as the straight line, the circle, the ball, the top (or sugar-loaf): the Greek geometers appropriated for the geometrical forms corresponding to the last two of these, the words σφαῖρα and κῶνος, our cone and sphere, and they extended the word cone to mean the complete figure obtained by producing the straight lines of the surface both ways indefinitely. And so algebra would seem to have arisen from the sort of easy puzzles in regard to numbers which may be made, either in the picturesque forms of the Bija-Ganita with its maiden with the beautiful locks, and its swarms of bees amid the fragrant blossoms, and the one queen bee left humming around the lotus flower; or in the more prosaic form in which a student has presented to him in a modern text-book a problem leading to a simple equation.

The Greek geometry may be regarded as beginning with Plato (B.C. 430–347): the notions of geometrical analysis, loci, and the conic sections are attributed to him, and there are in his "Dialogues" many very interesting allusions to mathematical questions: in particular the passage in the "Theætetus," where he affirms the incommensurability of the sides of certain squares. But the earliest extant writings are those of Euclid (B.C. 285): there is hardly anything in mathematics more beautiful than his wondrous fifth book; and he has also in the seventh, eighth, ninth, and tenth books fully and ably developed the first principles of the Theory of Numbers, including the theory of incommensurables. We have next Apollonius (about B.C. 247), and Archimedes (B.C. 287–212), both geometers of the highest merit, and the latter of them the founder of the science of statics (including therein hydrostatics): his dictum about the lever, his "Εὕρηκα," and the story of the defence of Syracuse, are well known. Following these we have a worthy series of names, including the astronomers Hipparchus (B.C. 150) and Ptolemy (A.D. 125), and ending, say, with Pappus (A.D. 400), but continued by their Arabian commentator , and the Italian and other European geometers of the sixteenth century and later, who pursued the Greek geometry.

The Greek arithmetic was, from the want of a proper notation, singularly cumbrous and difficult; and it was for astronomical purposes superseded by the sexagesimal arithmetic, attributed to Ptolemy, but probably known before his time. The use of the present so-called Arabic figures became general among Arabian writers on arithmetic and astronomy about the middle of the tenth century, but it was not introduced into Europe until about two centuries later. Algebra among the Greeks is represented almost exclusively by the treatise of Diophantus (A.D. 150), in fact a work on the Theory of Numbers containing questions relating to square and cube numbers, and other properties of numbers, with their solutions; this has no historical connection with the later algebra introduced into Italy from the East by Leonardi Bonacci of Pisa (A.D. 1202–1208), and successfully cultivated in the fifteenth and sixteenth centuries by Lucas Paciolus, or De Burgo, Tartaglia, Cardan, and Ferrari. Later on we have Vieta (1540–1603), Harriot, already referred to, Wallis, and others.

Astronomy is of course intimately connected with geometry; the most simple facts of observation of the heavenly bodies can only be *stated* in geometrical language; for instance, that the stars describe circles about the Pole-star, or that the different positions of the sun among the fixed stars in the course of the year form a circle. For astronomical calculations it was found necessary to determine the arc of a circle by means of its chord; the notion is as old as Hipparchus, a work of whom is referred to as consisting of twelve books on the chords of circular arcs; we have (A.D. 125) Ptolemy's "Almagest," the first book of which contains a table of arcs and chords with the method of construction; and among other theorems on the subject he gives there the theorem afterwards inserted in Euclid (Book VI. Prop. D.) relating to the rectangle contained by the diagonals of a quadrilateral inscribed in a circle. The Arabians made the improvement of using in place of the chord of an arc the sine, or half chord of double the arc, and so brought the theory into the form in which it is used in modern trigonometry: the before-mentioned theorem of Ptolemy, or rather a particular case of it, translated into the notation of sines, gives the expression for the sine of the sum of two arcs in terms of the sines and cosines of the component arcs; and it is thus the fundamental theorem on the subject. We have in the fifteenth and sixteenth centuries a series of mathematicians who with wonderful enthusiasm and perseverance calculated tables of the trigonometrical or circular functions, Purbach, Müller or Regiomontanus, Copernicus, Reinhold, Maurolycus, Vieta, and many others; the tabulations of the functions tangent and secant are due to Reinhold and Maurolycus respectively.

Logarithms were invented, not exclusively with reference to the calculation of trigonometrical tables, but in order to facilitate numerical calculations generally; the invention is due to John Napier of Merchiston, who died in 1618 at sixty-seven years of age; the notion was based upon refined mathematical reasoning on the comparison of the spaces described by two points, the one moving with a uniform velocity, the other with a velocity varying according to a given law. It is to be observed that Napier's logarithms were nearly but not exactly those which are now called (sometimes Napierian, but more usually) hyperbolic logarithms—those to the base e; and that the change to the base 10 (the great step by which the invention was perfected for the object in view) was indicated by Napier but actually made by Henry Briggs, afterwards Savilian Professor at Oxford (d. 1630). But it is the hyperbolic logarithm which is mathematically important. The direct function e^x or exp. x, which has for its inverse the hyperbolic logarithm, presented itself, but not in a

prominent way. Tables were calculated of the logarithms of numbers, and of those of the trigonometrical functions.

The circular function and the logarithm were thus invented each for a practical purpose, separately and without any proper connection with each other. The functions are connected through the theory of imaginaries, and form together a group of the utmost importance throughout mathematics: but this is mathematical theory; the obligation of mathematics is for the discovery of the functions.

Forms of spirals presented themselves in Greek architecture, and the curves were considered mathematically by Archimedes; the Greek geometers invented some other curves, more or less interesting, but recondite enough in their origin. A curve which might have presented itself to anybody, that described by a point in the circumference of a rolling carriage wheel, was first noticed by Mersenne in 1615, and is the curve afterwards considered by Roberval, Pascal, and others, under the name of the Roulette, otherwise the Cycloid. Pascal (1623-1662) wrote at the age of seventeen his "Essais pour les Coniques," in seven short pages, full of new views on these curves, and in which he gives, in a paragraph of eight lines, his theory of the inscribed hexagon.

Kepler (1571-1630) by his empirical determination of the laws of planetary motion, brought into connection with astronomy one of the forms of conic, the ellipse, and established a foundation for the theory of gravitation. Contemporary with him, for most of his life, we have Galileo (1564-1642), the founder of the science of dynamics; and closely following upon Galileo, we have Isaac Newton (1643-1727): the "Philosophiæ naturalis Principia Mathematica," known as the "Principia," was first published in 1687.

The physical, statical, or dynamical questions which presented themselves before the publication of the "Principia" were of no particular mathematical difficulty, but it is quite otherwise with the crowd of interesting questions arising out of the theory of gravitation, and which, in becoming the subject of mathematical investigation, have contributed very much to the advance of mathematics. We have the problem of two bodies, or what is the same thing, that of the motion of a particle about a fixed centre of force, for any law of force; we have also the (mathematically very interesting) problem of the motion of a body attracted to two or more fixed centres of force; then, next preceding that of the actual solar system—the problem of three bodies; this has ever been and is far beyond the power of mathematics, and it is in the lunar and planetary theories replaced by what is mathematically a different problem, that of the motion of a body under the action of a principal central force and a disturbing force; or (in one mode of treatment) by the problem of disturbed elliptic motion. I would remark that we have here an instance in which an astronomical fact, the observed slow variation of the orbit of a planet, has directly suggested a mathematical method, applied to other dynamical problems, and which is the basis of very extensive modern investigations in regard to systems of differential equations. Again, immediately arising out of the theory of gravitation, we have the problem of finding the attraction of a solid body of any given form upon a particle, solved by Newton in the case of a homogeneous sphere, but which is far more difficult in the next succeeding cases of the spheroid of revolution (very ably treated by Maclaurin) and of the ellipsoid of three unequal axes: there is perhaps no problem of mathematics which has been treated by as great a variety of methods, or has given rise to so much interesting investigation as this last problem of the attraction of an ellipsoid upon an interior or exterior point. It was a dynamical problem, that of vibrating strings, by which Lagrange was led to the theory of the representation of a function as the sum of a series of multiple sines and cosines; and connected with this we have the expansions in terms of Legendre's functions P_n, suggested to him by the question just referred to of the attraction of an ellipsoid; the subsequent investigations of Laplace on the attractions of bodies differing slightly from the sphere led to the functions of two variables called Laplace's functions. I have been speaking of ellipsoids, but the general theory is that of attractions, which has become a very wide branch of modern mathematics; associated with it we have in particular the names of Gauss, Lejeune-Dirichlet, and Green; and I must not omit to mention that the theory is now one relating to n-dimensional space. Another great problem of celestial mechanics, that of the motion of the earth about its centre of gravity, in the most simple case, that of a body not acted upon by any forces, is a very interesting one in the mathematical point of view.

I may mention a few other instances where a practical or physical question has connected itself with the development of mathematical theory. I have spoken of two map projections—the stereographic, dating from Ptolemy; and Mercator's projection, invented by Edward Wright about the year 1600: each of these, as a particular case of the orthomorphic projection, belongs to the theory of the geometrical representation of an imaginary variable. I have spoken also of perspective, and (in an omitted paragraph) of the representation of solid figures employed in Monge's descriptive geometry. Monge, it is well known, is the author of the geometrical theory of the curvature of surfaces and of curves of curvature: he was led to this theory by a problem of earthwork—from a given area, covered with earth of uniform thickness, to carry the earth and distribute it over an equal given area, with the least amount of cartage. For the solution of the corresponding problem in solid geometry he had to consider the intersecting normals of a surface, and so arrived at the curves of curvature (see his "Mémoire sur les Déblais et les Remblais," *Mém. de l'Acad.*, 1781). The normals of a surface are, again, a particular case of a doubly infinite system of lines, and are so connected with the modern theories of congruences and complexes.

The undulatory theory of light led to Fresnel's wave-surface, a surface of the fourth order, by far the most interesting one which had then presented itself. A geometrical property of this surface, that of having tangent planes each touching it along a plane curve (in fact, a circle), gave to Sir W. R. Hamilton the theory of conical refraction. The wave-surface is now regarded in geometry as a particular case of Kummer's quartic surface, with sixteen conical points and sixteen singular tangent planes.

My imperfect acquaintance as well with the mathematics as the physics prevents me from speaking of the benefits which the theory of Partial Differential Equations has received from the hydrodynamical theory of vortex motion, and from the great physical theories of electricity, magnetism, and energy.

It is difficult to give an idea of the vast extent of modern mathematics. This word "extent" is not the right one: I mean extent crowded with beautiful detail—not an extent of mere uniformity, such as an objectless plain, but of a tract of beautiful country seen at first in the distance, but which will bear to be rambled through and studied in every detail of hill-side and valley, stream, rock, wood, and flower. But, as for anything else, so for a mathematical theory—beauty can be perceived, but not explained. As for mere extent, I might illustrate this by speaking of the dates at which some of the great extensions have been made in several branches of mathematical science.

And in fact, in the Address as written, I speak at considerable length of the extensions in geometry since the time of Descartes, and in other specified subjects since the commencement of the century: these subjects are the general theory of the function of an imaginary variable; the leading known functions, viz. the elliptic and single theta-functions and the Abelian and multiple theta-functions; the Theory of Equations and the Theory of Numbers. I refer also to some theories outside of ordinary mathematics: the multiple algebra or linear associative algebra of the late Benjamin Peirce; the theory of Argand, Warren, and Peacock in regard to imaginaries in plane geometry; Sir W. R. Hamilton's quaternions, Clifford's biquaternions, the theories developed in Grassmann's "Ausdehnungslehre," with recent extensions thereof to non-Euclidian space by Mr. Homersham Cox; also Boole's "Mathematical Logic," and a work connected with logic, but primarily mathematical and of the highest importance, Schubert's "Abzählende Geometrie" (1878). I remark that all this in regard to theories outside of ordinary mathematics is still on the text of the vast extent of modern mathematics.

In conclusion I would say that mathematics have steadily advanced from the time of the Greek geometers. Nothing is lost or wasted; the achievements of Euclid, Archimedes, and Apollonius are as admirable now as they were in their own days. Descartes' method of coordinates is a possession for ever. But mathematics have never been cultivated more zealously and diligently, or with greater success, than in this century—in the last half of it, or at the present time: the advances made have been enormous, the actual field is boundless, the future full of hope. In regard to pure mathematics we may most confidently say:—

"Yet I doubt not through the ages one increasing purpose runs,
And the thoughts of men are widened with the process of the suns."

SECTION A

MATHEMATICAL AND PHYSICAL

Opening Address by Prof. Olaus Henrici, Ph.D., F.R.S., President of the Section.

On reading through the addresses delivered by my predecessors in this office, I was struck by the fact that in nearly every case the speaker began with a lamentation over his unfitness for the work before him, and those seemed to me to be the more eloquent on these points who showed by their address that they least needed an excuse. The amount of excuse given appears in fact to be directly proportional to the gifts of the speaker, and hence inversely proportional to the need of such an excuse.

Under these circumstances I cannot express my sense of my own unfitness for this post better than by saying nothing about it. I must, however, beg your indulgence for my shortcomings, both as regards my address and my manner of conducting the general business of this section.

As the Presidential chair is occupied by one of the most illustrious of mathematicians, it would be presumptuous for me to attempt to give an account of the recent progress of mathematics. I propose only to speak for a short time on that part of mathematics which has always been most attractive to myself—that is, pure geometry as apart from algebra, but I shall confine myself to some considerations relating to the teaching of geometry in this country. Pure geometry seems to me to be of the greatest educational value, and almost indispensable in many applications; but it has scarcely ever been introduced at Cambridge, the centre of mathematics and mathematical education in England.

The number of geometrical methods now in use is astonishingly great. These differ, on the one hand, according to the nature of the result aimed at, but, on the other, according to the amount of algebra employed, and to the relation in which this algebra stands to the pure "*Anschauung*." I use the word *Anschauung* because I know of no English equivalent; the German word has the philosophic meaning rendered by intuition, and retains its original concrete meaning of looking at a thing, and might perhaps be translated : intuition by inspection. It is the inspection of figures which is of the greatest importance in geometry. It is hereby of little consequence whether the figures are seen by the physical eye or only mentally ; because the conception of that space in which we perceive everything and without which we can perceive nothing, which therefore is, according to Kant, a form of our *Anschauung*, is built up in our mind through many generations in conformity with sensual impressions.

It would be of interest, if time permitted, to follow up the gradual development and extension of geometry into the wider science of algebra, from the first introduction of the latter in the theory of proportion to the present state, where there exists really no essential difference between the two, where geometry is only one manifestation of algebra, but so complete a one that at least within its number of dimensions it again contains algebra.

In some of the methods just referred to no algebra is used at all, whilst others may be distinguished according to the nature of the algebra used, whether equations containing one, two, three, or more variables are employed. In such a division, Von Staudt's system, without a vestige of algebra, would occupy the one end, and the purely algebraical theory of invariants with geometrical interpretation the other.

There is, however, not only a difference in the amount of algebra used, but, if possible, a greater one in the manner in which the symbols are interpreted. And it is here that algebra has apparently the greater power. One algebraical theorem, by being read in different ways, by giving ever different meanings to the symbols, reveals a variety of geometrical and other theorems. We have in it the crystallised form, the very essence of the mathematical truth, but in the most abstract form conceivable. Now this most abstract form is the highest and the most perfect which mathematical truth as such can assume, and which it must assume before a theory is really complete in the eyes of a pure mathematician. It is only in this shape that it is ready to be turned to account in any direction where it may be needed.

In thus placing algebra on the highest pinnacle, the reasons will be apparent which will make many mathematicians, not to mention others, prefer the truths it reveals cast in a mould which connects them with concrete things rather than with abstract notions. In fact, to be thoroughly at home in the highest theories of pure algebra requires some of the genius of men like Cayley and Sylvester who have founded, and to a great extent built up, modern algebra. But even they constantly make use of geometry to assist them in their investigations, and no one could have expressed this more strongly than Prof. Sylvester himself in his brilliant address delivered from this chair at the Exeter meeting of our Association.

If this is so, surely every progress in the spread of the knowledge of pure geometry should be welcomed and encouraged ; but in England pure geometry is almost unknown excepting in the elements as contained in Euclid and in the old-fashioned geometrical conics. The modern methods of synthetic projective geometry as developed on the Continent have never become generally known here. The few men who have thoroughly made themselves acquainted with them, and who have preferred purely geometrical reasoning, have not belonged to Cambridge, and have thus stood somewhat outside the national system of training mathematical teachers. The late Prof. Smith introduced these methods at Oxford, and there was some expectation that he would have written, if he had been spared, a text-book which might have done much to introduce the subject more widely. His principal mathematical work lay, however, in another direction.

The one English mathematician whose mathematical thought is purely geometrical is Dr. Hirst, a pupil of Steiner, who in the position which he has just relinquished has been able to introduce, as the first, modern geometrical methods into a regular system of professional education, whilst showing at the same time by his original work what can be done with these methods.

Other mathematicians who have studied these methods—and I believe there are many—have made use of them by translating the geometrical into algebraical reasoning.

Towards the early possibility of such a translation much was done by the labours of the late Mr. Spottiswoode, who years ago wrote the first connected treatise on the theory of determinants, and who up to the last few years employed some of his leisure hours in working out geometrical problems, the work consisting always of some beautiful piece of algebra.

It is not often that our Section has to mourn in one year the loss of two such men as Smith and Spottiswoode.

It is easy to see how the neglect complained of has come to pass. In England when mathematics, after having lain dormant for about a century, began to revive, the first necessity was to become acquainted with the enormous amount of work meanwhile done on the Continent. This acquaintance was made through France, at that time nearly all the standard works being in the French language, which was at the same time the language best known to English students. The subjects principally taken up were the calculus and its application to mechanics. And I believe I am not far wrong when I say that the wonderful writings of Lagrange, with their extraordinary analytical elegance, had the greatest influence. But in his works anything geometrical was studiously avoided. Lagrange prided himself that there was no figure in his "Mécanique analytique."

The best analytical methods of the Continent were thus introduced into England, rapidly assimilated and made the foundation of new theories, so that the mathematical activity in this country is now at least as great as it ever has been anywhere.

But whilst analysis, algebra, and with it analytical geometry, made rapid progress, pure geometry was not equally fortunate. Here the hold which Euclid had long obtained, strengthened, no doubt, by Newton's example, prevented any change in the methods of teaching.

Most of all, perhaps, solid geometry has suffered, because Euclid's treatment of it is scanty, and it seems almost incredible that a great part of it—the mensuration of areas of simple curved surfaces and of volumes of simple solids—is not included in ordinary school teaching. The subject is, possibly, mentioned in arithmetic, where, under the name of mensuration, a number of rules are given. But the justification of these rules is not supplied, except to the student who reaches the application of the integral calculus; and what is almost worse is that the general relations of points, lines, and planes, in space, is scarcely touched upon, instead of being fully impressed on the student's mind.

The methods for doing this have long been developed in the new geometry which originated in France with Monge. But these have never been thoroughly introduced.

Works written in the German language naturally received even less attention. But it was in Germany, at the beginning of the second quarter of this century, that geometry received at the hands of several masters an impulse which put the subject on an entirely new footing.

I may mention here especially four men of whom each invented a new method and established a new system of geometry. Two of these, Möbius and Plücker, still use algebra, but in perfectly new and original manners, which, although very different from each other, have this in common, that in both we have not algebra interpreted geometrically, but rather geometry veiled in an algebraic garb. The geometrical meaning is never lost sight of.

But perfectly independent of algebra was the great Steiner, the greatest geometrician since the times of Euclid, Appolonius, and Archimedes. In his celebrated "Systematische Entwickelungen" he has laid the foundation of a pure geometry, on which a wonderful edifice has since been raised. His treatment of the principle of duality, and his method of generating conics by projective, or homographic, rows of pencils which have been extended to curves of all degrees, have given to geometrical reasoning a generality never before dreamed of. He is in one respect the opposite of Lagrange, hating and despising analysis as much as ever Lagrange disliked pure geometry. Steiner started from the geometry of the Greeks, Euclid's elements, and a few other *metrical* properties he takes for granted; but then he goes on with essentially modern methods of his own to investigate what are now called projective properties of curves and surfaces.

This metrical foundation Von Staudt changed. In his "Geometrie der Lage," published fifteen years after Steiner's "Entwickelungen," he established a most remarkable and complete system, into which the notion of a magnitude does not enter at all. He shows that projective properties of figures, which have no relation whatsoever to measurements, can be established without any mention of them. He goes so far as even to give a geometrical definition of a number in its relation to geometry as determining the position of a point, in his theory of what he calls "Würfe"; and one of the most interesting parts of his work is the purely geometrical treatment of imaginary points, lines, and planes.

In the hands of these men, and since their time, pure geometry has become a most important instrument for research, rivalling in power the more or less algebraical methods, and surpassing them all in the manner in which they raise before the mind's eye a clear realisation of the forms and figures which are the object of the investigation.

In close connection with these methods stand descriptive geometry and geometrical drawing, which teach how to represent figures on a plane or other surface. These have been treated as arts unknown at English universities, and relegated to the drawing office. Instead of this they ought to be an essential and integral part of the teaching of geometry in connection with the purely geometrical methods.

As far as the progress of science is concerned, this neglect of pure geometry in England has been of little consequence—perhaps it has rather been a gain. For science itself it is often an advantage that a centre of learning becomes one-sided, neglects many parts in order to concentrate all its energy on some particular points and make rapid progress in the directions in which these lie. At present, when mathematics flourishes as never before, when almost every nation, however small, has its eminent mathematician, there are so many such centres that what is neglected at one place is pretty surely taken up and advanced at another. But what may suffer if one side of a science is not cultivated in a country is the industry which would have gained by its applications.

In considering the teaching of any mathematical or other scientific subject, we cannot at the present time neglect the wants of the ever-increasing class of men who require what has been called technical education. Among these the large number who want mathematics at all require geometry much more than algebra and analysis, and geometry as applied to drawing and mensuration.

This want has been supplied by the numerous science classes spread over the country, with their head-quarters at the Science and Art Department at South Kensington, whose examinations—now, however, put in competition with those of the City and Guilds of London Institute, and others—have pretty much guided and regulated the teaching. A great deal of good has thus been done, but there is still much room for improvement. The teaching of geometry especially, as judged by the text-books which have come before me, is somewhat deplorable. And this is so, principally, because the spirit of Euclid and the methods of the ancient Egyptians and Greeks, rather than the fundamentally different ideas and methods of modern geometry, still rule supreme; though the latter have had their origin partly in technical wants.

In what is called geometrical drawing or practical geometry, for instance, there are first given a number of elementary constructions—such as drawing parallels and perpendiculars, or bisecting the distance between two given points. They are solved by aid of those instruments only which Euclid knew—viz. the pair of compasses for drawing circles, and the straight edge for drawing straight lines. But there is no draughtsman who would not, as a matter of course, use set squares for the former problem, and solve the latter by trial rather than by construction. Then again there come constructions like the division of the circumference of a circle into seven parts, which cannot be solved accurately, but which is very easily solved by trial. Instead of that, a *construction* is given which takes much more time, and is by no means more accurate. For, after all, our lines drawn on the paper are not without thickness, so that, for this reason alone, every part of the construction is affected by some small error; and it is absurd to employ a construction, though theoretically true for ideal figures as conceived in our mind, in preference to a much simpler one which, within our practical limits, is equally, or perhaps more, correct.

This is very much like the manner in which I found problems on decimal fractions treated by the candidates for the Matriculation Examination at the London University, and which reflected little credit on the manner in which the important subject of decimals is handled at our schools. It is so characteristic that I may be excused for giving it here. The problem, for instance, being to give the product of two decimal fractions, exact to, say, four decimals, each of the factors having the same number of places. This was almost regularly performed as follows. First, the decimals are converted into vulgar fractions, then these are duly multiplied, numerator by numerator, and denominator by denominator, and then the resulting fraction is again converted to a decimal, with as many places as it may yield, and, lastly, of these the first four are taken and put down, duly marked *Answer*. Or a candidate, standing however on a far higher level, multiplies both decimals out in the proper fashion, but to eight places, and cuts off four places at the end. No wonder that the public at large will hear nothing of the decimal system of weights and measures if the very essence of the decimal system of numbers is so little understood by the men who have to train the minds of the young generation!

I need scarcely say that I do not mean to blame the Science and Art Department, far less the teachers who have simply to follow suit. They act up to their light, and cannot be expected to introduce methods which are practically unknown at Cambridge, and of which the only good text-books are in foreign languages—books which are probably not at all suitable for introduction into our schools without considerable change.

It is satisfactory to learn that an association has recently been formed under the presidency of Prof. Huxley "to effect the general advancement of the profession of science and art teaching by securing improvements in the schemes of study, and the establishment of satisfactory relations between teachers and the Science and Art Department, the City and Guilds of London Institute, and other public authorities."

The good wishes of all who have the cause of sound education at heart must go with such an undertaking, one of the principal aims of which seems to be to save teaching from being any longer enslaved by examinations, and to promote greater accord between the teacher and the examiner. It is to be hoped that this association will consider geometry as one of the subjects included under the designation of science.

It is by the neglect of pure geometry and its applications to geometrical drawing that Cambridge has lost, or rather has never had, contact with the practical needs of the nation. All the marvels of modern engineering have sprung into existence without its help. The great engineers have had to depend to a degree, now unheard of, upon costly experiments, until they themselves gradually discovered mathematical methods adapted to their purposes.

Only the electrical engineer found ready to his hands a complete theory of which the mathematical part has been to a very great extent developed at Cambridge, or by men who have had their mathematical training there. This theory is, however, in its very nature less geometrical. One at least of the great men to whom the present theory of electricity is due, the late Clerk Maxwell, had the keenest appreciation of the value of modern geometry. I remember a characteristic letter of his being read to the Council of the London Mathematical Society, in which

the writer, forgetting the subject of his letter, burst out into an enthusiastic praise of a German text-book, the "Geometrie der Lage," by Reye, through which Maxwell, evidently for the first time, got any idea of this subject.

The engineer will always prefer geometrical methods to analysis, and has invented for himself a great variety of them. Originally these are disjointed, being invented for special purposes. It is the business of the mathematician afterwards to connect, simplify, and extend them, as has been done to a great extent by Culmann in Zürich, or by Cremona at the Polytechnic School at Rome.

Of these methods a few may be mentioned. First of all the graphical determination of stresses in certain girders invented both by mathematicians and by engineers. Its application is so simple that no engineer will ever use any other method if once he knows this one. It is so well adapted to its purpose, that I venture to say that a simpler method is impossible, being fully aware how dangerous such a statement is. Nay, if I were asked to give the formulæ to obtain the stresses by calculation, I should write these down from a sketch of the diagram, this being the simplest way of obtaining them.

Another problem which recurs again and again is the determination of the area of a figure representing perhaps a plot of land or the section of a beam. Here also the advantage is altogether on the side of the graphical method.

It is unnecessary to multiply these examples. But to make full use of graphical methods the draughtsman ought to have a thoroughly geometrical education. For instance, the real nature of the reciprocal diagrams already mentioned is only understood by aid of a peculiar reciprocal relation between points and planes in space closely connected with the theory of the linear complex, as has been shown by Cremona.

I have mentioned already the "Analytical Mechanics" of Lagrange, which is without any trace of geometry, although there is scarcely a branch of applied mathematics which is in its very nature more geometrical. In fact one part of it, now separated as kinematics, treats solely of changes in position and shape of geometrical quantities, and differs from pure geometry only in this, that the changes are considered as referring not to space alone, but also to time.

What mechanics gains by introducing geometry to the full will be apparent to all who have become acquainted with modern Continental text-books on the subject.

Let us compare the analytical with the geometrical reduction of a system of forces acting on a rigid body, or, to use Clifford's nomenclature, the reduction of a system of rotors, which may represent either forces or rotations, or any other quantities which have certain fundamental properties in common with those, so that they may be represented by rotors. In the analytical process the system is reduced to a rotor and a vector, that is a resultant force and a couple. In the geometrical treatment we see that this is only one way of reducing the rotors to two, viz. the one which is best fitted to be treated by analysis. But there is a multitude of other reductions. These all appear as of equal importance in the geometrical method. Furthermore, this method shows us in the simplest way possible how all the line pairs which may be the lines of action of two resultant rotors, although there are infinities of infinities of such pairs, are arranged in space, so that one gets a clear picture of all these reductions in one's mind.

Again, compare Möbius's geometrical investigation of the rays of light passing through a system of lenses with that of Gauss, whose very name suggests simplicity and elegance. The celebrated "cardinal points" appear in Gauss's original paper as the result of a somewhat long though certainly elegant analysis, whilst by Möbius they are the natural outcome of his geometry, so that any student once started on this method is bound to come across these points, or rather across pairs of points, of which the cardinal points of Gauss are only one special case. The whole is, in fact, contained in the following easily proved proposition: The rays of light starting from a point in the axis of the system before entering the first lens, and after leaving the last, form two homographic pencils in perspective position.

This is only one small part of the advantage which optics can derive from geometry.

That the old-established mode of teaching the elements of geometry based on Euclid requires a thorough and fundamental change has been often acknowledged, among others, at Exeter and Bradford, by two of the most eminent mathematicians who have occupied this chair, and besides by the many teachers who constitute the Association for the Improvement of Geometrical Teaching, which itself grew out of the action of our Section. I know, therefore, of no opportunity better suited to review the progress made in this direction than the present one, as the subject has on several occasions occupied the attention of our Section. Nevertheless I have hesitated on entering on this somewhat delicate question, because I fear that I have little to offer but criticism, which might seem hostile to the association just named. But I hope that the many earnest workers who have devoted much time and thought to the drawing up of syllabuses on different parts of our subject will excuse the remarks of one who has himself tried his hand at the same work, and who therefore may be supposed somewhat to know the difficulties that have to be overcome.

When the syllabus on the elements of plane geometry appeared, I resolved to give it a thorough trial, and took the best means in my power to form an opinion on its merits by introducing it into one of my classes. The fact that it did not quite satisfy me, and that I gave up its use again, does not of course prove that it fails also for use in schools, for which it was originally intended.

Let me add that the more I have become acquainted with the difficulty of the whole subject the greater has become my admiration for Euclid's book, whilst my conviction of its unfitness as a school book has equally gained in strength.

In considering the merits of Euclid as a text-book it is desirable to distinguish clearly between the general educational value of its teaching and the gain of geometrical knowledge. It is with the latter chiefly that I am concerned, whilst it is of course through the former that Euclid has got so firm a hold at all schools; and to the great majority of boys this is undoubtedly of most importance, and no reform would have the slightest chance of becoming generally introduced which neglects this. But improvement in both directions may well go together, and the logical reasoning employed in Euclid would gain to many boys much, both in clearness and interest, if the subject-matter reasoned about became in itself better understood.

Probably a great deal could be done by introducing some of the elements of logic into the teaching of language. I have been assured by an eminent scholar that the laws of forming a sentence—the fact that a sentence in its simplest form consists of subject, object, and copula—was not explained in English schools. If this grammatical part of logic were properly treated of in connection with language, and if at the same time acquaintance with geometrical objects, particularly through the medium of geometrical drawing and the many methods used in the Kinder-Gartens, were more secured, then a systematic course of geometry would become both easier and more useful.

Much indeed may be done by introducing simple geometrical teaching into the nursery, and into the earliest instruction of children, following the example of the Kinder-Gartens, and it is pleasing to see that the latter are rapidly gaining ground in England. It is true that these schools may still be improved. In geometry they seem to, and perhaps at present are bound to, work mostly towards Euclid. But many able men and women are actively engaged in perfecting them, and it is of interest to know that Clifford had it in his mind to write a geometry for the nursery and the Kinder-Garten.

In a curious contrast to the mode of teaching geometry stands that of teaching algebra. In the first everything is sacrificed to logic. Axioms and definitions without end are given, though to the beginner a more rapid dive into the subject would be much more suitable. In algebra, on the other hand, the boy is at once plunged into the midst of it. No axiom is mentioned. A number of rules are stated, and the schoolboy is made to practise them mechanically until he can perform, and that often with considerable skill, a number of most complicated calculations—but calculations which are often of very little use for actual applications. Simplifications of equations follow in senseless monotony, until the poor fellow really thinks that solving a simple equation does not mean the finding of a certain number which satisfies the equation, but the going mechanically through a certain regular process which at the end yields some number. The connection of that number with the original equation remains to his mind somewhat doubtful. Then there are processes, like the finding of the G.C.M., which most of the boys never have any opportunity of using, excepting, perhaps, in the examination room. A more rational treatment of the subject, introducing from the beginning reasoning rather than calculation, and applying the results obtained to various problems taken from all parts of science as well as from everyday life, would be more interestin

to the student, give him really useful knowledge, and would be at the same time of true educational value.

The chief progress in geometrical teaching has to be sought in the introduction of modern ideas and methods into the very elements, and modern teaching ought to take full account of this.

In favour of this view I might bring forward the opinions of many teachers and mathematicians from England as well as from abroad, but I will confine myself to one quotation. Prof. Sylvester gives his opinion thus:—"I should rejoice to see mathematics taught with that life and animation which the presence and example of her young and buoyant sister (viz. natural and experimental science) could not fail to impart, short roads preferred to long ones, Euclid honourably shelved or buried 'deeper than did ever plummet sound' out of the schoolboy's reach, morphology introduced into the elements of algebra—projection, correlation, motion accepted as aids to geometry—the mind of the student quickened and elevated and his faith awakened by early initiation into the ruling ideas of polarity, continuity, infinity, and familiarisation with the doctrine of the imaginary and inconceivable. It is this living interest in the subject which is so wanting in our traditional and mediæval modes of teaching."

If from this point of view we now look towards the work of the Association for the Improvement of Geometrical Teaching, the result is not as satisfactory as might have been wished. There is very little of the influence of modern ideas to be found in the different syllabuses which have been published. Even in the one headed "Modern Geometry" there is nothing of the genius of modern thought. The subject-matter is partly taken from modern geometry, but for modern methods one looks in vain. In the geometrical conics, too, one would like to see Steiner's generation of conics, but of these there is no trace.

Nevertheless it is satisfactory to see that the use of the syllabus on plane geometry has spread pretty widely, and it is to be hoped that it will continue to do so. A thorough reform in the direction indicated will be a difficult task, and it will perhaps be a long time before it is possible. At present it has not even been settled which series of axioms will ultimately be adopted. Of the various systems which have been proposed since the investigations of Riemann and Helmholtz, I may mention here Clifford's suggestion to replace Euclid's axiom about parallels by the new one, which maintains that in a plane similar figures exist, or, more completely, that at any part in a plane a figure is possible which is similar to any given figure in that plane. This axiom is somewhat startling as long as we have the usual theory of similar figures in our mind. But the notion of similar figures is truly axiomatic, and it has lately become my conviction that this axiom may be extremely fruitful, and the working out of a syllabus of plane geometry based on it would be very desirable.

Possibly many such attempts have still to be made before a new Euclid finds the materials sufficiently prepared for him to raise the hoped-for edifice.

SECTION B

CHEMICAL SCIENCE

OPENING ADDRESS BY J. H. GLADSTONE, PH.D., F.R.S., V.P.C.S., PRESIDENT OF THE SECTION.

A SECTIONAL address usually consists either of a review of the work done in the particular science during the past year, or of an exposition of some branch of that science to which the speaker has given more especial attention. I propose to follow the latter of these practices, and shall ask the indulgence of my brother chemists while I endeavour to place before them some thoughts on the subject of Elements.

Though theoretical and practical chemistry are now intertwined, with manifest advantage to each, they appear to have been far apart in their origin. Practical chemistry arose from the arts of life, the knowledge empirically and laboriously acquired by the miner and metallurgist, the potter and the glass-worker, the cook and the perfumer. Theoretical chemistry derived its origin from cosmogony. In the childhood of the human race the question was eagerly put, "By what process were all things made?" and some of the answers given started the doctrine of elements. The earliest documentary evidence of the idea is probably contained in the Shoo King, the most esteemed of the Chinese classics for its antiquity. It is an historical work, and comprises a document of still more venerable age, called "The Great Plan, with its Nine Divisions," which purports to have been given by Heaven to the Great Yu, to teach him his royal duty and "the proper virtues of the various relations." Of course there are wide differences of opinion as to its date, but we can scarcely be wrong in considering it as older than Solomon's writings. The First Division of the Great Plan relates to the Five Elements. "The first is named Water; the second, Fire; the third, Wood; the fourth, Metal; the fifth, Earth. The nature of water is to soak and descend; of fire, to blaze and ascend; of wood, to be crooked and to be straight; of metal, to obey and to change; while the virtue of the earth is seen in seed-sowing and ingathering. That which soaks and descends becomes salt; that which blazes and ascends becomes bitter; that which is crooked and straight becomes sour; that which obeys and changes becomes acrid; and from seed sowing and ingathering comes sweetness."[1]

A similar idea of five elements was also common among the Indian races, and is stated by Professor Rodwell to have been in existence before the fifteenth century B.C., but, though the number is the same, the elements themselves are not identical with those of the ancient Chinese classic; thus, in the Institutes of Menu, the "subtle ether" is spoken of as being the first created, from which, by transmutation, springs air, whence, by the operation of a change, rises light or fire; from this comes water, and from water is deposited earth. These five are curiously correlated with the five senses, and it is very evident that they are not looked upon as five independent material existences, but as derived from one another. This philosophy was accepted alike by Hindoos and Buddhists. It was largely extended over Asia, and found its way into Europe. It is best known to us in the writings of the Greeks. Among these people, however, the elements were reduced to four—fire, air, earth, and water—though Aristotle endeavoured to restore the "blue ether" to its position as the most subtle and divine of them all. It is true that the fifth element, or "quinta essentia," was frequently spoken of by the early chemists, though the idea attaching to it was somewhat changed, and the four elements continued to retain their place in popular apprehension, and still retain it even among many of the scholars who take degrees at our universities. The claim of wood to be considered an element seems never to have been recognised in the West, unless, indeed, we are to seek this origin for the choice of the word ὕλη to signify that original chaotic material out of which, according to Plato and his school, all things were created.[2] The idea also of a primal element, from which the others, and everything else, were originated, was common in Greece, the difficulty being to decide which of the four had the greatest claim to this honour. Thales, as is well known, in the sixth century B.C. affirmed that water was the first principle of things; but Anaxamenes afterwards looked upon air, Heraclytus upon fire, and Theracleides on earth, as the primal element. This notion of elements, however, was essentially distinct from our own. It was always associated with the idea of the genesis of matter rather than with its ultimate analysis, and the idea of *simple* as contrasted with *compound* bodies probably never entered into the thoughts of the contending philosophers.

The modern idea appears to have had a totally different origin, and we must again travel back to China. There, also in the sixth century B.C., the great philosopher Lao-tse was meditating on the mysteries of the world and the soul, and his disciples founded the religion of Taou. They were materialists; nevertheless they believed in a "finer essence," or spirit, that rises from matter, and may become a star; thus they held that the souls of the five elements, water, metal, fire, wood, and earth, arose and became the five planets. These speculations naturally led to a search after the sublimated essences of things, and the means by which this immortality might be secured. It seems that at the time of Tsin-she-hwang, the builder of the Great Wall, about two centuries before Christ, many romantic stories were current of immortal men inhabiting islands in the Pacific Ocean. It was supposed that in these magical islands was found the "herb of immortality" growing, and that it gave them

[1] Quoted from the translation by the Rev. Dr. Legge. In that most obscure classic, the "Yi-King," fire and water wind and thunder, the ocean and the mountains, appear to be recognised as the elements.

[2] Students of the Apocrypha will remember the expression in the Book of Wisdom, xi. 17, 'ἡ παντοδύναμός σου χεὶρ καὶ κτίσασα τὸν κόσμον ἐξ ἀμόρφου ὕλης' ('Thy Almighty hand, that made the world of matter without form') The same book contains two allusions to the ordinary elements, vii. 17, and xix. 18 to 20. The word στοιχεῖον is used in the New Testament only in a general sense (2 Pet. iii. 10), or in its more popular meaning of the first steps in knowledge.

exemption from the lot of common mortals. The emperor determined to go in search of these islands, but some untoward event always prevented him.[1]

Some two or three centuries after this a Taouist, named Weipahyang, wrote a remarkable book called "The Uniting Bond." It contains a great deal about the changes of the heavenly bodies, and the mutual relation of heaven and men; and then the author proceeds to explain some transformations of silver and water. About elixir he tells us, "What is white when first obtained becomes red after manipulation on being formed into the elixir" ("tan," meaning red or elixir). "That substance, an inch in diameter, consists of the black and the white, that is, water and metal combined. It is older than heaven and earth. It is most honourable and excellent. Around it, like a wall, are the sides of the cauldron. It is closed up and sealed on every side, and carefully watched. The thoughts must be undisturbed, and the temper calm, and the hour of its perfection anxiously waited for. The false chemist passes through various operations in vain. He who is enlightened expels his evil passions, is delighted morning and night, forgets fame and wealth, comprehends the true objects of life, and gains supernatural powers. He cannot then be scorched by fire, nor drowned in water, &c., &c. . . . The cauldron is round like the full moon, and the stove beneath is shaped like the half-moon. The lead ore is symbolised by the White Tiger; and it, like metal amongst the elements, belongs to the West. Mercury resembles the sun, and forms itself into sparkling globes; it is symbolised by the Blue Dragon belonging to the East, and it is assigned to the element wood. Gold is imperishable. Fire does not injure its lustre. Like the sun and moon, it is unaffected by time. Therefore the elixir is called 'the Golden Elixir.' Life can be lengthened by eating the herb called Hu ma; how much more by taking the elixir, which is the essence of gold, the most imperishable of all things! The influence of the elixir, when partaken of, will extend to the four limbs; the countenance will become joyful; white hair will be turned black; new teeth will grow in the place of old ones, and age at once become youth. . . . Lead ore and mercury are the bases of the process by which the elixir is prepared; they are the hinge upon which the principles of light and darkness revolve."

This description suggests the idea that the elixir of the Taouists was the red sulphide of mercury—vermilion—for the preparation of which the Chinese are still famous. That Weipahyang believed in his own philosophy is testified by a writer named Ko-hung, who, about a century afterwards, wrote the lives of celebrated Taouists. He tells how the philosopher, after preparing the elixir, took it, with his disciples, into a wood, and gave it first to his dog, then took it himself, and was followed by one of his pupils. They all three died, but, it appears, rose to life again, and to immortality. This brilliant example did not remain without imitators; indeed, two emperors of the Tang family are said to have died from partaking of the elixir. This circumstance diminished its popularity, and alchemy ceased to be practised in the Celestial Empire.

At the beginning of the seventh century the doctrine of Lao-tse was in great favour at the Chinese Court; learning was encouraged, and there was much enterprise. At the same time the disciples of Mohammed carried their arms and his doctrines over a large portion of Asia, and even to the Flowery Land. Throughout the eighth century there were frequent embassies between eastern and western Asia, wars with the Caliphs, and even a matrimonial alliance. We need not wonder, therefore, that the teachings of the Taouist alchemists penetrated westward to the Arabian philosophers. It was at this period that Yeber-Abou-Moussah-Djaferal-Sofé, commonly called Geber, a Sabæan of great knowledge, started what to the West was a new philosophy about the transmutation of metals, the Philosopher's Stone, and the Elixir of Life; and this teaching was couched in highly poetic language, mixed with astrology and accompanied by religious directions and rites. He held that all metals were composed of mercury, sulphur, and arsenic, in various proportions, and that the noblest metal could be procured only by a very lengthy purification. It was in the salts of gold and silver that he looked for the Universal Medicine. Geber himself was an experimental philosopher, and the belief in transmutation led to the acquirement of a considerable amount of chemical knowledge amongst the alchemists of Arabia and Europe. This

[1] Nearly all the statements relating to this Taouist alchemy are derived from the writings of the Rev. Joseph Edkins, of Pekin, and the matter is treated in greater detail in an article on the "Birth of Alchemy," in the "Argonaut," vol. iii. p. 1.

gradually brought about a conviction that the three reputed elementary bodies, mercury, sulphur, and salt or acid, were not really the originators of all things. There was a transition period, during which the notion was itself suffering a transmutation. The idea became gradually clearer that all material bodies were made up of certain constituents, which could not be decomposed any further, and which, therefore, should be considered as elementary. The introduction of quantitative methods compelled the overthrow of mediæval chemistry, and led to the placing of the conception of simple and compound bodies upon the foundation of scientific fact. Lavoisier, perhaps, deserves the greatest credit in this matter, while the labours of the other great chemists of the eighteenth and the beginning of the nineteenth centuries were in a great measure directed to the analysis of every conceivable material, whether solid, liquid, or gaseous. These have resulted in the table of so-called elements, now nearly seventy in number, to which fresh additions are constantly being made.

Of this ever-growing list of elements not one has been resolved into simpler bodies for three-quarters of a century; and we, who are removed by two or three generations from the great builders of our science, are tempted to look upon these bodies as though they were really simple forms of matter, not only unresolved, but unresolvable. The notation we employ favours this view and stamps it upon our minds.

Is it, however, a fact that these reputed elements are really simple bodies? or, indeed, are they widely different in the nature of their constitution from those bodies which we know to be chemical compounds? Thus, to take a particular instance, are fluorine, chlorine, bromine, and iodine essentially distinct in their nature from the compound halogens, cyanogen, sulphocyanogen, ferricyanogen, &c.? Are the metals lithium, sodium, and potassium essentially distinct from such alkaline bases as ammonium, ethylamine, di-ethylamine, &c.? No philosophical chemist would probably venture to answer this question categorically with either "yes" or "no." Let us endeavour to approach it from three different points of attack—(1) the evidence of the spectroscope, (2) certain peculiarities of the atomic weights, and (3) specific refraction.

1. *The Spectroscope.*—It was at first hoped that the spectroscope might throw much light upon the nature of elements, and might reveal a common constituent in two or more of them; thus, for instance, it was conceivable that the spectrum line of bromine or iodine vapour might consist of the rays given by chlorine *plus* some others. All expectations of this have hitherto been disappointed; yet, of the other, hand, it must not be supposed that such a result disproves the compound nature of elements, for as investigation proceeds it becomes more and more clear that the spectrum of a compound is not made up of the spectra of its component parts.

Again, the multiplicity of rays given out by some elements, when heated, in a gaseous condition, such as iron, has been supposed to indicate a more complex constitution than in the case of those metals, such as magnesium, which give a more simple spectrum. Yet it is perfectly conceivable that this may be due to a complexity of arrangement of atoms all of the same kind.

Again, we have changes of a spectrum at different temperatures; new rays appear, others disappear; or even there occurs the very remarkable change from a fluted spectrum to one of sharp lines at irregular intervals, or to certain recurring groups of lines. This, in all probability, does arise from some redistribution, but it may be a redistribution in a molecular grouping of atoms of the same kind, and not a dissociation or rearrangement of dissimilar atoms.

A stronger argument has been derived from the revelations of the spectroscope in regard to the luminous atmospheres of the sun. There we can watch the effect of heat enormously transcending that of our hottest furnaces, and of movements compared with which our hurricanes and whirlwinds are the gentlest of zephyrs. Mr Lockyer, in studying the prismatic spectra of the luminous prominences or spots of the sun, has frequently observed that on certain days certain lines, say of the iron spectrum are non-existent and on other days certain other lines disappear, and that in almost endless variety; and he has also remarked that occasionally certain lines of the iron spectrum will be crooked or displaced, thus showing the vapour to be in very rapid motion, while others are straight, and therefore comparatively at rest. Now, as a gas cannot be both at rest and in motion at the same time and the same place, it seems very clear that the two sets of lines must originate in two distinct layers of atmosphere, one above the other, and Mr. Lockyer's conclusion is

that the iron molecule was dissociated by heat, and that its different constituents, on account of their different volatility, or some other cause, had floated away from one another. This seems to me the easiest explanation of the phenomenon; and, as dissociation by heat is a very common occurrence, there is no *a priori* improbability about it. But we are not shut up to it, for the different layers of atmosphere are certainly at different temperatures, and most probably of different composition. If they are of different temperatures, the variations of the spectrum may only be an extreme case of what must be acknowledged by every one more or less—that bodies emit, or cease to emit, different rays as their temperature increases, and notably when they pass from the liquid to the gaseous condition. And again, if the composition of the two layers of atmosphere be different, we have lately learnt how profoundly the admixture of a foreign substance will sometimes modify a luminous spectrum.

2. *Peculiarities of Atomic Weights.*—At the meeting of this Association at Ipswich, in 1851, M. Dumas showed that in several cases analogous elements form groups of three, the middle one of which has an atomic weight intermediate between those of the first and third, and that many of its physical and chemical properties are intermediate also. During the discussion upon his paper, and subsequently,[1] attention was drawn to the fact that this is not confined to groups of three, but that there exist many series of analogous elements having atomic weights which differ by certain increments, and that these increments are in most cases multiples of 8. Thus we have lithium, 7; sodium, 23, *i.e.* 7 + 16; potassium, 39, *i.e.* 7 + (16 × 2); and the more recently discovered rubidium, 85, *i.e.* 7 + (16 × 5) nearly; and cæsium, 133, *i.e.* 7 + (16 × 8) nearly. This is closely analogous to what we find in organic chemistry, where there are series of analogous bodies playing the part of metals, such as hydrogen, methyl, ethyl, &c., differing by an increment which has the atomic weight 14, and which we know to be CH_2. Again, there are elements with atomic weights nearly the same or nearly multiples of one another, instances of which are to be found in the great platinum group and the great cerium group.[2] This suggests the analogy of isomeric and polymeric bodies. There is also this remarkable circumstance: the various members of such a group as either of those just mentioned are found together at certain spots on the surface of the globe, and scarcely anywhere else. The chemist may be reminded of how in the dry distillation of some organic body he has obtained a mixture of polymerised hydrocarbons, and may perhaps be excused if he speculates whether in the process of formation of the platinum or the cerium group, however and whenever it took place, the different elements had been made from one another and imperfectly polymerised.

But this is not the largest generalisation in regard to the peculiarities of these atomic weights. Newlands showed that, by arranging the numbers in their order, the octaves presented remarkable similarities, and, on the same principle, Mendeléeff constructed his well-known table. I may remind you that in this table the atomic weights are arranged in horizontal and vertical series, those in the vertical series differing from one another, as a rule, by the before-mentioned multiples of 8—namely 16, 16, 24, 24, 24, 24, 32, 32—the elements being generally analogous in their atomicity and in other chemical characters. Attached to the elements are figures, representing various physical properties, and these in the horizontal series appear as periodic functions of the atomic weights. The table is incomplete, especially in its lower portions, but, with all its imperfections and irregularities, there can be no doubt that it expresses a great truth of nature. Now, if we were to interpolate the compound bodies which act like elements—methyl, 15; ammonium, 18; cyanogen, 26—into Mendeléeff's table, they would be utterly out of place, and would upset the order both of chemical analogy and of the periodicity of the physical properties.

3. *Specific Refraction.*—The specific refraction has been determined for a large majority of the elements, and is a very fundamental property, which belongs to them apparently in all their combinations, so long at least as the atomicity[3] is unchanged. If the figures representing this property be inserted into Mendeléeff's table, we find that in the vertical columns the figures almost invariably decrease as the atomic weights increase. If, however, we look along the horizontal columns, or better still if we plot the figures in the table by which Lothair Meyer has shown graphically that the molecular volume is a periodic function of the atomic weights, we shall see that they arrange themselves in a series of curves similar to but not at all coincident with his. The observations are not so complete or accurate as those of the molecular volumes, but they seem sufficient to establish the fact, while the points of the curves would appear to be, not the alkaline metals, as in Meyer's diagram, but hydrogen, phosphorus and sulphur, titanium and vanadium, selenium, antimony. Now, if we were to insert the specific refractions of cyanogen, ammonium, and methyl into this table, we should again show that it was an intrusion of strangers not in harmony with the family of elements.

But there is another argument to be derived from the action of light. The refraction equivalent of a compound body is the sum of the refraction equivalents of its compounds; and, if there is anything known for certain in the whole subject, it is that the refraction equivalent of an organic compound advances by the same quantity (7·6) for every increment of CH_2. If, therefore, the increment between the different members of a group of analogous elements, such as the alkaline metals, be of the same character, we may expect to find that there is a regular increase of the refraction equivalent for each addition of 16. But this is utterly at variance with fact: thus, in the instance above quoted, the refraction equivalent of lithium being 3·8, that of sodium is 4·8, of potassium 8·1, of rubidium 14·0, and of cæsium about 13·7. Neither does the law obtain in those series in which the increment is not a multiple of 8, as in the case of the halogens, where the increment of atomic weight is 45, and the refraction equivalents are chlorine 9·9, bromine 15·3, and iodine 24·5.

The refraction equivalents of isomeric bodies are generally identical, and the refraction equivalents of polymeric bodies are in proportion to their atomic weights. Among the groups of analogous elements of the same, or nearly the same, atomic weight we do find certain analogies: thus cobalt and nickel are respectively 10·8 and 10·4, while iron and manganese are respectively 12·0 and 12·2. But, as far as observation has gone at present, we have reason to conclude that, if metals stand to one another in the ratio of 2 : 1 in atomic weight, their refraction equivalents are much nearer together than that; while, on the other hand, the equivalent of sulphur, instead of being the double of that of oxygen, is at least five times as great.

The general tendency of these arguments is evidently to show that the elementary radicals are essentially different from the compound radicals, though their chemical functions are similar.

There remains still the hypothesis that there is a "primordial element," from which the others are derived by transmutation. With the sages of Asia it was the "blue ether," with Thales water, with Dr. Prout hydrogen. The earlier views have passed away, and the claims of hydrogen are being fought out on the battle-field of atomic weights and their rigorous determination.

There does not appear to be any argument which is fatal to the idea that two or more of our supposed elements may differ from one another rather in form than in substance, or even that the whole seventy are only modifications of a prime element; but chemical analogies seem wanting. The closest analogy would be if we could prepare two allotropic conditions of some body, such as phosphorus or cyanogen, which should carry their allotropism into all their respective compounds, no compound of the one form being capable of change into a compound of the other. Our present knowledge of allotropism, and of variations in atomicity, affords little, if any, promise of this.

The remarkable relations between the atomic weights of the elements, and many peculiarities of their grouping, force upon us the conviction that they are not separate bodies created without reference to one another, but that they have been fashioned or built up from one another, according to some general plan. This plan we may hope gradually to understand better, but if we are ever to transform one of these supposed elements into another, or to split up one of them into two or three dissimilar forms of matter, it will probably be by the application of some method of analysis hitherto unknown.

Nothing can be of greater promise than the discovery of new methods of research; hence I need make no apology to others who have lately done excellent work in chemistry if I single out the Bakerian Lecture of this year, by Mr. Crookes, on "Radiant Matter Spectroscopy." It relates to the prismatic analysis, not of the light transmitted or absorbed in the ordinary way by a solid or liquid, nor of that given out by incandescent gas, but the

[1] "Phil. Mag.," May, 1853.

[2] Another curious instance is the occurrence of nickel and cobalt in all meteoric irons, with occasionally chromium or manganese, the atomic weights and other properties of which are very similar.

[3] This exception includes not merely such changes as that from a ferrous to a ferric salt, but the different ways in which the carbon is combined in such bodies as ethene, benzene, and pyrene.

analysis of the fluorescence that manifests itself in certain bodies when they are exposed to an electric discharge in a highly exhausted vacuum. He describes, in an interesting and even amusing manner, his three years' quest after the origin of a certain citron band, which he observed in the spectrum of the fluorescence of many substances, till he was led into that wonderful labyrinth of uncertain elements which are found together in samarskite, and eventually he proved the appearance to be due to yttrium. As the test is an extremely delicate one, he has obtained evidence of the very general dissemination of that element, in very minute quantities—and not always very minute—for the polypes that built up a certain pink coral were evidently able to separate the earth from the sea water, as their calcareous secretion contained about $\frac{1}{2}$ per cent. of yttrium. We have reason to hope that this is only the first instalment of discoveries to be made by this new method of research.

I cannot conclude without a reference to the brightening prospects of technical chemistry in this country. I do not allude to the progress of any particular industry, but to the increased facilities for the education of those engaged in the chemical manufactures. First as to the workpeople. Hitherto the young artisan has had little opportunity of learning at school what would be of the greatest service to him in his after career. The traditions of the Middle Ages were all in favour of literary culture for the upper classes, and the education suited for these has been retained in our schools for the sons of the people. It is true that some knowledge of common things has been given in the best schools, and the Education Department has lately encouraged the teaching of certain sciences in the upper standards. In the Mundella Code, however, which came into operation last year, "elementary science" may receive a grant in all the classes of a boys' or girls' school, and in the suggested scheme there is mentioned simple lessons on "the chemical and physical principles involved in one of the chief industries of England, among which Agriculture may be reckoned," while "Chemistry" is inserted among "the specific subjects of instruction" that may be given to the older children. It is impossible, as yet, to form an estimate of the extent to which managers and teachers have availed themselves of this permission, for the examinations of Her Majesty's inspectors under the new code have only just commenced; but one of the best of the Board Schools in London has just passed satisfactorily in chemistry, both with boys and girls. I trust that in those parts of the country where chemical industries prevail, chemistry may be largely taken up in our elementary schools.

The great deficiency in our present educational arrangements is the want of the means of teaching a lad who has just left the common school the principles of that industry by which he is to earn his livelihood. The more purely scientific chemistry, however, may be learnt by him now in those evening classes which may be formed under the Education Department, as well as in those that have long been established under the Science and Art Department. The large amount of attention that is now being given to the subject of technical education is creating in our manufacturing centres many technical classes and colleges for students of older growth.

As to inventors and the owners of our chemical factories, in addition to the Chemical Society and the Chemical Institute, there has recently been founded the Society of Chemical Industry. It came into existence with much promise of success; at the close of its second year it numbered 1400 members; it has now powerful sections in London, Manchester, Liverpool, Newcastle, and Birmingham; and it diffuses information on technical subjects in a well-conducted monthly journal.

May the abstract science and its useful applications ever prove helpful to one another, and become more and more one chemistry for the benefit of mankind.

SECTION C

GEOLOGY

Opening Address by Professor W. C. Williamson, LL.D., F.R.S., President of the Section.

Much of the second decade of my life was spent in the practical pursuit of geology in the field, and throughout most of that period I enjoyed almost daily intercourse with William Smith, the father of English Geology; but in later years circumstances restricted my studies to the Palæontological side of the science. Hence I was anxious that the council of the British Association should place in this chair some one more familiar than myself with the later developments of geographical geology. But my friend, Professor Bonney, failing to recognise the force of my objections, intimated to me that I might render some service to the Association by placing before you a sketch of the present state of our knowledge of the vegetation of the Carboniferous Age.

This being a subject respecting which I have formed some definite opinions, I am going to act upon the suggestion. To some this may savour of "shop-talk." But such is often the only talk which a man can indulge in intelligently, and to close his mouth on his special themes may compel him either to talk nonsense or to be silent.

Whilst undertaking this task I am alive to the difficulties which surround it, especially those arising from the wide differences of opinion amongst palæobotanists on some fundamental points. On some of the most important of these there is a substantial agreement between the English and German palæontologists. The dissentients are chiefly, though not entirely, to be found amongst those of France, who have, in my humble opinion, been unduly influenced by what is in itself a noble motive—viz. a strong reverence for the views of their illustrious teacher, the late Adolphe Brongniart. Such a tendency speaks well for their hearts, though it may, in these days of rapid scientific progress seriously mislead their heads. I shall, however, endeavour to put before you faithfully the views entertained by my distinguished French friends M. Renault, M. Grand-Eury, and the Marquis of Saporta, giving, at the same time, what I deem to be good reasons for not agreeing with them. I believe that many of our disagreements arise from geological differences between the French Carboniferous strata and those in our own islands. There are some important types of Carboniferous plants that appear to be much better represented amongst us than in France. Hence we have, I believe, more abundant material than the French palæontologists possess for arriving at sound conclusions respecting these plants. We have rich sources supplying specimens in which the internal organisation is preserved, in Eastern Lancashire and Western Yorkshire, Arran, Burntisland, and other scattered localities. France has equally rich localities at Autun and at St. Étienne. But some important difference exists between these localities. The French objects are preserved in an impracticable siliceous matrix, extremely troublesome to work, except in specimens of small size. Ours, on the other hand, are chiefly embedded in a calcareous material which, whilst it preserves the objects in an exquisite manner, does not prevent our dissecting examples of considerable magnitude. But, besides this, we are much richer in huge Lepidodendroid and Sigillarian trees, with their Stigmarian roots, than the French are; hence we have a vast mass of material illustrating the history of these types of vegetation, in which they seem to be seriously deficient. This fact alone appears to me sufficient to account for many of the wide differences of opinion that exist between us respecting these trees. My second difficulty springs out of the imperfect state of our knowledge of the subject. One prominent cause of this imperfection lies in the state in which our specimens are found. They are not only too frequently fragmentary, but most of those fragments only present the external forms of the objects. Now, mere external forms of fossil plants are somewhat like similarities of sound in the comparative study of languages. They are too often unsafe guides. On the other hand, microscopic internal organisations in the former subjects are like grammatical identities in the latter one. They indicate deep affinities that promise to guide the student safely to philosophical conclusions. But the common state in which our fossil plants are preserved presents a source of error that is positive as well as negative. Most of those from our coal-measures consist of inorganic shale, sandstone, or ironstone, invested by a very thin layer of structureless coal. The surface of the inorganic substance is moulded into some special form dependent upon structural peculiarities of the living plants, which structures were sometimes external, sometimes internal, and sometimes intermediate ones. Upon this inorganic cast we find the thin film of structureless coal, which, though of organic origin, is practically as inorganic as the clay or sandstone which it invests; but its surface displays specific sculpturings which are apt to be regarded as always representing the outermost surface of the plant when living, whereas this is not always the case. That the coaly film is a relic of the carbonaceous substance of the living plant is unquestionable; but the thinnest of these films are often the sole remaining representatives of structures that must originally been have many inches, and in some instances even many feet, in thickness. In such cases most of

the organic material has been dissipitated, and what little remains has often been consolidated in such a way that it is merely moulded upon the sculptured inorganic substance which it covers, and hence affords no information respecting the exterior of the fossil when a living organism. It is, in my opinion, from specimens like these that the smooth bark of the Calamite has been credited with a fluted surface, and the Trigonocarpons with a merely triangular exterior and a misleading name, as it long caused the inorganic casts known as Sternbergiæ to be deemed a strange form of plant that had no representative amongst living types. In other cases the outermost surface of the bark is brought into close contact with the surface of the vascular cylinder. I have a Stigmaria in which the bases of the rootlets appear to be planted directly upon that cylinder, the whole of the thick intermediate bark having disappeared. In other examples that vascular zone has also gone. Thus the innermost and outermost surfaces of a cylinder, originally many inches apart, are, through the disappearance of the intermediate structures, brought into close approximation. In such cases, leaves and other external appendages appear to spring directly from what is merely an inorganic cast of the interior of the pith. I believe that many of our Calamites are in this condition. Such examples have suggested the erroneous idea that the characteristic longitudinal flutings belong to the exterior of the bark.

Fungi.—Entering upon a more detailed review of our knowledge of the Carboniferous plants, and commencing at the bottom of the scale, we come to the lowly group of the Fungi, which are unquestionably represented by the *Peronosporites antiquarius*[1] of Worthington Smith. There seems little reason for doubting that this is one of the Phycomycetous Fungi, possibly somewhat allied to the *Saprolegniæ*; but since we have as yet no evidence respecting its fructification, these closer relationships must, for the present, remain undetermined. So far as I know, this is the only Fungus satisfactorily proved to exist in the Carboniferous rocks, unless the *Excipulites Neesii* of Goeppert and one or two allied forms belong to the Fungoid group. The *Polyporites Bowmanni* is unquestionably a scale of a Holoptychian fish.

Algæ.—Numerous objects supposed to belong to this family have been discovered in much older rocks than Carboniferous ones. The subject is a thorny one. That marine plants of some kind must have existed simultaneously with the molluscous and other plant-eating animals of Palæozoic times is obviously indisputable. But what those plants were is another question. The widest differences of opinion exist in reference to many of them. A considerable number of those recognised by Schimper, Saporta, and other palæobotanists, are declared by Nathorst to be merely inorganic tracks of marine animals—and in the case of many of these I have little doubt that the Swedish geologist is right. Others have been shown to be imperfectly preserved fragments of plants of much higher organisation than Algæ, branches of Conifers even being included amongst them. I have as yet seen none of Carboniferous age that could be indisputably identified with the family of Algæ, though there are many that look like, and may probably be, such. The microscope alone can settle this question, though even this instrument fails to secure unity of opinion in the case of Dawson's *Prototaxites*, and no other of the supposed seaweeds hitherto discovered have been sufficiently well preserved to bear the microscopic test; hence I think that their existence in Carboniferous rocks can only be regarded as an unproved probability. Mere superficial resemblances do not satisfy the severe demands of modern science, and probabilities are an insufficient foundation upon which to build evolutionary theories.

Seeing what extremely delicate cell-structures *are* preserved in the Carboniferous beds, it cannot appear other than strange that the few imperfect Fungoid relics just referred to constitute the only terrestrial cellular Cryptogams that have been discovered in the Carboniferous strata. The Darwinian doctrine would suggest that these lower forms of plant life ought to have abounded in that primæval age; and that they were capable of being preserved is proved by the numerous specimens met with in Tertiary deposits. Why we do not find such in the Palæozoic beds is still an unsolved problem.

Vascular Cryptogams.—The Vascular Cryptogams, next to be considered, burst upon us almost suddenly and in rich profusion during the Devonian age; they are equally silent in the Devonian and Carboniferous strata as to their ancestral descent.

Ferns.—The older taxonomic literature of Palæozoic Fern life is, with few exceptions, of little scientific value. Hooker and others have uttered in vain wise protests against the system that has been pursued. Small fragments have had generic and specific names assigned to them, with supreme indifference to the study of morphological variability amongst living types. The undifferentiated tip of a terminal pinnule has had its special name, whilst the more developed structures forming the lower part of a frond have supplied two or three more species. Then the distinct forms of the fertile fronds may have furnished additional ones, whilst a further cause of confusion is seen in the wide difference existing between a young half-developed seedling and the same plant at an advanced stage of its growth. Any one who has watched the development of a young *Polypodium aureum* can appreciate this difference. Yet, in the early stages of palæontological research, observers could scarcely have acted otherwise than as they did in assigning names to these fragments—if only for temporary working purposes. Our error lies in misunderstanding the true value of such names. At present the study of fossil ferns is affording some promise of a newer and healthier condition. We are slowly learning a little about the fructification of some species, and the internal organisation of others. Facts of these kinds, cautiously interpreted, are surer guides than mere external contours; unfortunately, such facts are, as yet, but few in number, and when we have them we are too often unable to identify our detached sporangia, stems, and petioles with the fronds of the plants to which they primarily belonged.

That all the Carboniferous plants included in the genera *Pecopteris*, *Neuropteris*, and *Sphenopteris* are ferns appears to be most probable; but what the true affinities of the objects included in these ill-defined genera may be is very doubtful. Here and there we obtain glimpses of a more definite kind. That the Devonian *Palæopteris Hibernica* is a Hymenophyllous form appears to be almost certain; and on corresponding grounds we may conclude that the Carboniferous forms *Sphenopteris trichomanoides*, *S. Humboltii*,[1] and *Hymenophyllum Weissii*,[2] belong to the same group. The fructification of the two latter leaves little room for doubting their position, whilst the foliage of some other species of Sphenopteris is suggestive of similar conclusions, but until their fructification is discovered this cannot be determined. An elegant form of Sphenopteris (*S. tenella*, Brong., *S. lanceolata* of Gutbier), recently described by Mr. Kidson of Stirling, abundantly justifies caution in dealing with these Sphenopterides. This plant possesses a true Sphenopteroid foliage, but its fructification is that of a Marattiaceous *Danaid.* The sporangia are elongated vertically, and have the round terminal aperture of both the recent and fossil *Danaiæ*—a group of plants far removed from the Hymenophyllaceous type of Sphenopterid already referred to.

Whether or not this *Sphenopteris* was really Marattiaceous in other features than its fructification is uncertain; but I think that we have indisputably got stems and petioles of Marattiaceæ from the Carboniferous strata. My friend M. Renault and I, without being aware of the fact, simultaneously studied the *Medullosa elegans* of Colta. This plant was long regarded as the stem of a true Monocotyledon, a decision the accuracy of which was doubted first by Brongniart and afterwards by Binney. M. Renault's memoir and my part vii. appeared almost simultaneously. We then found that we had alike determined the supposed Monocotyledon to be not only a fern, but to belong to the peculiarly aberrant group of the *Marattiaceæ.* As yet we know nothing of its foliage and fructification.

M Grand-Eury has figured[3] a remarkable series of ferns from the coal-measures of the basin of the Loire, the sporangia of which exhibit marked resemblances to those of the Marattiaceæ. This is especially the case with his specimens of *Asterotheca* and *Scolecopteris*,[4] as also with his *Pecopteris Marattiæthecu*, *P. Angiotheca*, and *P. Danaeætheca*, but there is some doubt as to the dehiscence of the sporangia of these plants; hence their Marattiaceous character is not absolutely established.

That the coal-measures contain the remains of arborescent ferns has long been known, especially from their abundance at Autun. In Lancashire I have only met with the stems or petioles of one species preserving their internal organisation.[5] The Rev. H. H. Higgins obtained stems that appear to have been tree-ferns from Ravenhead, in Lancashire, and it is probable that

[1] "Memoir" xi. p. 299.

[1] "Schimper," vol. i. p. 408. [2] *Ibid.* p. 415.

[3] "Flore Carbonifère du Départment de la Loire et du Centre de la France."

[4] *Loc. cit.* Tab. viii. Figs 1-5.

[5] *Psaronius Renaultii*, Memoir vii. p. 10, and Memoir xii. Pl. iv. Figs. 16. These and other similar references are to my series of Memoirs "On the Organisation of the Fossil Plants of the Coal-measures," published in the "Philosophical Transactions"

most of the plants included in the genera *Psaronius*, *Caulopteris*, and *Protopteris*, are also tree-ferns.

There yet remains another remarkable group of ferns, the sporangia of which are known to us through the researches of M. Renault. In these the fertile pinnules are more or less completely transmuted into small clusters of oblong sporangia. In one case M. Renault believes that he has identified these organs with a stem or petiole of a type not uncommon at Oldham and Halifax, belonging to Corda's genus *Zygopteris*. Renault has combined this with some others to constitute his group of *Botryoptéridées*, an altogether extinct and generalised type. This review shows that whilst forms identifiable with the *Hymenophyllaceæ* and *Marattiaceæ* existed in the Carboniferous epoch, and we find here and there traces of affinities with some other more recent types, most of the Carboniferous ferns are generalised primæval forms which only become differentiated into later ones in the slow progress of time.

Equisetaceæ and *Asterophyllitæ*, Brong. *Calamariæ*, Endlicher. *Equisetineæ*, Schimper.

Confusion culminates in the history of this variously-named group. Hence the subject is a most difficult one to treat in a concise way. The confusion began when Brongniart separated the plants contained in the group into two divisions—one of which (*Équisétacés*) he identified with the living Equisetums, and the other (*Astérophyllitées*) he regarded as being Gymnospermous Dicotyledons. To Schimper belongs the merit, as I believe it to be, of steadily resisting this division; nevertheless, palæobotanists are still separated into two schools on the subject; Dawson, Renault, Grand-Eury, and Saporta adhere to the Brongniartian idea, whilst the British and German palæontologists have always adopted the opposite view, rejecting the idea that any of these plants were other than Cryptogams.

A fundamental feature of the entire group is in the fact that their foliar appendages, however morphologically and physiologically modified, are arranged in nodal verticils. This appears to be the only characteristic which the plants possess in common.

Calamites and *Calamodendron*. In his "Prodrome" (1828), and in his later "Végétaux Fossiles," Brongniart adopted the former of these generic names as previously employed by Suckow, Schlotheim, Sternberg, and Artis. It was only in his "Tableau des Genres de Végétaux Fossiles" ("Dictionnaire universel d'Histoire Naturelle," 18 9) that he divided the genus, introducing the second name to represent what he believed to be the Gymnospermous division of the group. A long series of investigations, extending over many years, has convinced me that no such Gymnospermous type exists.[1] The same conclusion has more recently been arrived at by Vom c. M. D. Stur,[2] after studying many continental examples in which structure is preserved. What I regard as an error appears to have had an intelligible origin—the fertile source of similar errors in other groups.

Nearly all the Calamitean fossils found in shales and sandstones consist of an inorganic, superficially fluted substance, coated over with a thin film of structureless coal (see "Histoire des Végétaux Fossiles," Vol. i., Pl. 22), *the latter being exactly moulded upon and following the outlines of the inorganic fluted cast that underlies it.* Brongniart and those who adopt his views believe that the external surface of this coal-film exactly represents the corresponding external surface of the original plant. Hence the conclusion was arrived at that the plant had a very large central fistular cavity surrounded by a very thin layer of cellular and vascular tissues as in some living Equisetums. On the other hand, Brongniart also obtained some specimens of what he primarily believed to be Calamites, in which the central pith was surrounded by a thick layer of woody tissue arranged in radiating laminated wedges, separated by medullary rays. The exogenous structure of this woody zone was too obvious to escape his practised eye. But, not supposing it possible that any Cryptogam could possess a cambium-layer and an exogenous mode of development, Brongniart came to the conclusion that the thin-walled specimens found in the shales and sandstones were true *Equisetaceæ*, those with the thick woody cylinders being exogens of another type. His conclusion that they were Gymnosperms was a purely hypothetical one, justified by no one feature of their organisation.

My researches, based upon a vast number of specimens of all sizes, from minute twigs little more than the thirtieth of an inch in diameter, to thick stems at least thirteen inches across, led me to the conclusion that we have but one type of Calamite; and that the differences which misled Brongniart are merely due to variations in the mode of their preservation.[1] It became clear to me that the outer surface of the coaly film in the specimens preserved in the shales and sandstones did *not* represent the outer surface of the living plant, but was only a fractional remnant of the carbon of that plant which had undergone a complete metamorphosis; the greater part of what originally existed had disappeared, probably in a gaseous state, and the little that remained, displaying no organic structure, *had been moulded upon the underlying inorganic cast of the medullary cavity.* This cast is always fluted longitudinally and constructed transversely at intervals of varying lengths. Both these features were due to impressions made by the organism upon the inorganic sand or mud filling the medullary cavity whilst it was in a plastic state, and which subsequently became more or less hardened; the longitudinal grooves being caused by the pressure of the inner angles of the numerous longitudinally vascular wedges, and the transverse ones partly by the remains of a cellular nodal diaphragm, which crossed the fistular medullary cavity, and partly by a centripetal encroachment of the vascular zone at each of the same points.[2]

My cabinets contain an enormous number of sections of these plants in which the minutest details of their organisation are exquisitely preserved. These specimens, as already observed, show their structure in every stage of their growth, from the smallest twigs to stems more than a foot in diameter. Yet these various examples are all, without a solitary exception, constructed upon one common plan. That plan is an extremely complicated one; far too complex to make it in the slightest degree probable that it could coexist in two such very different orders of plants as the *Equisetaceæ* and the *Gymnospermæ*; yet, though very complex, it is, even in many of its minuter details, unmistakably the plan upon which the living Equisetums are constructed. The resemblances are too clear as well as too remarkable, in my mind, to leave room for any doubt on this point. The great differences are only such as necessarily resulted from the gradual attainment of the arborescent form so unlike the lowly herbaceous one of their living representatives. On the other hand, no living Gymnosperm possesses an organisation that *in any solitary feature* resembles that of the so called Calamodendra. The two have absolutely nothing in common; hence the conclusion that these Calamodendra were Gymnospermous plants is as arbitrary an assumption as could possibly be forced upon science; an assumption that no arguments derived from the merely external aspects of structureless specimens could ever induce me to accept.

These Calamites exhibit a remarkable morphological characteristic which presents itself to us here for the first time, but which we shall find recurs in other Palæozoic forms. Some of our French botanical friends group the various structures contained in plants into several "*Appareils*,"[3] distinguished by the functions which those structures have to perform. Amongst others we find the "*Appareil de soutiens*," embracing those hard woody tissues which may be regarded as the supporting skeleton of the plant, and the "*Appareil conducteur*," which M. van Tieghem describes as composed of two tissues: "Le tissu criblé qui transporte essentiellement les matières insolubles, et le tissu vasculaire qui conduit l'eau et les substances dissoutes." Without discussing the scientific limits of this definition, it suffices for my present purpose. In nearly all flowering plants these two "*Appareils*" are more or less blended. The supporting wood cells are intermingled in varying degrees with the sap-conducting vessels. It is so even in the lower Gymnosperms, and in the higher ones these wood cells almost entirely replace the vessels. It is altogether otherwise with the fossil Cryptogams. The vascular cylinder in the interior of the Calamites, for example, consists wholly of *barred* vessels, a slight modification of the scalariform type so common in all Cryptogams. No trace of the "*Appareil de soutiens*" is to be found amongst them. The vessels are, in the most definite sense, the "*Appareils conducteurs*" of these plants; no such absolutely undifferentiated unity of tissue is to be found in any living plants other than Cryptogams.

But these Calamites, when living, towered high into the air. My friend and colleague, Professor Boyd Dawkins, recently assisted me in measuring one found in the roof of the Moorside colliery near Ashton-under-Lyne by Mr. George Wild, the very intelligent manager of that and some neighbouring collieries.

1 "Memoirs" i. ix and xii.
2 "Zur Morphologie der Calamarien."

1 "Memoirs" i. and ix.
2 See "Memoir" i. Pl. xxiv. Fig. 10, and Pl. xxvi. Fig. 24.
3 Van Tieghem, "Traité de Botanique," p. 679.

The flattened specimen ran obliquely along the roof, each of its two extremities passing out of sight, burying themselves in the opposite sides of the mine. Yet the portion which we measured was 30 feet long, its diameter being 6 inches at one end, and $4\frac{1}{2}$ inches at the other. The mean length of its internodes at its broader end was 3 inches, and at its narrower one $1\frac{1}{2}$ inches. What the real thickness of this specimen was when all its tissues were present we have no means of judging, but the true diameter of the cylinder represented by the fossil when uncompressed has been only 4 inches at one end of the 30 feet, and $2\frac{1}{2}$ inches at the other. Whatever its entire diameter when living, the vascular cylinder of this stem must have been at once tall and slender, and consequently must have required some "*Appareil de soutien*," such as its exogenous vascular zone did not supply. This was provided in a very early stage of growth by the introduction of a second cambium-layer into the bark; which, though reminding us of the cork-cambium in ordinary exogenous stems, produced not cork but prosenchymatous cells.[1] In its youngest state the bark of the Calamites was a very loose cellular parenchyma, but in the older stems much of this parenchyma became inclosed in the prosenchymatous tissue referred to, and which appears to have constituted the greater portion of the matured bark. The sustaining skeleton of the plant, therefore, was a hollow cylinder developed centrifugally on the inner side of an inclosing cambium-zone. That this cambium-zone must have had some protective periderm external to it is obvious; but I have not yet discovered what it was like. We shall find a similar cortical provision for supporting lofty cryptogamous stems in the *Lepidodendra* and *Sigillariæ*.

The Carboniferous rocks have furnished a large number of plants having their foliage arranged in verticils, and which have had a variety of generic names assigned to them; such are *Asterophyllites*, *Sphenophyllum*, *Annularia*, *Bechera*, *Hippurites*, and *Schizoneura*. Of these genera, *Sphenophyllum* is distinguished by the small number of its wedge-shaped leaves, and the structure of its stems has been described by M. Renault. *Annularia* is a peculiar form in which the leaves forming each verticil, instead of being all planted at the same angle upon the central stem, are flattened obliquely nearly in the plane of the stem itself. *Asterophyllites* differs from *Sphenophyllum*, chiefly in the larger number and in the linear form of its leaves. Some stems of this type have virtually the same structure[2] as those of *Sphenophyllum*, a structure which differs widely from that of the Calamites, and of which, consequently, these plants cannot constitute the leaf-bearing branches. But there is little doubt that true Calamitean branches have been included in the genus *Asterophyllites*; I have specimens, for which I am indebted to Dr. Dawson, which I should unhesitatingly have designated *Asterophyllites* but for my friend's positive statement that he detached them from stems of a Calamite. Of the internal organisation of the stems of the other genera named we know nothing.

It is a remarkable fact that, notwithstanding the number of young Calamitean shoots that we have obtained from Oldham and Halifax in which the structure is preserved, we have not met with one with the leaves attached. This is apparently due to the fact that most of the specimens are decorticated ones. We have a sufficient number of corticated specimens to show us what the bark was, but such specimens are not common. They clearly prove, however, that their bark had a smooth, and not a furrowed, external surface.

There yet remains for consideration the numerous reproductive strobili, generally regarded as belonging to plants of this class, *Equisetinæ*. We find some of these strobili associated with stems and foliage of known types, as in *Sphenophyllum*,[3] but we know nothing of the internal organisation of these Sphenophylloid strobili. We have strobili connected with stems and foliage of *Annularia*,[4] but we are equally ignorant of the organisation of these; so far as that organisation can be ascertained from Sterzel's specimen, it seems to have alternating sterile and fertile bracts with the sporangia of the latter arranged in fours, as in *Calamostachys*.[5] On the other hand, we are now very familiar with the structure of the *Calamostachys Binneana*, the prevalent strobilus in the calcareous nodules found in the lower coal-measures of Lancashire and Yorkshire. It has evidently been a sessile spike, the axial structures of which were trimerous[1] (rarely tetramerous), having a cellular medulla in its centre. Its appendages were exact multiples of those numbers. Of the plant to which it belonged, we know nothing. On the other hand, we have examples, supposed to be of the same genus, as *C. paniculata*,[2] and *C. polystachya*,[3] united to stems with Asterophyllitean leaves, but whether or not these fruits have the organisation of *C. Binneana*, we are unable to say.

We are also acquainted with the structure of the two fruits belonging to the genera *Bruckmannia*[4] and *Volkmannia*.[5] This latter term has long been very vaguely applied.

There still remain the genera *Stachannularia*, *Palæostachya*, *Macrostachya*, *Cingularia*, *Huttonia*, and *Calamitina*, all of which have the phyllomes of their strobili, fertile and sterile, arranged in verticils, and some of them display Asterophyllitean foliage. But these plants are only known from structureless impressions. That all these curious spore-bearing organisms have close affinities with the large group of the Equisetums cannot be regarded as certain, but several of them undoubtedly have peculiarities of structure suggestive of relations with the Calamites. This is especially observable in the longitudinal canals found in the central axis of each type, apparently identical with what I have designated the internodal canals of the Calamites.[6] The position and structure of their vascular bundles suggest the same relationship, whilst in many the position of the sporangia and sporangiophores is eminently Equisetiform. Renault's *Bruckmannia Grand-Euryi*, and *B. Decaisnei*, and a strobilus which I described in 1870,[7] exhibit these Calamitean affinities very distinctly.

One strobilus which I described in 1880[8] must not be overlooked. As is well known, all the living forms of Equisetaceous plants are isosporous. We only discover heterosporous vascular cryptogams amongst the *Lycopodiaceæ*, and the *Rhizocarpæ*. My strobilus is identical in every detailed feature of its organisation with the common *Calamostachys Binneana*, excepting that it is heterosporous, having microspores in its upper and macrospores in its lower part; a state of things suggestive of some link between the *Equisetinæ* and the heterosporous *Lycopodiaceæ*.

Lycopodiaceæ.—This branch of my subject suggests memories of a long conflict which, though it is virtually over, still leaves, here and there, the ground-swell of a stormy past. At the meeting of the British Association at Liverpool in 1870, I first announced that a thick, secondary, exogenous growth of vascular tissue existed in the stems of many Carboniferous cryptogamic plants, especially in the Calamitean and Lepidodendroid forms. But, at that time, the ideas of M. Brongniart were so entirely in the ascendant, that my notions were rejected by every botanist present. Though the illustrious French palæontologist knew that such growths existed in *Sigillariæ* and in what he designated *Calamodendra*, he concluded that, *de facto*, such plants could not be Cryptogams. Time, however, works wonders. Evidence has gradually accumulated proving that—with the conspicuous exception of the ferns—nearly every Carboniferous Cryptogam was capable of developing such zones of secondary growth. The exceptional position of the ferns still appears to be as true as it was when I first proclaimed their exceptional character at Liverpool. At that time I was under the impression that the secondary wood was only developed in such plants as attained to arboreal dimensions, but I soon afterwards discovered that it occurred equally in many small plants like *Sphenophyllum*, *Asterophyllites* and other diminutive types.

After thirteen years of persevering demonstration, these views, at first so strongly opposed, have found almost universal acceptance. Nevertheless, there still remain some few who believe them to be erroneous ones. In the later stages of this discussion the botanical relations subsisting between *Lepidodendron*, *Sigillaria*, and *Stigmaria* have been the chief themes of debate. In this country we regard the conclusion that *Stigmaria* is not only a root, but the root alike of *Lepidodendron* and *Sigillaria*, as settled beyond all dispute. Nevertheless M. Renault and M. Grand-Eury believe that it is frequently a leaf-bearing rhizome,

[1] "Memoir" ix. Pl. xx. Figs. 14, 15, 18, 19, and 20.
[2] "Memoir," Part v. Plates i.—v., and Part ix. Pl. xxi. Fig. 32.
[3] Lesquereux, "Coal Flora of Pennsylvania," Pl. ii. Fig. 687.
[4] "Ueber die Fruchtähren von Annularia Sphenophylloides." Von T. Sterzel, "Zeitschr. d. Deutschen Geolog. Gesellschaft," Jahrg. 1882.
[5] M. Renault has described a strobilus under the name of *Annularia longifolia*, but which appears to me very distinct from that genus.

[1] It is an interesting fact that transverse sections of the young strobili of *Lycopodium Alpinum* exhibit a similar trimerous arrangement, though differing widely in the positions of its sporangia.
[2] Weiss, "Abhandlungen zur Geologischen Specialkarte von Preuszen und Thüringischen Staaten," Taf. xiii. Fig. 1.
[3] *Idem*. Taf. xvi. Figs. 1, 2.
[4] Renault, "Annales de Sciences naturelles," Bot., Tome iii. Pl. iii.
[5] *Idem*. Pl. ii.
[6] "Memoir" i. Pl. xxiv. Fig. 14 *e*, and Pl. xxvi. Fig. 24 *e*.
[7] "Memoirs of the Literary and Philosophical Society of Manchester," 3rd series, vol. iv. p. 248.
[8] "Memoir" xi. Pl. liv. Figs. 23, 24.

from which aërial stems are sent upwards. I am satisfied that there is not a shadow of foundation for such a belief. The same authors, along with their distinguished countryman, the Marquis de Saporta, believe with Brongniart that it is possible to separate *Sigillaria* widely from *Lepidodendron*. They leave the latter plant amongst the *Lycopods*, and elevate the former to the rank of a Gymnospermous exogen. I have in vain demonstrated the existence of a large series of specimens of the same species of plant, young states of which display all the essential features of structure which they believe to characterise *Lepidodendron*, whilst, in its progress to maturity, every stage in the development of the secondary wood, regarded by them as characteristic of a *Sigillaria*, can be followed step by step.[1] Nay, more: my cabinet contains specimens of young dichotomously branching twigs, on which one of the two diverging branches has only the centripetal cylinder of the *Lepidodendron*, whilst the other has begun to develop the secondary wood of the *Sigillaria*.[2]

The distinguished botanist of the Institut, Ph. van Tieghem, has recently paid some attention to the conclusions adopted by his three countrymen in this controversy, and has made an important advance upon those conclusions, in what I believe to be the right direction. He recognises the Lycopodiaceous character of the *Sigillariæ*, and their close relations to the *Lepidodendra*;[3] and he also accepts my demonstration of the unipolar, and consequently Lycopodiaceous, character of the fibro-vascular bundle of the Stigmarian rootlet, a peculiarity of structure of which M. Renault has hitherto denied the existence. But along with these recognitions of the accuracy of my conclusions he gives fresh currency to several of the old errors relating to parts of the subject to which he has not yet given personal attention. Thus he considers that the *Sigillariæ*, though closely allied to the *Lepidodendra*, are distinguished from them by possessing the power of developing the centrifugal or exogenous zone of vascular tissue already referred to. He characterises the *Lepidodendra* as having "*un seul bois centripète*," notwithstanding the absolute demonstrations to the contrary contained in my "Memoir" xi. Dealing with the root of *Sigillaria*, which in Great Britain at least is the well-known *Stigmaria ficoides*, following Renault, he designates it a "*rhizome*," limiting the term root to what we designate the rootlets. He says, "Le rhizome des Sigillaires a la même structure que la tige aérienne, avec des bois primaires tantôt isolés à la périphérie de la moelle, tantôt confluents au centre et en un axe plein; seulement les fasceaux libéro-ligneux secondaires y sont séparés par de plus larges rayons," &c.

Now, *Stigmaria* being a root, and not a rhizome, contains no representative of the primary wood of the stem. This latter is, as even M. Brongniart so correctly pointed out long ago, the representative of the medullary sheath, and the fibro-vascular bundles which it gives off are all foliar ones, as is the case with the bundles given off by this sheath in all exogenous plants. But in the *Lepidodendra* and *Sigillariæ*, as in all living exogens, it is not prolonged into the root. In the latter, as might be expected *a priori*, we only find the secondary or exogenous vascular zone. Having probably the largest collection of sections of *Stigmariæ* in the world, I speak unhesitatingly on these points. M. van Tieghem further says, 'La tige aérienne part d'un rhizome rameux très-développé nommé *Stigmaria*, sur lequel s'insèrent à la fois de petites feuilles et des racines parfois dichotomées.' I have yet to see a solitary fact justifying the statement that leaves are intermingled with the rootlets of *Stigmaria*. The statement rests upon an entire misinterpretation of sections of the fibro-vascular bundles supplying those rootlets and an ignorance of the nature and positions of the rootlets themselves. More than forty years have elapsed since John Eddowes Bowman first demonstrated that the *Stigmariæ* were true roots, and every subsequent British student has confirmed Bowman's accurate determination.

M. Lesquereux informs me that his American experiences have convinced him that *Sigillaria* is Lycopodiaceous. Dr. Dawson has now progressed so far in the same direction as to believe that there exists a series of Sigillarian forms which link the *Lepidodendra* on the one hand with the Gymnospermous exogens on the other. As an evolutionist I am prepared to accept the possibility that such links may exist. They certainly do, so far as the union of *Lepidodendron* with *Sigillaria* is concerned. I have not yet seen any from the higher part of the chain that are absolutely satisfactory to me, but Dr. Dawson thinks that he has found such. I may add that Schimper and the younger German school have always associated *Sigillaria* with the *Lycopodiaceæ*. But there are yet other points under discussion connected with these fossil Lycopods.

M. Renault affirms that some forms of *Halonia* are subterranean rhizomes, and the late Mr. Binney believed that *Haloniæ* were the roots of *Lepidodendron*. I am not acquainted with a solitary fact justifying either of these suppositions, and unhesitatingly reject them. We have the clearest evidence that some *Haloniæ* at least are true terminal, and, as I believe, strobilus-bearing, branches of various Lepidodendroid plants, and I see no reason whatever for separating *Halonia regularis* from those whose fruit-bearing character is almost absolutely determined. Its branches, like the others, are covered throughout their entire circumference, and in the most regularly symmetrical manner, with leaf-scars, a feature wholly incompatible with the idea of the plant being either a root or a rhizome. M. Renault has been partly led astray in this matter by misinterpreting a figure of a specimen published by the late Mr. Binney. That specimen being now in the museum of Owens College, we are able to demonstrate that it has none of the features which M. Renault assigns to it.

The large round or oval distichously-arranged scars of *Ulodendron* have long stimulated discussion as to their nature. This, too, is now a well-understood matter. Lindley and Hutton long ago suggested that they were scars whence cones had been detached, a conclusion which was subsequently sustained by Dr. Dawson and Schimper, and which structural evidence led me also to support.[1] The matter was set at rest by Mr. d'Arcy Thompson's discovery of specimens with the strobili *in situ*. Only a small central part of the conspicuous cicatrix characterising the genus represented the area of organic union of the cone to the stem. The greater part of that cicatrix has been covered with foliage, which, owing to the shortness of the cone-bearing branch, was compressed by the base of the cone. The large size of many of these biserial cicatrices on old stems has been due to the considerable growth of the stem subsequently to the fall of the cone.

Our knowledge of the terminal branches of the large-ribbed *Sigillariæ* is still very imperfect. Palæontologists who have urged the separation of the *Sigillariæ* from the *Lepidodendra* have attached weight to the difference between the longitudinally-ridged and furrowed external bark of the former plants, along which ridges the leaf-scars are disposed in vertical lines, and the diagonally-arranged scars of *Lepidodendron*. They have also dwelt upon the alleged absence of branches from the Sigillarian stems. I think that their mistake, so far as the branching is concerned, has arisen from their expectation that the branches must necessarily have had the same vertically-grooved appearance, and longitudinal arrangement of the leaf-scars, as they observed in the more aged trunks; hence they have probably *seen* the branches of *Sigillariæ* without *recognising* them. Personally I believe this to have been the case. I further entertain the belief that the transition from the *vertical* phyllotaxis, or leaf arrangement of the Sigillarian leaf-scars, to the *diagonal* one of the *Lepidodendra* will ultimately be found to be effected through the subgenus *Favularia*, in many of which the diagonal arrangement becomes quite as conspicuous as the vertical one. This is the case even in Brongniart's classic specimen of *Sigillaria elegans*, long the only fragment of that genus known which preserved its internal structure. The fact is, the shape of the leaf-scars, as well as their proximity to each other, underwent great changes as Lepidodendroid and Sigillarian stems advanced from youth to age. Thus Presl's genus Bergeria was based on forms of Lepidodendroid scars which we now find on the terminal branches of unmistakable *Lepidodendra*.[2] The phyllotaxis of *Sigillaria*, of the type of *S. occulata*, passes by imperceptible gradations into that of *Favularia*. In many young branches the leaves were densely crowded together, but the exogenous development of the interior of the stem, and its consequent growth both in length and thickness, pushed these scars apart at the same time that it increased their size and altered their shape. We see precisely the same effects produced upon the large fruit scars of Ulodendron by the same causes. The Carboniferous Lycopods were mostly arborescent, but some few dwarf forms, apparently like the modern *Selaginellæ*, have been found in the Saarbrücken coal-fields. Many, if not all, the arborescent forms produced secondary wood, by means of a cambium-layer, as they increased in age. In the case of some of them[3] this was done in a very rudimentary manner, nevertheless sufficiently so to demonstrate

[1] "Memoir" xi. Plates xlvii.—lii. [2] *Idem.* Pl. xlix. Fig. 8.
[3] "Traité de Botanique," p. 1304.

[1] "Memoir" ii. p. 222. [2] See "Memoir" xii. Pl. xxxiv.
[3] *E.g. L. Harcourtii*, "Memoir" ix. Pl. xlix. Fig. 11.

what is essential to the matter, viz. the existence of a cambium-layer producing "centrifugal growth of secondary vascular tissue."

As already pointed out in the case of the Calamites, the vascular axis of these *Lepidodendra* was purely an *appareil con ucteur*, unmixed with any wood cells; hence the *appareil de soutien* had to be supplied elsewhere. This was done, as in the Calamites: a thick, persistent, hypodermal zone of meristem[1] developed a layer of prismatic prosenchyma of enormous thickness,[2] which incased the softer structures in a strong cylinder of self-supporting tissue. We have positive evidence that the fructification of many of these plants was in the form of heterosporous strobili. Whether or not such was the case with all these *Lepidostrobi* we are yet unable to determine. But the incalculable myriads of their macrospores, seen in so many coals, afford clear evidence that the heterosporous types must have preponderated vastly over all others.

Gymnosperms.—Our knowledge of this part of the Carboniferous vegetation has made great progress during the last thirty years. This progress began with my own discovery[3] that all our British *Dadoxylons* possessed what is termed a discoid pith, such as we see in the white jasmine, some of the American hickories, and several other plants; at the same time I demonstrated that most of our objects hitherto known as *Artisias* and *Sternbergias* were merely inorganic casts of these discoid medullary cavit es. Further knowledge of this genus seems to suggest that it was not only the oldest of the true Conifers in point of time, but also one of the lowest of the coniferous types.

Cycads.—The combined labours of Grand Eury, Brongniart, and Renault have revealed the unexpected predominance in some localities of a primitive but varied type of Cycadean vegetation. Observers have long been familiar with certain seeds known as *Trigonocarpons* and *Cardiocarpons*, and with large leaves to which the name of *Noeggerathia* was given by Sternberg. All these seeds and leaves have been tossed from family to family at the caprice of different classifiers, but in all cases without much knowledge on which to base their determinations. The rich mass of material disinterred by M. Grand-Eury at St. Etienne, and studied by Brongniart and M. Renault, has thrown a flood of light upon some of these objects, which now prove to be primæval types of Cycadean vegetation.

Mr. Peach's discovery of a specimen demonstrating that the *Antholithes Pitcairniæ*[4] of Lindley and Hutton was not only, as these authors anticipated, "the inflorescence of some plant," but that its seeds were the well-known *Cardiocarpons*, was the first link in an important chain of new evidence. Then followed the rich discoveries at St. Etienne, where a profusion of seeds, displaying wonderfully their internal organisation, was brought to light by the energy of M. Grand-Eury, which seeds M. Brongniart soon pronounced to be Cycadean. At the same time I was obtaining many similar seeds from Oldham and Burntisland, in which also the minute organisation was preserved. Dawson, Newberry, and Lesquereux have also shown that many species of similar seeds, though with no traces of internal structure, occur in the coal-measures of North America.

Equally important was the further discovery by M. Grand-Eury that the *Antholithes*, with their Cardiocarpoid seeds, were but one form of the monoclinous catkin-like inflorescences of the *Noeggerathiæ*, now better known by Unger's name of *Cordaites*. These investigations suggest some important conclusions: 1st. The vast number and variety of these Cycadean seeds, as well as the enormous size of some of them, is remarkable, showing the existence of an abundant and important Carboniferous vegetation, of most of which no trace has yet been discovered other than these isolated seeds. 2nd. Most of the seeds exhibit the morphological peculiarity of having a large cavity (the "cavité pollinique" of Brongniart) between the upper end of the nucelle and its investing episperm, and immediately below the micropile of the seed. That this cavity was destined to have the pollen grains drawn into it, and be thus brought into direct connection with the apex of the nucelle, is shown by the various examples in which such grains are still found in that cavity.[1] 3rd. M. Grand-Eury has shown that some of his forms of *Cordait s* possessed the discoid or Sternbergian pith which I had previously found in *Dadoxylon*; and, lastly, these *Cordaites* prove that a diclinous form of vegetation existed at this early period in the history of the flowering plants, but whether in a monœcious or a diœcious form we have as yet no means of determining. Their reproductive structures differ widely from the true cones borne by most Cycads at the present day.

Conifers.—It has long been remarked that few real cones of Conifers have hitherto been found in the Carboniferous rocks, and I doubt if any such have yet been met with. Large quantities of the woody stems now known as *Dadoxylons* have been found both in Europe and America. These stems present a true coniferous structure both in the pith, medullary sheath, wood, and bark.[2] The wood presents one very peculiar feature. Its foliar bundles, though in most other respects exactly like those of ordinary Conifers, are given off, not singly, but in pairs.[3] I have only found this arrangement of double foliar bundles in the Chinese Gingko (*Salisburia adiantifolia*).[4] This fact is not unimportant when connected with another one. Sir Joseph Hooker long ago expressed his opinion that the well-known *Trigonocarpons*[5] of the coal measures were the seeds of a Conifer allied to this *Salisburia*. The abundance of the fragments of *Dadoxylon*, combined with the readiness with which cones and seeds are preserved in a fossil state, make it probable that the fruits belonging to these woody stems would be so preserved. But of cones we find no trace, and, as we discover no other plant in the Carboniferous strata to which the *Trigonocarpons* could with any probability have belonged, these facts afford grounds for associating them with the *Dadoxylons*. These combined reasons, viz. the structure of the stems with their characteristic foliar bundles, and the Gingko-like character of the seeds, suggest the probability that these *Dadoxylons*, the earliest of known Conifers, belonged to the *Taxineæ*, the lowest of these coniferous types, and of which the living *Salisburia* may perhaps be regarded as the least advanced recent form.

Thus far our attention has been directed only to plants whose affinities have been ascertained with such a degree of probability as to make them available witnesses, so far as they go, when the question of vegetable evolution is *sub judice*. But there remain others, and probably equally important ones, respecting which we have yet much to learn. In most cases we have only met with detached portions of these plants, such as stems or reproductive structures, which we are unable to connect with their other organs. The minute tissues of these plants are preserved in an exquisite degree of perfection; hence we are able to affirm that, whatever they may be, they differ widely from every type that we are acquainted with amongst living ones. The exogenous stems or branches from Oldham and Halifax which I described under the name of *Astromyelon*,[6] and of which a much fuller description will be found in my forthcoming Memoir xii., belong to a plant of this description The remarkable conformation of its bark obviously indicates a plant of more or less aquatic habits, since it closely resembles those of *Myriophyllum*, *Marsilea*, and a number of other aquatic plants belonging to various classes. But its general features suggest nearer affinities to the latter genus than to any other. Another very characteristic stem is the *Heterangium Grievii*,[7] only found in any quantity at Burntisland, but of which we have recently obtained one or two small specimens at Halifax. This plant displays an abundant supply of primary, isolated, vascular bundles, surrounded by a very feeble development of secondary vascular tissue. Still more remarkable is the *Lyginodendron Oldhamium*,[8] a stem not uncommon at Oldham, and not unfrequently found at Halifax. Unlike the *Heterangium*, its primary vascular elements are feeble, but its tendency to develop secondary zylem is very characteristic of the plant. An equally peculiar feature is seen in the outermost layer of its cellular bark, which is penetrated by innumerable longitudinal laminæ of prosenchymatous tissue, which is arranged in precisely the same way as is the hard bast in the lime and similar trees,

[1] "Memoir" ix. Pl. xxv. Figs. 93, 94, 98, 99, 100, and 101.

[2] "Memoir" xi. Pl. xlviii. Fig. 4 *ff'*. "Memoir" ii. Pl. xxix. Fig. 42 *k*. "Memoir" iii. Pl. xliii. Fig. 17.

[3] "On the Structure and Affinities of the Plants hitherto known as Sternbergias," "Memoirs of the Literary and Philosophical Society of Manchester," 1851. M. Renault, in his "Structure comparée de quelques Tiges de la Flore Carbonifère" p. 285, has erroneously attributed this discovery to Mr. Dawes, including my illustration from the Jasminium and Juglans. Mr. Dawes' explanation was a very different one.

[4] "Fossil Flora," p. 82.

[1] "Memoir" viii. Pl. ii. Figs. 70 and 72. Brongniart, "Recherches sur les Graines Fossiles Silicifiées," Pl. xvi. Figs. 1, 2; Pl. xx. Fig. 2.

[2] Dr. Dawson finds the discoid pith in one of the living Canadian Conifers.

[3] "Memoir" viii. Pl. lviii. Fig. 48, and Pl. ix. Figs. 44-46.

[4] "Memoir" xii. Pl. xxxiii. Figs. 28, 29.

[5] "Memoir" viii. Figs. 94-115.

[6] "Memoir" ix., in which I only described decorticated specimens Messrs. Cash and Hick described a specimen io which the peculiar bark was preserved under the name of *Astromyelon Williamsonis*. See "Proceedings of the Yorkshire Polytechoic Society," vol. vii. part iv. 1881.

[7] "Memoir" iii. [8] "Memoir" iii.

affording another example of the introduction into the outer bark of the *appareil de soutien*. As might have been anticipated from this addition to the bark, this plant attained arborescent dimensions, very large fragments of sandstone casts of the exterior surface of the bark[1] being very abundant in most of the leading English coal-fields. Corda also figured it[2] from Radnitz, confounding it, however, with his Lepidodendroid *Sagenaria fusiformis*, with which it has no true affinity. Of the smaller plants of which we know the structure but not the systematic position, I may mention the beautiful little *Kaloxylons*.[3] We have also obtained a remarkable series of small spherical bodies, to which I have given the provisional generic name of *Sporocarpon*.[4] Their external wall is multicellular; hence they cannot be spores. Becoming filled with free cells, which display various stages of development as they advance to maturity, we may infer that they are reproductive structures. Dr. Dawson informs me that he has recently obtained some similar bodies, also containing cells, from the Devonian beds of North and South America. Except in calling attention to some slight resemblance existing between my objects and the sporangiocarps of *Pilularia*,[5] I have formed no opinion respecting their nature. Dr. Dawson has pointed out that his specimens also suggest relations with the Rhizocarpæ.

I am unwilling to close this address without making a brief reference to the bearing of our subject upon the question of evolution. Various attempts have been made to construct a genealogical tree of the vegetable kingdom. That the Cryptogams and Gymnosperms made their appearance, and continued to flourish on this earth, long prior to the appearance of the monocotyledonous and dicotyledonous flowering plants, is at all events a conclusion justified by our present knowledge so far as it goes. Every one of the supposed Palms, Aroids, and other Monocotyledons has now been ejected from the lists of Carboniferous plants, and the Devonian rocks are equally devoid of them. The generic relations of the Carboniferous vegetation to the higher flowering plants found in the newer strata have no light thrown upon them by these Palæozoic forms. These latter do afford us a few plausible hints respecting some of their Cryptogamic and Gymnospermous descendants, and we know that the immediate ancestors of many of them flourished during the Devonian age, but here our knowledge practically ceases. Of their still older genealogies scarcely any records remain. When the registries disappeared, not only had the grandest forms of Cryptogamic life that ever lived attained their highest development, but even the yet more lordly Gymnosperms had become a widely diffused and flourishing race. If there is any truth in the doctrine of evolution, and especially if long periods of time were necessary for a world-wide development of lower into higher races, a terrestrial vegetation must have existed during a vast succession of epochs ere the noble Lycopods began their prolonged career. Long prior to the Carboniferous age they had not only made this beginning, but during that age they had diffused themselves over the entire earth. We find them equally in the Old World and in the New. We discover them from amid the ice-clad rocks of Bear Island and Spitzbergen to Brazil and New South Wales. Unless we are prepared to concede that they were simultaneously developed at these remote centres, we must recognise the incalculable amount of time requisite to spread them thus from their birthplace, wherever that may have been, to the ends of the earth. Whatever may have been the case with the southern hemisphere, we have also clear evidence that in the northern one much of this wide distribution must have been accomplished prior to the Devonian age. What has become of this pre-Devonian flora? Some contend that the lower cellular forms of plant life were not preserved because their delicate tissues were incapable of preservation. But why should this be the case? Such plants are abundantly preserved in Tertiary strata, why not equally in Palæozoic ones? The explanation must surely be sought, not in their incapability of being preserved, but in the operation of other causes. But the Carboniferous rocks throw another impediment in the way of constructors of these genealogical trees. Whilst Carboniferous plants are found at hundreds of separate localities, widely distributed over the globe, the number of spots at which these plants are found displaying any internal structure is extremely few. It would be difficult to enumerate a score of such spots. Yet each of those favoured localities has revealed to us forms of plant life of which the ordinary plant-bearing shales and sandstones of the same localities show no traces. It seems, therefore, that whilst there was a general resemblance in the more conspicuous forms of Carboniferous vegetation from the Arctic circle to the extremities of the southern hemisphere, each locality had special forms that flourished in it either exclusively or at least abundantly, whilst rare elsewhere. It would be easy, did time allow, to give many proofs of the truth of this statement. Our experiences at Oldham and Halifax, at Arran and Burntisland, at St. Etienne and Autun, tell us that such is the case. If these few spots which admit of being searched by the aid of the microscope have recently revealed so many hitherto unknown treasures, is it not fair to conclude that corresponding novelties would have been furnished by all the other plant-producing localities if these plants had been preserved in a state capable of being similarly investigated? I have no doubt about this matter; hence I conclude that there is a vast variety of Carboniferous plants of which we have as yet seen no traces, but every one of which must have played some part, however humble, in the development of the plant races of later ages. We can only hope that time will bring these now hidden witnesses into the hands of future palæontologists. Meanwhile, though far from wishing to check the construction of any legitimate hypothesis calculated to aid scientific inquiry, I would remind every too ambitious student that there is a haste that retards rather than promotes progress; that arouses opposition rather than produces conviction; and that injures the cause of science by discrediting its advocates.

[1] "Memoir" iv. Pl. xxvii.
[2] "Flora der Vorvelt," Tab. 6, Fig. 4.
[3] "Memoir" vii. [4] "Memoirs" ix. x. [5] "Memoir" ix. p. 348.

NOTES

We are glad to be able to publish this week an article by a distinguished foreign botanist on Bentham and Hooker's great work, "Genera Plantarum."

We regret to announce the death, on the 15th inst., of the eminent physicist, M. Joseph-Antoine-Ferdinand Plateau, Emeritus Professor at the University of Ghent. Professor Plateau was a Foreign Member of the Royal Society, Member of the Academy of Sciences of Berlin, and Corresponding Member of the Paris Academy of Sciences. He was in his eighty-second year.

Admiral Sir Richard Collinson, K.C.B., Deputy Master of the Trinity Corporation, died last week at his residence, Haven Green, Ealing. He was born in 1811 at Gateshead, of which place his father was rector. He entered the navy in 1823, was employed in various surveying expeditions under Captain Belcher and others from 1831 to 1839, took an active part in the first Chinese war, and remained afterwards four years on the China coast, making plans of harbours and laying down the coast line. He commanded the expedition, consisting of the *Enterprise* and *Investigator*, despatched by the Admiralty in 1850 in search of Sir John Franklin and his companions, and on his return to England in 1854 Captain Collinson received the medal of the Royal Geographical Society for his explorations in Arctic regions. He received his promotion to flag rank in 1862, was elected an Elder Brother of the Trinity House in the same year, and has been Deputy Master of that Corporation since 1875.

The death is announced of Mr. Werdermann, the inventor of the well-known semi-incandescent electric light.

Herr Marno, the well-known explorer of North Central Africa, has died at Khartoum.

The Astronomische Gesellschaft met in Vienna last week.

The Lord President of the Committee of Council on Education has appointed Valentine Ball, M.A., F.R.S., Professor of Geology and Mineralogy in the University of Dublin, Director of the Dublin Museum of Science and Art. Prof. V. Ball is the brother of the Astronomer Royal for Ireland, and the author of several interesting and important works, among which may be enumerated "The Economic Geology of India" and "Experiences of Jungle Life in India"; his appointment is regarded as in every way an excellent one. In addition to his geological

attainments, Prof. V. Ball is also known by his papers on various ethnological subjects. This appointment will leave the Chair of Geology and Mineralogy in the University of Dublin vacant after next Michaelmas Term.

THE Improvement Commissioners of Bournemouth, at a meeting on Tuesday, discussed the desirability of inviting the British Association to visit Bournemouth. It was unanimously decided to invite it for 1885.

THE last news received by the Russian Geographical Society from the Lena meteorological station is dated April 3. The observers have suffered to some extent from the hard winter, and especially from the winds, and it was with difficulty that they succeeded in maintaining a moderate temperature in their house. Still they were all in good health. The lowest temperature observed was $-52^{\circ}\cdot3$ Celsius on February 9. In January and February it usually did not fall below -40°, excepting during quite calm weather. In March the thermometer oscillated about -40°, and at the beginning of April it began to rise to -19°. M. Yurgens found great difficulties with the magnetic instruments, the range of deviation of the needles during the magnetic perturbations being as much as 25° from the magnetic meridian, and those which measure the horizontal intensity showing deviations of as much as 90°.

THE subterranean rooms of the Paris Observatory are ready for the reception of the magnetic instruments. Three sets will be arranged—one for registering, the second for direct observation as established by Lamont at Munich, and the third will be composed of the old instruments used by Arago for comparing the numbers taken in former times.

CIRCUMSTANCES, says *Science*, were not favourable to the production of remarkable essays at the recent meeting of the American Association. The attendance was not large. The officers of the meeting, and especially those who had to make addresses, could scarcely be expected to produce elaborate papers in addition to their other labours. As the number of addresses per meeting has increased, we may observe more readily some of the effects of the system that demands them. The most evident result is that usually where we gain one good address we lose two or three good papers. The distance of the meeting from their homes affected especially members of Sections A, B, C, and D, devoted to the exact sciences. Perhaps it affected the quality as well as the number of their papers. There were not many from the east to present essays, though quite as many as could have reasonably been expected; but there were scarcely any from the locality of the meeting and its neighbourhood. Local interest, both as to authors and hearers, was of course deficient. In short, there was nothing remarkable in those sections to spur production, and the product was not remarkable. It was good, but not great.

THE fourth annual "Cryptogamic Meeting" of the Essex Field Club will take place in Epping Forest on Saturday, September 29. A large number of botanists have promised to be present and act as referees. In the evening a meeting for the exhibition of botanical specimens will be held in the Assembly Room at the "Roebuck" Hotel, Buckhurst Hill, when the following papers will be read:—"Recent Additions to the Fungus Flora of Epping Forest," by Dr. M. C. Cooke, M.A., F.L.S.; "The 'Lower Orders' of Fungi," by Worthington G. Smith, F.L.S.; "Fungi as Poisons," by Dr. Wharton, M.A., F.L.S. Botanists wishing to attend the meeting or to exhibit specimens should communicate with the Hon. Secretary, Mr. W. Cole, Buckhurst Hill, Essex.

MR. SIMMONS and a companion left Hastings in a balloon at 3.20 p.m. on Thursday last, and landed in about seven hours at Cape La Hogue, in France.

THE additions to the Zoological Society's Gardens during the past week include two Chinese Rhesus Monkeys (*Macacus lasiotus* ♂ ♀) from China, presented by Mr. G. A. Conder; a Pig-tailed Monkey (*Macacus nemestrinus* ♂) from Java, presented by Mr. Robert Smith; a Hog Deer (*Cervus porcinus* ♂) from India, presented by Mr. D. Charles Horne; a Snow Bunting (*Plectrophanes nivalis*), European, presented by Mr. E. J. Gibbins; two Ring Doves (*Columba palumbus*), British, presented by Mrs. Courage; two Land Rails (*Crex pratensis*), British, presented by Dr. Marshall; a Robben Island Snake (*Coronella phocarum*), a Rufescent Snake (*Leptodira rufescens*), a Ring-hals Snake (*Sepedon hæmachetes*) from South Africa, presented by the Rev. G. H. R. Fisk, C.M.Z.S.; a Grey Seal (*Halichœrus gryphus*) from Cornwall, two Margined Tortoises (*Testudo marginata*), South European, a Glass Snake (*Pseudopus pallasi*) from Dalmatia, deposited.

A PLEA FOR PURE SCIENCE[1]

I AM required to address the so-called Physical Section of this Association. Fain would I speak pleasant words to you on this subject; fain would I recount to you the progress made in this subject by my countrymen, and their noble efforts to understand the order of the universe. But I go out to gather the grain ripe to the harvest, and I find only tares. Here and there a noble head of grain rises above the weeds; but so few are they that I find the majority of my countrymen know them not, but think that they have a waving harvest, while it is only one of weeds after all. American science is a thing of the future, and not of the present or past; and the proper course of one in my position is to consider what must be done to create a science of physics in this country, rather than to call telegraphs, electric lights, and such conveniences by the name of science. I do not wish to underrate the value of all these things: the progress of the world depends on them, and he is to be honoured who cultivates them successfully. So also the cook who invents a new and palatable dish for the table, benefits the world to a certain degree; and yet we do not dignify him by the name of a chemist. And yet it is not an uncommon thing, especially in American newspapers, to have the *applications* of science confounded with pure science; and some obscure American who steals the ideas of some great mind of the past and enriches himself by the application of the same to domestic uses, is often lauded above the great originator of the idea, who might have worked out hundreds of such applications had his mind possessed the necessary element of vulgarity. I have often been asked which was the more important to the world, pure or applied science. To have the applications of a science, the science itself must exist. Should we stop its progress and attend only to its applications, we should soon degenerate into a people like the Chinese, who have made no progress for generations, because they have been satisfied with the applications of science, and have never sought for reasons in what they have done. The reasons constitute pure science. They have known the application of gunpowder for centuries; and yet the reasons for its peculiar action, if sought in the proper manner, would have developed the science of chemistry, and even of physics, with all their numerous applications. By contenting themselves with the fact that gunpowder would explode, and seeking no further, they have fallen behind in the progress of the world; and we now regard this oldest and most numerous of nations as only barbarians. And yet our own country is in this same state. But we have done better; for we have taken the science of the Old World and applied it to all our uses, accepting it like the rain of heaven, without asking whence it came, or even acknowledging the debt of gratitude we owe to the great and unselfish workers who have given it to us. And, like the rain of heaven, this pure science has fallen upon our country, and made it great and rich and strong.

To a civilised nation of the present day the applications of science are a necessity; and our country has hitherto succeeded in this line only for the reason that there are certain countries in the world where pure science has been and is cultivated, and where the study of nature is considered a noble pursuit. But such countries are rare, and those who wish to pursue pure

[1] Condensed abstract of the address of Prof. H. A. Rowland of Baltimore, vice-president of Section B (Physics), before the American Association at Minneapolis, August 15. In using the word science the author refers to physical science, "as I know nothing of natural science. Probably my remarks will, however, apply to both, but I do not know."

science in our own country must be prepared to face public opinion in a manner which requires much moral courage. They must be prepared to be looked down upon by every successful inventor whose shallow mind imagines that the only pursuit of mankind is wealth, and that he who obtains most has best succeeded in this world. Everybody can comprehend a million of money; but how few can comprehend any advance in scientific theory; especially in its more abstruse portions! And this, I believe, is one of the causes of the small number of persons who have ever devoted themselves to work of the higher order in any human pursuit. Man is a gregarious animal, and depends very much, for his happiness, on the sympathy of those around him; and it is rare to find one with the courage to pursue his own ideals in spite of his surroundings. In times past, men were more isolated than at present, and each came in contact with a fewer number of people. Hence that time constitutes the period when the great sculptures, paintings, and poems were produced. Each man's mind was comparatively free to follow its own ideals, and the results were the great and unique works of the ancient masters. To-day, the railroad and the telegraph, the books and newspapers, have united each individual man with the rest of the world: instead of his mind being an individual, a thing apart by itself, and unique, it has become so influenced by the outer world, and so dependent upon it, that it has lost its originality to a great extent. The man who in times past would naturally have been in the lowest depths of poverty, mentally and physically, to-day measures tape behind a counter, and with lordly air advises the naturally born genius how he may best bring his outward appearance down to a level with his own. A new idea he never had, but he can at least cover his mental nakedness with ideas imbibed from others. So the genius of the past soon perceives that his higher ideas are too high to be appreciated by the world: his mind is clipped down to the standard form; every natural offshoot upwards is repressed, until the man is no higher than his fellows. Hence the world, through the abundance of its intercourse, is reduced to a level. What was formerly a grand and magnificent landscape, with mountains ascending above the clouds, and depths whose gloom we cannot now appreciate, has become serene and peaceful. The depths have been filled, and the heights levelled, and the wavy harvests and smoky factories cover the landscape.

As far as the average man is concerned, the change is for the better. The average life of man is far pleasanter, and his mental condition better, than before. But we miss the vigour imparted by the mountains. We are tired of mediocrity, the curse of our country. We are tired of seeing our artists reduced to hirelings, and imploring Congress to protect them against foreign competition. We are tired of seeing our countrymen take their science from abroad, and boast that they here convert it into wealth. We are tired of seeing our professors degrading their chairs by the pursuit of applied science instead of pure science; or sitting inactive while the whole world is open to investigation; lingering by the wayside while the problem of the universe remains unsolved.

For generations there have been some few students of science who have esteemed the study of nature the most noble of pursuits. Some have been wealthy, and some poor; but they have all had one thing in common—the love of nature and its laws. To these few men the world owes all the progress due to applied science, and yet very few ever received any payment in this world for their labours.

But there will be those in the future, as well as in the past, who will do so; and for them higher prizes than any yet obtained are waiting. We have but yet commenced our pursuit of science, and stand upon the threshold wondering what there is within. We explain the motion of the planet by the law of gravitation; but who will explain how two bodies, millions of miles apart, tend to go toward each other with a certain force?

We now weigh and measure electricity and electric currents with as much ease as ordinary matter, yet have we made any approach to an explanation of the phenomenon of electricity? Light is an undulatory motion, and yet do we know what it is that undulates? Heat is motion, yet do we know what it is that moves? Ordinary matter is a common substance, and yet who shall fathom the mystery of its internal constitution?

How shall we, then, honour the few, the very few, who, in spite of all difficulties, have kept their eyes fixed on the goal, and have steadily worked for pure science, giving to the world a most precious donation, which has borne fruit in our greater knowledge of the universe and in the applications to our physical life which have enriched thousands and benefited each one of us? There are also those who have every facility for the pursuit of science, who have an ample salary and every appliance for work, yet who devote themselves to commercial work, to testifying in courts of law, and to any other work to increase their present large income. Such men would be respectable if they gave up the name of professor, and took that of consulting chemists or physicists. And such men are needed in the community. But for a man to occupy the professor's chair in a prominent college, and, by his energy and ability in the commercial applications of his science, stand before the local community in a prominent manner, and become the newspaper exponent of his science, is a disgrace both to him and his college. It is the deathblow to science in that region. Call him by his proper name, and he becomes at once a useful member of the community. Put in his place a man who shall by precept and example cultivate his science, and how different is the result! Young men, looking forward into the world for something to do, see before them this high and noble life, and they see that there is something more honourable than the accumulation of wealth. They are thus led to devote their lives to similar pursuits, and they honour the professor who has drawn them to something higher than they might otherwise have aspired to.

I do not wish to be misunderstood in this matter. It is no disgrace to make money by an invention, or otherwise, or to do commercial scientific work under some circumstances. But let pure science be the aim of those in the chairs of professors, and so prominently the aim that there can be no mistake. If our aim in life is wealth, let us honestly engage in commercial pursuits and compete with others for its possession. But if we choose a life which we consider higher, let us live up to it, taking wealth or poverty as it may chance to come to us, but letting neither turn us aside from our pursuit.

The work of teaching may absorb the energies of many; and indeed this is the excuse given by most for not doing any scientific work. But there is an old saying that where there is a will there is a way. Few professors do as much teaching or lecturing as the German professors, who are also noted for their elaborate papers in the scientific journals. A university should not only have great men on its faculty, but have numerous minor professors and assistants of all kinds, and should encourage the highest work, if for no other reason than to encourage the student to his highest efforts. But, assuming that the professor has high ideals, wealth such as only a large and high university can command is necessary to allow him the fullest development.

And this is specially so in our science of physics. In the early days of physics and chemistry many of the fundamental experiments could be performed with the simplest apparatus. And so we often find the names of Wollaston and Faraday mentioned as needing scarcely anything for their researches. Much can even now be done with the simplest apparatus; and nobody, except the utterly incompetent, need stop for want of it. But the fact remains that one can only be free to investigate in all departments of chemistry and physics, when he not only has a complete laboratory at his command, but a friend to draw on for the expenses of each experiment. That simplest of the departments of physics, namely, astronomy, has now reached such perfection that nobody can expect to do much more in it without a perfectly equipped observatory; and even this would be useless without an income sufficient to employ a corps of assistants to make the observations and computations.

But would it not be possible to so change public opinion that no college could be founded with a less endowment than say 1,000,000 dollars, or no university with less than three or four times that amount?

The total wealth of the 400 colleges and universities was in 1880 about 40,000,000 dollars in buildings, and 43,000,000 dollars in productive funds. This would be sufficient for one great university of 10,000,000 dollars, four of 5,000,000 dollars, and twenty-six colleges of 2,000,000 dollars each. But such an idea can of course never be carried out. Government appropriations are out of the question, because no political trickery must be allowed around the ideal institution.

In the year 1880 the private bequests to all schools and colleges amounted to about 5,500,000 dollars. We must make the need of research and of pure science felt in the country. We must live such lives of pure devotion to our science, that all shall see that we ask for money, not that we may live lives of indolent ease at the expense of charity, but that we may work for that which has advanced and will advance the world more than any other subject, both intellectually and physically. We must live

such lives as to neutralise the influence of those who in high places have degraded their profession, or have given themselves over to ease, and do nothing for the science which they represent. Let us do what we can with the present means at our disposal. There is not one of us who is situated in the position best adapted to bring out all his powers, and to allow him to do most for his science. All have their difficulties, and I do not think that circumstances will ever radically change a man. If a man has the instinct of research in him, it will always show itself in some form.

I do not believe anybody can be thorough in any department of science, without wishing to advance it. In the study of what is known, in the reading of the scientific journals, and the discussions therein contained of the current scientific questions, one would obtain an impulse to work, even though it did not before exist. And the same spirit which prompted him to seek what was already known, would make him wish to know the unknown. And I may say that I never met a case of thorough knowledge in my own science, except in the case of well-known investigators. I have met men who talked well, and I have sometimes asked myself why they did not do something; but further knowledge of their character has shown me the superficiality of their knowledge.

What would astronomy have done without the endowments of observatories? By their means, that science has become the most perfect of all branches of physics, as it should be from its simplicity. There is no doubt, in my mind, that similar institutions for other branches of physics, or, better, to include the whole of physics, would be equally successful. A large and perfectly equipped physical laboratory, with its large revenues, its corps of professors and assistants, and its machine-shop for the construction of new apparatus, would be able to advance our science quite as much as endowed observatories have astronomy. But such a laboratory should not be founded rashly. The value will depend entirely on the physicist at its head, who has to devise the plan, and to start it into practical working. Such a man would be always rare, and could not always be obtained. After one had been successfully started, others could follow; for imitation requires little brains.

One could not be certain of getting the proper man every time, but the means of appointment should be most carefully studied so as to secure a good average. There can be no doubt that the appointment should rest with a scientific body capable of judging the highest work of each candidate. Should any popular element enter, the person chosen would be either of the literary-scientific order, or the dabbler on the outskirts who presents his small discoveries in the most theatrical manner. What is required is a man of depth, who has such an insight into physical science that he can tell when blows will best tell for its advancement.

Such a grand laboratory as I describe does not exist in the world, at present, for the study of physics. But no trouble has ever been found in obtaining means to endow astronomical science. Everybody can appreciate, to some extent, the value of an observatory; as astronomy is the simplest of scientific subjects, and has very quickly reached a position where elaborate instruments and costly computations are necessary to further advance. The whole domain of physics is so wide that workers have hitherto found enough to do. But it cannot always be so, and the time has even now arrived when such a grand laboratory should be founded. Shall our country take the lead in this matter, or shall we wait for foreign countries to go before? They will be built in the future, but when and how is the question.

As stated before, men are influenced by the sympathy of those with whom they come in contact. It is impossible to immediately change public opinion in our favour; and, indeed, we must always seek to lead it, and not be guided by it. We must create a public opinion in our favour, but it need not at first be the general public. We must be contented to stand aside, and see the honours of the world for a time given to our inferiors; and must be better contented with the approval of our own consciences, and of the very few who are capable of judging our work, than of the whole world beside. Let us look to the other physicists, not in our own town, not in our own country, but in the whole world, for the words of praise which are to encourage us, or the words of blame which are to stimulate us to renewed effort. For what to us is the praise of the ignorant? Let us join together in the bonds of our scientific societies, and encourage each other, as we are now doing, in the pursuit of our favourite study; knowing that the world will some time recognise our services, and knowing, also, that we constitute the most important element in human progress.

But danger is also near, even in our societies. When the average tone of the society is low, when the highest honours are given to the mediocre, when third-class men are held up as examples, and when trifling inventions are magnified into scientific discoveries, then the influence of such societies is prejudicial. A young scientist attending the meetings of such a society soon gets perverted ideas. To his mind a molehill is a mountain, and the mountain a molehill. The small inventor or the local celebrity rises to a greater height, in his mind, than the great leader of science in some foreign land. He gauges himself by the molehill and is satisfied with his stature; not knowing that he is but an atom in comparison with the mountain, until, perhaps, in old age, when it is too late. But, if the size of the mountain had been seen at first, the young scientist would at least have been stimulated in his endeavour to grow.

We call this a free country, and yet it is the only one where there is a direct tax upon the pursuit of science. The low state of pure science in our country may possibly be attributed to the youth of the country; but a direct tax to prevent the growth of our country in that subject cannot be looked upon as other than a deep disgrace. I refer to the duty upon foreign books and periodicals. One would think that books in foreign languages might be admitted free; but to please the half-dozen or so workmen who reprint German books, not scientific, our free intercourse with that country is cut off.

The time is almost past, even in our own country, when third-rate men can find a place as teachers because they are unfit for everything else. We wish to see brains and learning, combined with energy and immense working power, in the professor's chair; but, above all, we wish to see that high and chivalrous spirit which causes one to pursue his idea in spite of all difficulties, to work at the problems of nature with the approval of his own conscience and not of men before him.

The whole universe is before us to study. The greatest labour of the greatest minds have only given us a few pearls; and yet the limitless ocean, with its hidden depths filled with diamonds and precious stones, is before us. The problem of the universe is yet unsolved, and the mystery involved in one single atom yet eludes us. The field of research only opens wider and wider as we advance, and our minds are lost in wonder and astonishment at the grandeur and beauty unfolded before us. Shall we help in this grand work, or not? Shall our country do its share, or shall it still live in the almshouse of the world?

CONTENTS

THURSDAY, SEPTEMBER 27, 1883

HERMANN MÜLLER'S "FERTILISATION OF FLOWERS"

The Fertilisation of Flowers. By Prof. Hermann Müller. Translated and Edited by D'Arcy W. Thompson, B.A., Scholar of Trinity College, Cambridge, with a Preface by Charles Darwin. With Illustrations. (London: Macmillan and Co., 1883.)

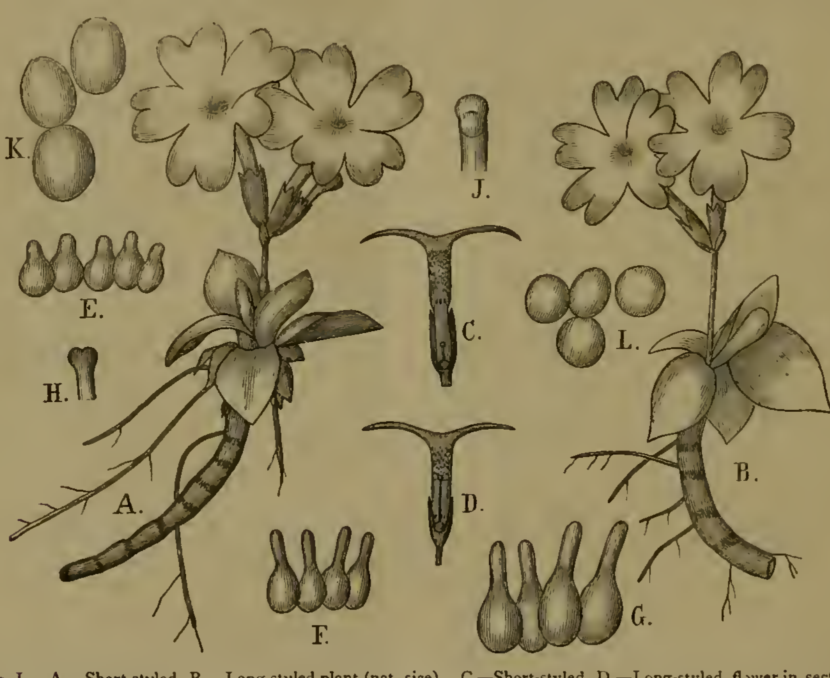
CHRISTIAN CONRAD SPRENGEL'S treatise on the structure and fertilisation of flowers, after well nigh a century of oblivion, has come to be recognised as one of the most interesting of books, and his theory of the adaptation of flowers to fertilisation by insects is one that will ever be associated with his name. In the "Origin of Species" Darwin referred to Sprengel's researches, and one of the results of the now well-known Chapter IV. of that great work was to show the value of Sprengel's labours, and this has caused his book to play a prominent part in the investigation of the prime causes which determine the forms of flowers. The idea of cross-fertilisation can scarcely be said to have established itself until 1859, and was a most powerful impetus to research based upon Sprengel's observations. First among the results we had Darwin's own work on Orchids and on plants with heterogynous forms of styles, and attracted by these there came a long line of other more or less able investigators, of whom Hildebrand, Delpino, Fritz Müller, and others may be mentioned—some devoting themselves more to the details of floral mechanisms, others to the proof of the advantages of cross-fertilisation. More comprehensive were the views of Hermann Müller, who, in 1872, published his important "Befruchtung der Blumen durch Insekten und die gegenseitigen Anpassungen beider." In this the author's aim was to consider each case of cross-fertilisation in all its possible bearings, the advantage to the flower and to the insect, and how the one in its contrivances to assure its ends acted and reacted on the other; there was the evolution of the powers of the insect step by step with some advantage to the plant. Naturally the scheme was too vast, too grand to be entirely accomplished through the labours, direct or indirect, of any one man; and, so far as regarded anthophilous insects, Hermann Müller chiefly confined his attention to the bees, describing the modifications which fit them for a floral part, and proving that such modifications had been gradually evolved. This work of H. Müller's has been the guide-book of a host of workers during these last eleven years, and we most cordially greet its appearance now in an English translation by Mr. D'Arcy W. Thompson. The very recent death of its painstaking and worthy author adds a peculiar interest to its publication; in it he has incorporated all his most recent observations, so that it is not only a translation but a new and importantly enlarged edition—a monument to his fame. We regret that the translator did not think fit to give us the author's preface, which, though but four pages, contained much of practical interest, gave us an insight into Müller's labours as a teacher of natural science in the High School at Lippstadt, and would have been a worthy affix to the genial prefatory notice of Charles Darwin. One other regret and we are done. Why are the modest but pregnant words on the title-page of the original nowhere alluded to in the translation? This work, for which Darwin felt grateful—this book containing "an enormous mass of original observations on the fertilisation of flowers and on the part which insects play in the work," we quote again Darwin—the author himself styles "Ein Beitrag zur Erkenntniss des Ursächlichen Zusammenhanges in der Organischen Natur," but the translation says nothing of this.

So far as we have been able to judge, the translation has been most successfully accomplished, but a great deal of new material has been added. Some of the original is omitted, and many new figures have been introduced. The systematic part of the book, most happily for the reader, has been rearranged from Endlicher's system to that of Bentham and Hooker's "Genera Plantarum." The translator, disliking the word "pollination" as a translation for "Bestäubung," "has throughout [not quite] used the word 'fertilisation' to imply application of pollen to the stigma, without definite reference to the result of the act; that is to say, he has translated 'Bestäubung' and 'Befruchtung' by the same English word." We would have much preferred the use of the "ungainly" word, though possibly a more gainly one might have been invented. For it is awkward in a scientific treatise to refer to a sterile fertilisation (Bestäubung), while a sterile besprinkling or dusting of pollen would sound no way queer; the two functions of besprinkling and fertilising at any rate are distinct, and we should have some way of saying so. The list of all works relating to the subject is a very important addition to the book, and the copious and well compiled indices deserve our grateful thanks. The translator's acknowledgments to his friend the assistant curator of Cambridge University Herbarium reminds us that not only since these pages were sent by him to press have Darwin and Hermann Müller ceased from their labours, but that Mr. T. H. Corry of Caius College has, in the very first promise of his career, and while in the pursuit of the very flowers he loved so well, fallen a victim to a boating accident, and added one to the memories that will cling around this volume.

As an example of the illustrations we are enabled to give the accompanying woodcut of the pretty Alpine Primrose (*Primula integrifolia*). It is one of the red-flowered heterostyled species, and is adapted for Lepidoptera by its colour and the narrowing of the mouth of the tube. It will be remembered that the species of primrose were the subject of a series of interesting researches by Darwin which showed that in the common Primrose (*P. veris*) the stigma in the long-styled form possesses papillæ three times as long as those of the short-styled form, and that the pollen grains of the long stamen are half as large again as those of the short. The same holds good of *P. auricula* and *P. sinensis*, and these Primulas are very unproductive in the absence of insects, but fully productive when artificially fertilised or when insects have access to them.

The last few pages of this translation treat of the subject of the origin of flowers, which has chiefly been discussed by Hermann Müller, since the appearance of the first edition, in a series of essays, several of which appeared from time to time in these columns, tracing

the first appearance of vegetation to aquatic forms. With the change to dry localities, from the vascular Cryptogams seem to have been developed wind-fertilised unisexual flowers—thus first the Gymnosperms, and from these afterwards the Angiosperms have arisen. Finally from the wind-fertilised Angiosperms entomophilous flowers arose; insects came first accidentally and afterwards regularly to seek their food on flowers, and natural selection fostered and perfected every change which favoured insect visits, and thereby aided cross-fertilisation. With the transition to insect-fertilisation came on the one hand great economy of pollen, but on the other hand the uncertainty of insect visits made it as a rule necessary that self-fertilisation should remain possible. Thus, though descended from unisexual (anemophilous) ancestors, entomophilous flowers are usually hermaphrodite, and are capable to a great extent of fertilising themselves when insect visits fail. But in the course of further development many of them have so increased their means of attracting insects (by colour, perfume, honey, &c.) that the power of self-fertilisation has become superfluous, and finally has been lost. Insects, in cross-fertilising flowers, endow them with offspring, which, in the struggle for existence, vanquish those individuals of the same species which are the offspring of self-fertilisation. The insects must therefore operate by selection in the same way as do unscientific cultivators among men, who preserve the most pleasing or most useful specimens, and reject or neglect the others. In both cases selection in course of time brings those variations to perfection which correspond to the taste or to the needs of the selective agent. Different groups of insects, according to their sense of taste or colour, the length of their tongues, their way of movement, and their dexterity, have produced various odours, colours, and forms of flowers, and thus have flowers and insects progressed together towards perfection. All this leads on to the final proposition with which this general retrospect ends, that the forms, colours, and odours of the flowers in a particular region must depend in the closest manner upon the insect fauna of the region, and especially upon the relative abundance of the various classes of insects in it.



Primula integrifolia, L. A.—Short-styled, B.—Long-styled plant (nat. size). C.—Short-styled, D.—Long-styled flower in section (nat. size). E.—Stigmatic papillæ of short-styled flower. F, G.—Ditto of long-styled flower. H.—Stigma of short-styled flower, I.—Ditto of long-styled flower (× 7). K.—Moistened pollen of short-styled flower, L.—Ditto of long-styled flower.

The whole subject of the fertilisation of flowers is one of still unexhausted resources. The student will in this volume have references to what is known, and will find out easily the immense amount of details still waiting investigation. An almost new subject is one that has been lately referred to in our columns by Prof. A. W. Bennett, on the constancy of insects in their visits to flowers; and several new lines of research are pointed out in Mr. Darwin's preface. It is a subject within the reach of all honest, patient observers. It is limited practically to no clime and season. Some of Müller's observations were made on flowers grown in windows, and all were carried on amid the somewhat scant leisure of a busy professional life.

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications.*

[*The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to insure the appearance even of communications containing interesting and novel facts.*]

Iguanodon

SINCE I wrote the account of M. Dollo's researches on Iguanodon, which appeared in NATURE of September 6 (p. 439), I have had the advantage of some conversation with Prof. Marsh

on the subject, and am anxious to state one or two matters which I learned from him concerning questions raised in my review. Prof. Marsh has visited Brussels since I was there, and since M. Dollo's memoirs referred to by me were in print, and has examined the Iguanodon skeletons with M. Dollo, this being the second occasion on which he has seen the collection. After having examined the specimens now available he is of opinion that the question whether the bones considered by M. Dollo to be sternal are in reality such, or clavicles, is still an open one. The form of the bones, which are undoubtedly identical with those in the British Museum specimen determined by Prof. Marsh to be clavicles, is exactly that of clavicles and unlike that of any known sternal bones. There can be no doubt that they belong to the pectoral arch, but the position in which they have been found in two Bernissart specimens points to their belonging rather behind than in front of the coracoids. It is, however, Prof. Marsh believes, just possible that they may have fallen forward into the position in which they there occur, and he awaits the results to be attained from their examination in the other Bernissart specimens before making up his mind. In the closely-allied Hypsilophodon the sternum is a single broad-keeled plate. In the case of the British Museum specimen one of the bones is attached to the scapula. At all events, he points out that, should these bones really prove to be sternal, it does not follow that Iguanodon had no clavicles at all, for there is a process on the scapula indicating the presence of a clavicle, and such a bone, possibly very small and rudimentary, may yet be found to exist.

The statement of Prof. Marsh that the post-pubis in Iguanodon is long and slender, and *incomplete*, is correct; the conclusion that it was not so arose from a misunderstanding of the exact meaning of the term incomplete, as used. It denotes that in Iguanodon the bone does not extend, as it does in some Dinosauria (Hypsilophodon), backwards as far as the ischia, or farther, as in some birds, and this, as will be seen by reference to the figure is the case in Iguanodon, in which the post-pubis does not extend much further back than just beyond the ischial tuberosity. The fact is proved clearly by British specimens as well as by those of Bernissart. Prof. Marsh has observed that in two or three of the Bernissart skulls sutures are distinctly to be seen.

H. N. MOSELEY

Prof. Henrici's Address at Southport

THOUGH a member of many years' standing of the British Association, I have not had the advantage of being present at the current meeting, and am altogether indebted to the report in NATURE for a knowledge of Prof. Henrici's opening address in Section A.

It is much to our advantage to have our educational deficiencies in certain points indicated to us in so candid and, at the same time, so kindly a manner as Prof. Henrici has done on this recent as well as on former occasions; and I hope we shall profit by such friendly criticism. Had I been present, however, I should have ventured to remark on two heads of the address, that I thought Prof. Henrici underrated (1) the extent to which the modern geometry has been cultivated in these countries by many who have not been fettered by the "slavery of examinations" (an expression in which I entirely sympathise), chiefly under the influence of the great geometer Chasles' works; (2) the character of the instruction our youth receive in decimal arithmetic, the abbreviated methods of processes in which being certainly found in our better class of text-books, notably in that of the late Prof. de Morgan, dating back some fifty years, may be assumed to be taught generally in our higher-grade schools, as I certainly know to be the case in several. Other remarks, turning rather on matters of opinion than of fact, which occur to me, would be considered, probably, out of place here.

J. J. WALKER

Scientific Aspects of the Java Catastrophe

YOUR excellent leading article on this great event omits to call attention to a factor which I have long maintained to be of the greatest interest and importance from the point of view of meteorology and geology in general. I allude to the quantity of gases or vapour emitted during the eruptions. This must bear a direct relation to the quantity of matter emitted (whatever its form) and also to the height and distance to which the matter may be ejected or carried.

Now I hold that such vast quantities of gases as must have been liberated on this occasion cannot be passed over or taken as having no action on our atmosphere. Whatever the addition made, temperature and currents are influenced by it either locally or over great extents of the earth's surface, and if it were possible to take account of the height attained by the gases, their temperature of liberation, and the point of the surface of the globe whence proceeding, some judgment might be attempted of their action. In the present state of meteorology we know nothing of these quantities, but it is justifiable to assume that the upper currents of the air may be thus profoundly influenced, and that in certain cases cyclones may thus be generated. The present very fine dry weather we are enjoying here, with the high and steady barometer, may be a result of the great eruption, and it will be worth while to note if any abnormal conditions of atmosphere be found to prevail during the coming months.

J. P. O'REILLY

Dublin, September 16

"Elevation and Subsidence"

MR. YOUNG appears to think that I hold the view that rocky matter will melt at a lower temperature when under greater pressure. I did not intend, in my letter of August 24th, to express such an opinion as my own, but only to say that this was not a settled question; quoting the experiments upon which the doubt was founded.

Again, I merely mentioned the hypothesis that the matter of the nucleus may be above its own critical temperature as "conceivable." To all Mr. Young's present queries I should be disposed to answer in the affirmative, except to the second—"Do not the 'rigidity' calculations incontestably show that the earth is extremely rigid, *i.e.* solid?" As a geologist I do not concern myself anxiously about the nucleus. But to hold that the superficial parts are rigid I assert to be absolutely contrary to the known facts of geology. Perhaps it will be said that they ought to be, and therefore so much the worse for the facts.

Again, I say that mere plasticity of the upper layers will not explain the phenomena. The arrangement of rocks in the interior of mountain chains shows that the crust has been pushed over the surface towards them. It must, therefore, rest on a lubricating substratum. Again, mountains tend to rise and sedimental plains to sink. If mere plasticity were all, the reverse would happen.

As I understand it, the tidal argument for rigidity amounts to this. If the earth were not rigid, the fortnightly tide would be inappreciable. But Prof. Darwin, after most laborious and involved reductions of observations made at the instance of the Indian Government, has come to the conclusion that such a tide can be detected—not of its full amount, however (so far negativing absolute rigidity), but something less than three-quarters of that. The undiminished amount ought to be $4\frac{1}{2}$ inches only. The barriers caused by my "roots of the mountains," which, as noticed by Mr. Gardner, would break up the continuity of the substratum, would, as I have elsewhere pointed out, be great obstacles to the formation of tides in it.

O. FISHER

Harlton, Cambridge, September 20

A Complete Solar Rainbow

ALTHOUGH I quoted Capt. Winchester's figures as to the diameter of the circumsolar bow, mentioned in my letter on p. 436, I may add that this measurement was checked by that of the chief officer (Mr. Grant), who took the distances from the horizon to the inner rim of the bow on both sides, and subtracted them from 180°. In the case of the Captain's measurements, in the first instance, he measured from the inner rim of the bow to the edge of the sun. This was doubled, and the diameter of the sun added to it. Under these circumstances I can hardly believe there could have arisen the mistake suggested by Dr. Ingleby.

September 17 D. MORRIS

C. M. INGLEBY (p. 489) is clearly mistaken in supposing that D. Morris's description in NATURE (p. 436) referred to a real rainbow, for he makes no mention of any rain, the phenomenon being on a thin film over the sky. It must have been a solar halo, differing from an ordinary one only in being more distinctly coloured than usual. I have on rare occasions seen small portions of an ordinary halo very brilliantly coloured, but never saw a complete one so.

T. W. BACKHOUSE

Sunderland, September 24

Animal Intelligence

I AM not aware whether or not the following case has appeared among the numerous instances under this head already given in the columns of NATURE. It is to be found in Vogt and Specht's "Die Säugetiere in Wort und Bild" (p. 11). The writer of the text of that work says:—"I have myself seen a case in which a chimpanzee, who had got himself a little scratched by the point of a slightly projecting nail in the wall of his cage, first carefully examined the same, then sought to remove it, and afterwards, when he was let out, immediately proceeded to search for the head of the nail on the outside of the wall, and then, on finding it, began to try to pull out the nail with his fingers and teeth, and when this was done for him with a pair of pincers, broke out into lively demonstrations of joy."

Camberwell, September 18 GEO. G. CHISHOLM

THE BRITISH ASSOCIATION

SOUTHPORT, *Tuesday*.

CONSIDERING general results, the Southport meeting must be regarded as a decided success. The number of tickets sold has been over 2,650, and the funds will therefore be ample to provide for scientific research. The supply of papers has been kept up in all Sections, and the quality of them has certainly reached a fair average. The weather with two exceptions has been fine, and the accommodation ample. The *soirées* have been all that could be wished, to which the beautiful trees, ferns, and palms in the Winter Gardens have contributed, and the exhibition connected with it afforded points of interest for people of varied tastes. The local officers have worked well and shown both application and forethought, and the excursions, if not of a particularly scientific character, have certainly been the means of the Association receiving much hospitality, and seeing many places of interest, and some of beauty.

The General Committee meeting on Monday was very largely attended, and after the exceedingly well expressed speeches of Principal Dawson and Sir Charles Tupper, the feeling was strong that the meeting in Canada will be a success, and that the greater the number who go the better will the Canadians be pleased. Sir Charles Tupper stated that, after a long experience of the Canadian House of Commons, he never saw a vote so unanimously passed as the appropriation of 4000*l*. for the forthcoming meeting. There are many who still think it a mistaken policy for the Association to leave the shores of these islands, but all of those who were present fully sympathised with the very strong expression of approval that met the announcement after announcement of cheap passages, free railway journeys, and magnificent hospitality offered by the Dominion. General satisfaction was expressed at announcement that those who are unable to spare the time for the long excursions to the Rocky Mountains and elsewhere after the Canada meeting will be allowed to make those expeditions before the meeting, which will commence on August 27, under the presidency of Prof. Lord Rayleigh.

For the 1885 meeting Birmingham and Nottingham did not put in applications, the competing towns being Aberdeen and Bournemouth. In favour of the former it was urged that the members of the Association by that time will be so accustomed to long journeys that they will think nothing of the distance to Aberdeen, and that the Scotch meetings have always been a success, both as to numbers and as to the position of those who attended. In favour of Bournemouth it was urged that first meetings were always a great success, as at Brighton and the present meeting at Southport. The vote was for the northern University town; but there was an expression of feeling that the claims of the watering-place should not be forgotten in 1886.

Prof. Ball's lecture was the most successful of the addresses delivered in the Pavilion. The building, as originally constructed, was oval in shape, with a gallery extending round it, and its acoustic properties were then good; subsequently an ordinary theatrical stage and appointments were added to it, which latter were only partially removed for the meeting, and the building was certainly but ill adapted for the large audiences which endeavoured to find room in it. The Reception Room at the Cambridge Hall left nothing to be desired, except a wish that it had been on the ground floor.

In the Geographical Section much interest was felt in a long paper by Mr. H. H. Johnson, on a visit to Mr. Stanley's stations on the River Congo. The author read a letter he had just received from Mr. Stanley, in which that explorer gives expression to his belief that the River Congo will give civilisation and commerce to the lost Continent. In this Section also an interesting paper was read by Mr. Wm. Hancock of the Chinese Imperial Custom Service, on the volcanic and earthquake regions of Central America; by the Rev. S. J. Perry, on Nos Vey and the south-west of Madagascar, which he visited for the late transit of Venus.

The address of Sir Frederick Bramwell to the working men was a very great success; his good voice and easy style told with effect on the crowded audience of working men who came to learn about the telephone, which was clearly shown to be an important factor in commercial life.

The following is the list of grants of money appropriated by the General Committee to scientific purposes for next year:—

A—Mathematics and Physics

Brown, Prof. Crum—Meteorological Observations on Ben Nevis	£50
Foster, Prof. G. Carey—Electrical Standards	50
Schuster, Prof.—Meteoric Dust	20
Abney, Capt.—Standard of White Light	20
Scott, Mr. R. H.—Synoptic Charts of the Indian Ocean	50
Stewart, Prof. Balfour—Meteorological Observatory near Chepstow	25
Shoolbred, Mr. J. N.—Reduction of Tidal Observations	10
Darwin, Prof. G. H.—Harmonic Analysis of Tidal Observations	45

B—Chemistry

Odling, Prof.—Photographing the Ultra-Violet Spark Spectra	10

C—Geology

Etheridge, Mr. R.—Earthquake Phenomena of Japan	75
Williamson, Prof. W. C.—Fossil Plants of Halifax	15
Sorby, Dr. H. C.—British Fossil Polyzoa	10
Prestwich, Prof.—Erratic Blocks	10
Etheridge, Mr. R.—Fossil Phyllopoda of the Palæozoic Rocks	15
Hull, Prof. E.—Circulation of Underground Waters	15
Evans, Dr. J.—Geological Record	15
Green, Prof. A. H.—Raygill Fissure	15
Prestwich, Prof.—International Geological Map of Europe	20

D—Biology

Newton, Prof.—Zoological Bibliography	50
Sclater, P. L.—Natural History of Timor Laut	50
Lankester, Prof. Ray—Table at the Zoological Station at Naples	80
Harrison, J. Park—Facial Characteristics of Races in the British Isles	10
Hooker, Sir J.—Exploring Kilimandjaro and the adjoining Mountains of Equatorial Africa	500
Cordeaux, Mr. J.—Migration of Birds	20
Foster, Dr. M.—Coagulation of the Blood	50
Stainton, Mr. H. T.—Record of Zoological Literature	100

E—Geography

Godwin-Austen, Lieut.-Col.—Exploration of New Guinea	100

F—Economic Science and Statistics

Brabrook, Mr. E. W.—Preparation of the Final Report of the Anthropometric Committee	10

G—Mechanics

Bramwell, Sir F.—Patent Legislation £5

Total £1445

SECTION D

BIOLOGY

Opening Address by Prof. E. Ray Lankester, M.A., F.R.S., F.L.S., President of the Section.

It has become the custom for the presidents of the various Sections of this Association to open the proceedings of the departments with the chairmanship of which they are charged by formal addresses. In reflecting on the topics which it might be desirable for me to bring under your notice, as your president, on the present occasion, it has occurred to me that I might use this opportunity most fitly by departing somewhat from the prevailing custom of reviewing the progress of science in some special direction during the past year, and that, instead of placing before you a summary of the results recently obtained by the investigations of biologists in this or that line of inquiry, I might ask your attention and that of the external public (who are wont to give some kindly consideration to the opinions expressed on these occasions) to a matter which is even more directly connected with the avowed object of our Association, namely, "the Advancement of Science." I propose to place before you a few observations upon the provision which exists in this country for the advancement of that branch of science to which Section D is dedicated—namely, Biology.

I am aware that it is usual for those who speak of men of science and their pursuits to ignore altogether such sordid topics as the one which I have chosen to bring forward. A certain pride on the one hand, and a willing acquiescence on the other hand, usually prevents those who are professionally concerned with scientific pursuits from exposing to the public the pecuniary destitution and the consequent crippling and languor of scientific research in this country. Those Englishmen who take an interest in the progress of science are apt to suppose that, in some way which they have never clearly understood, the pursuit of scientific truth is not only its own reward, but also a sufficient source of food, drink, and clothing. Whilst they are interested and amused by the remarkable discoveries of scientific men, they are astonished whenever a proposal is mentioned to assign salaries to a few such persons sufficient to enable them to live decently while devoting their time and strength to investigation. The public are becoming more and more anxious to have the opinion or report of scientific men upon matters of commercial importance, or in relation to the public health; and yet in ninety nine cases out of a hundred they expect to have that opinion for the asking, although accustomed to pay other professional men handsomely for similar service. There is, it appears, in the public mind a vague belief that men who occupy their time with the endeavour to add to knowledge in this or that branch of science are mysteriously supported by the State Exchequer, and are thus fair game for attacking with all sorts of demands for gratuitous service; or, on the other hand, the notion at work appears sometimes to be that the making of new knowledge—in fact, scientific discovery—is an agreeable pastime, in which some ingenious gentlemen, whose business in other directions takes up their best hours, find relaxation after dinner or on the spare hours of Sunday. Such mistaken views ought to be dispelled with all possible celerity and determination. It is in part owing to the fact that the real state of the case is not widely and persistently made known to the public, that no attempt is made in this country to raise scientific research, and especially biological research, from the condition of destitution and neglect under which it suffers—a condition which is far below that of these same interests in France and Germany, and even in Holland, Belgium, Italy, and Russia, and is discreditable to England in proportion as she is richer than other States.

It appears to me that, in placing this matter before you, I may remove myself from any suggestion of self-interest by at once stating that the great defect to which I shall draw your attention is *not* that the few existing public positions which are open in this country to men who intend to devote their chief energies to biological research are endowed with insufficient salaries; but that there is not anything like *a sufficiently large number* of those posts, and that there is in that respect, from a national point of view, a pecuniary starvation of biology, a withholding of money which (to use another metaphor) is no less the sinews of the war of science against ignorance than of other less glorious campaigns. Surely men engaged in the scientific profession may advocate the claim of science to maintenance and needful pecuniary provision! It seems to me that we should, if necessary, swallow, rather than be controlled by, that pride which tempts us to paint the scientific career as one far above and independent of pecuniary considerations; whereas all the while we know that knowledge is languishing, that able men are drawn off from scientific research into other careers, that important discoveries are approached and their final grasp relinquished, that great men depart and leave no disciples or successors, simply for want of that which is largely given in other countries, of that which is most abundant in this country, and is so lavishly expended on armies and navies, on the development of commercial resources, on a hundred injurious or meaningless charities—viz., money.

I have no doubt that I have the sympathy of all my hearers in wishing for more extensive provision in this country for the prosecution of scientific research, and especially of biological research. I need hardly remind this audience of the almost romantic history of some of the great discoveries which have been made in reference to the nature and history of living things during the past century. The microscope, which was a drawing-room toy a hundred years ago, has, in the hands of devoted and gifted students of nature, been the means of giving us knowledge which, on the one hand, has saved thousands of surgical patients from terrible pain and death, and, on the other hand, has laid the foundation of that new philosophy with which the name of Darwin will for ever be associated. When Ehrenberg and, later, Dujardin described and figured the various forms of Monas, Vibrio, Spirillum, and Bacterium which their microscopes revealed to them, no one could predict that fifty years later these organisms would be recognised as the cause of that dangerous suppuration of wounds which so often defeated the beneficent efforts of the surgeon and made an operation in a hospital ward as dangerous to the patient as residence in a plague-stricken city. Yet this is the result which the assiduous studies of the biologists, provided with laboratories and maintenance by Continental States, have in due time brought to light. Theodore Schwann, professor at Liége, first showed that these Bacteria are the cause of the putrefaction of organic substances, and subsequently the French chemist Pasteur, professor in the École Normale of Paris, confirmed and extended Schwann's discovery, so as to establish the belief that all putrefactive changes are due to such minute organisms, and that if these organisms can be kept at bay no putrefaction can occur in any given substance.

It was reserved for our countryman, Joseph Lister, to apply this result to the treatment of wounds, and by his famous antiseptic method to destroy by means of special poisons the putrefactive organisms which necessarily find their way into the neighbourhood of a wound, or of the surgeon's knife and dressings, and to ward off by similar means the access of such organisms to the wounded surface. The amount of death, not to speak of the suffering short of death, which the knowledge of Bacteria gained by the microscope has thus averted is incalculable.

Yet further, the discoveries of Ehrenberg, Schwann, and Pasteur are bearing fruit of a similar kind in other directions. It seems in the highest degree probable that the terrible scourge known as tubercular consumption or phthisis is due to a parasitic Bacterium (Bacillus), discovered two years since by Koch of Berlin, as the immediate result of investigations which he was commissioned to carry on at the public expense, in the specially erected Laboratory of Public Health, by the German Imperial Government. The diseases known as erysipelas and glanders or farcy have similarly, within the past few months in German State-supported laboratories, been shown to be due to the attacks of special kinds of Bacteria. At present this knowledge has not led to a successful method of combating those diseases, but we can hardly doubt that it will ultimately do so. We are warranted in this belief by the fact that the disease known as "splenic fever" in cattle and "malignant pustule" or anthrax in man has likewise been shown to be due to the action of a special kind of Bacterium, and that this knowledge has, in the hands of MM. Toussaint and Pasteur, led to a treatment in relation to this disease similar to that of vaccination in relation to small-pox. By cultivation a modified growth of the anthrax parasite is obtained, which is then used in order to inoculate cattle and sheep with a mild form of the disease, such inoculation having the result of rendering the cattle and sheep free from the attacks of the severe form of disease, just as vaccination or inoculation with cow-pox protects man from the attack of the deadly small-pox. One other case I may call to mind in which knowledge

of the presence of Bacteria as the cause of disease has led to successful curative treatment. A not uncommon affliction is inflammation of the bladder accompanied by ammoniacal decomposition of the urine. Microscopical investigation has shown that this ammoniacal decomposition is entirely due to the activity of a Bacterium. Fortunately this Bacterium is at once killed by weak solutions of quinine, which can be injected into the bladder without causing any injury or irritation. This example appears to have great importance, because it is the fact that many kinds of Bacteria are not killed by solutions of quinine, but require other and much more irritant poisons to destroy their life, which could not be injected into the bladder without causing disastrous effects. Since some Bacteria are killed by one poison and some by another, it becomes a matter of the keenest interest to find out all such poisons; and possibly among them may be some which can be applied so as to kill the Bacteria which produce phthisis, erysipelas, glanders, anthrax, and other scourges of humanity, whilst not acting injuriously upon the body of the victim in which these infinitesimal parasites are doing their deadly work. In such ways as this biology has turned the toy "magnifying-glass" of the last century into a saver of life and health.

No less has the same agency revolutionised the thoughts of men in every branch of philosophy and speculation. The knowledge of the growth of the chick from the egg and of other organisms from similarly constituted beginnings has been slowly and continuously gained by prodigious labour, extending over generation after generation of students who have occupied the laboratories and lived on the stipends provided by the Governments of European States—not English, but chiefly German. It is this history of the development of the individual animal and plant from a simple homogeneous beginning to a complex heterogeneous adult which has furnished the starting-point for the wide-reaching Doctrine of Evolution. It is this knowledge, coupled with the knowledge of the myriad details of structure of all kinds of animals and plants which the faithful occupants of laboratories and the guardians of biological collections have in the past hundred years laboriously searched out and recorded—it is this which enabled Darwin to propound, to test, and to firmly establish his theory of the origin of species by natural selection, and finally to bring the origin, development, and progress of man also into the area of physical science. I have said enough, in referring only to two very diverse examples of the far-reaching consequences flowing from the discoveries of single-minded investigators in biological science, to remind my hearers that in the domain of biology, as in other sciences, the results attained by those who have laboured simply to extend our knowledge of the structure and properties of living things, in the faith that every increase of knowledge will ultimately bring its blessing to humanity, have in fact led with astonishing rapidity to conclusions affecting most profoundly both the bodily and the mental welfare of the community.

We who know the beneficent results which must flow more and more from the labours of those who are able to create new knowledge of living things, or, in other words, are able to aid in the growth of biological science, must feel something more than regret—even indignation—that England should do so small a proportion of the laborious investigation which is necessary, and is being carried on for our profit by other nationalities. It must not be supposed, because we have had our Harvey and our Darwin, our Hunter and our Lister, that therefore we have done and are doing all that is needful in the increase of biological science. The position of this country in relation to the progress of science is not to be decided by the citation of great names.

We require to look more fully into the matter than this. The question is not whether England has produced some great discoverers, or as many as any other nationality, but whether we might not, with advantage to our own community and that of the civilised world generally, do far more in the field of scientific investigation than we do.

It may be laid down as a general proposition, to which I know of no important exception, that scientific discovery has only been made by one of two classes of men, namely—(1) those whose time could be devoted to it in virtue of their possessing inherited fortunes; (2) those whose time could be devoted to it in virtue of their possessing a stipend or endowment especially assigned to them for that purpose.

Now it is a very remarkable fact that in England, far more than in any other country, the possessors of private fortunes have devoted themselves to scientific investigation. Not only have we in all parts of the country numerous *dilettanti*[1] who, especially in various branches of biology, do valuable work in continually adding to knowledge, quietly pursuing their favourite study without seeking to reach to any great eminence, but it is the fact that many of the greatest names of English discoverers in science are those of men who held no professional position designed to maintain an investigator, but owed their opportunity simply to the fact that they enjoyed a more or less ample income by inheritance. Thus, Harvey possessed a private fortune, Darwin also, and Lyell. Such also is true of some of the English naturalists, who more recently have most successfully devoted their energies to research. Those who wish to defend the present neglect of the Government and of public institutions to provide means for the carrying on of scientific research in this country are accustomed to declare as a justification for this neglect that we do very well without such provision, inasmuch as the cultivation of science here flourishes in the hands of those who are in a position of pecuniary independence. The reply to this is obvious. If those few of our countrymen who by accident are placed in an independent position show such ability in the prosecution of scientific research, how much more would be effected in the same direction were the machinery provided to enable those also who are *not* accidentally favoured by fortune to enter upon the same kind of work? The number of wealthy men who have distinguished themselves in scientific research in England is simply evidence that there is a natural ability and liking for such work in the English character, and is a distinct encouragement to those who have it in their power to do so to offer the opportunity of devoting themselves to research to a larger number of the members of the community. It is impossible to doubt that there are hundreds of men amongst us who have as great capacity for scientific discovery as those whom fortune has favoured with leisure and opportunity. It cannot be doubted that, were the means provided to enable even a proportion of such men to give themselves up to scientific investigation, great discoveries of no less importance to the world than those relative to the causes of disease and the development of living things from the egg—which I have cited—would be made as a direct consequence of their activity, whereas now we must wait until in due course of time these discoveries shall be made for us in the laboratories of Germany, France, or Russia.

It should further be pointed out that it is altogether a mistake to suppose that the existence amongst us of a few very eminent men is any evidence that we are contributing largely to the hard work of careful study and observation which really forms the material upon which the conclusions of eminent discoverers are based. You will find in every department of biological knowledge that the hard work of investigation is being carried on by the well-trained army of German observers. Whether you ask the zoologist, the botanist, the physiologist, or the anthropologist, you will get the same answer: it is to German sources that he looks for new information; it is in German workshops that discoveries, each small in itself, but gradually leading up to great conclusions, are daily being made. To a very large extent the business of those who are occupied with teaching or applying biological science in this country consists in making known what has been done in German laboratories; our English students flock to Germany to learn the methods of scientific research; and to such a state of weakness is English science reduced for want of proper nurture and support, that even on some of the rare occasions when a capable investigator of biological problems has been required for the public service, it has been necessary to obtain the assistance of a foreigner trained in the laboratories of Germany.

Let me now briefly explain what are the arrangements, in number and in kind, which exist in other countries for the purpose of promoting the advancement of biological science, which are wanting in this country.

In the German Empire, with a population of 45,000,000, there are twenty-one universities. These universities are very different from anything which goes by the name in this country. Amongst its other arrangements devoted to the study and teaching of all branches of learning and science, each university has five institutes, or establishments, devoted to the prosecution of researches in biological science. These are respectively the physiological, the zoological, the anatomical, the pathological,

[1] I use this word in its best and truest sense, and would refer those who have been accustomed to associate with it some implication of contempt, to the wise and appreciative remarks of Goethe on Dilettanti.

and the botanical. In one of these universities of average size, each of the institutes named consists of a spacious building containing many rooms fitted as workshops, provided with instruments, a museum, and, in the last instance, with an experimental garden. All this is provided and maintained by the State. At the head of each institute is the university professor respectively of physiology, of zoology, of anatomy, of pathology, or of botany. He is paid a stipend by the State, which in the smallest university is as low as 120*l.*, but may be in others as much as 700*l.*, and averages say 400*l.* a year. Considering the relative expenditure of the professional classes in the two countries, this average may be taken as equal to 800*l.* a year in England.[1] Besides the professor, each institute has attached to it, with salaries paid by the State, two qualified assistants, who in course of time will succeed to independent positions. A liberal allowance is also made to each institute by the State for the purchase of instruments, material for study, and for the pay of servants, so that the total expenditure on professor, assistants, laboratory service, and maintenance, averages 800*l.* a year for each institute—reaching as much as 2000*l.* or 3000*l.* a year in the larger universities. It is the business of the professor, in conjunction with his assistants and the advanced students, who are admitted to work in the laboratories free of charge, to carry on investigations, *to create new knowledge* in the several domains of physiology, zoology, anatomy, pathology, and botany. It is for this that the professor receives his stipend, and it is on his success in this field of labour that his promotion to a more important or better paid post in another university depends. In addition to and irrespectively of this part of his duties, each professor is charged with the delivery of courses of lectures and of elementary instruction to the general students of the university, and for this he is allowed to charge a certain fee to each student, which he receives himself, the total of such fees may, in the case of a largely attended university and a popular subject, form a very important addition to the professorial income; but it is distinctly to be understood that such payment by fees is only an *addition* to the professor's income, quite independent of his stipend and of his regular occupation in the laboratory: it is paid from a separate source and for a separate object. There are thus in the German Empire more than 100 such institutes devoted to the prosecution of biological discovery, carried on at an annual cost to the State of about 80,000*l.*, equal to about 160,000*l.* in England, providing posts of graduated value for 300 investigators, some of small value, sufficient to carry the young student through the earlier portion of his career, whilst he is being trained and acting as the assistant of more experienced men—others forming the sufficient but not too valuable prizes which are the rewards of continuous and successful labour.

In addition to these university institutes, there are in Germany such special laboratories of research, with duly salaried staff of investigators, as the Imperial Sanitary Institute of Berlin, and the large museums of Berlin, Bremen, and other large towns, corresponding to our own British Museum of Natural History.

Moreover, we must be careful to note, in making any comparison with the arrangements existing in England, that there are, in addition to the universities in Germany, a number of other educational institutions, at least equal in number, which are known as polytechnic schools, technical colleges, and agricultural colleges. These furnish posts of emolument to a limited number of biological students, who give courses of instruction to their pupils, but they have not the same arrangements for research as the universities, and are closely similar to those colleges which have been founded of late years in the provincial towns of England, such as Bristol, Nottingham, and Leeds. The latter are sometimes quoted by sanguine persons, who are satisfied with the neglected condition of scientific training and research in this country, as really sufficient and adequate representatives of the German universities. As a matter of fact, the excellent English colleges in question do not present anything at all comparable to the arrangements of a German university, and are, in respect of the amount of money which is expended upon them, the number of their teaching staff and the efficiency of their laboratories, inferior not merely to the smallest German university, but inferior to many of the technical schools of that country.

Passing from Germany, I would now ask your attention for a moment to an institution which is supported by the French Government, and which—quite irrespective of the French university system, which is not on the whole superior to our own—constitutes one of the most effective arrangements in any European State for the production of new knowledge. The institution to which I allude is the Collège de France in Paris—co-existing there with the Sorbonne, the École de Médecine, the École Normale, the Jardin des Plantes, and other State-supported institutions—in which opportunity is provided for those Frenchmen who have the requisite talent to pursue scientific discovery in the department of biology, and in other branches of science. I particularly mention the Collège de France, because it appears to me that the foundation of such a college in London would be one of the simplest and most direct steps that could be taken towards filling, in some degree, the void from which English science suffers. The Collège de France is divided into a literary and a scientific faculty. Each faculty consists of some twenty professors. Each professor in the scientific faculty is provided with a laboratory and assistants (as many as four assistants in some cases), and with a considerable allowance for the expenses of the instruments and materials required in research. The personal stipend of each professor is £400, which has been increased by an additional £100 a year in some cases from the Government Department charged with the promotion of higher studies. The professors in this institution, as in the German universities, when a vacancy occurs, have the right of nominating their future colleague, their recommendation being accepted by the Government. The professors are not expected to give any elementary instruction, but are directed to carry on original investigations, in prosecuting which they may associate with themselves pupils who are sufficiently advanced to join in such work; and it is further the duty of each professor to give a course of forty lectures in each year upon the results of the researches in which he is engaged. There are at present among the professors of the Collège de France four of the most distinguished among contemporary students of biological science: Professor Brown-Séquard, Professor Marey, Professor Balbiani, and Professor Ranvier. Every one who is acquainted with the progress of discovery in physiology, minute anatomy, and embryology, will admit that the opportunities afforded to these men have not been wasted; they have, as the result of the position in which they have been placed, produced abundant and most valuable work, and have, in addition, trained younger men to carry on the same line of activity. It was here, too, in the Collège de France, that the great genius of Claude Bernard found the necessary conditions for its development.

Let us now see how many and what kind of institutions there are in England devised so as to promote the making of new knowledge in biological science. Most persons are apt to be deceived in this matter by the fact that the terms "university," "professorship," and "college" are used very freely in England in reference to institutions which have no pecuniary resources whatever, and which, instead of corresponding to the German arrangements which go by these names, are empty titles, neither backed by adequate subsidy of the State nor by endowment from private sources.

In England, with its 25,000,000 inhabitants, there are only four universities which possess endowments and professoriates—viz., Oxford, Cambridge, Durham, and the Victoria (Owens College). Besides these, which are variously and specially organised each in its own way, there are the London Colleges (University and King's), the Normal School of Science at South Kensington, and various provincial colleges, which are to a small and varying extent in possession of funds which could be or are used to promote scientific research. Amongst all these variously arranged institutions there is an extraordinarily small amount of provision for biological research. In London there is one professorship only, that at the Normal School of Science, which is maintained by a stipend paid by the State, and has a laboratory and salaried assistants, similarly maintained, in connection with it. The only other posts in London which are provided with stipends intended to enable their holders to pursue researches in the domain of biological science, are the two chairs of physiology and of zoology at University College, which, through the munificence of a private individual (Mr. Jodrell), have been endowed to the extent of 300*l.* a year each. To these should be added, in our calculation, certain posts in connection with the British Museum of Natural History and the Royal Gardens at Kew, maintained by the State; though it must be remembered that a large part of the expenditure in those institutions is necessarily taken up in the preservation of great national collections, and is not applicable to the subvention of investigators. We may, however, reckon about six posts, great and small, in the British

[1] From the fact that the salaries of judges, civil servants, military and naval officers, parsons and schoolmasters, as also the fees of physicians and lawyers, are in Germany even less than half what is paid to their representatives in England, I think that we are justified in making this estimate.

Museum, and four at Kew, as coming into the category which we have in view. In London, then, we may reckon approximately some fourteen or fifteen subsidised posts for biological research In Oxford there fall under this category the professorship of anatomy and his assistant, that of physiology, that of zoology, that of botany. The Oxford professorships are well supported by endowment, averaging 700*l.* or 800*l.* a year; but they are inadequately provided with assistance as compared with corresponding German positions. Whilst Oxford has thus five posts, Cambridge has at present the same number, though the stipends are of less average value. In regard to Durham, it does not appear that the biological professorships (which have their seat in the Newcastle College of Science) are supported by stipends derived from endowment: they fall under another category, to which allusion will be made below, of purely teaching positions, supported by the fees paid for such teaching by pupils. The Victoria University (Owens College, Manchester), supports its professors of physiology, anatomy, zoology, botany, and pathology, by means partly of endowment, partly of pupils' fees. By the provision of adequate laboratories and of salaries for assistants to each professor, and of student-fellowships, Owens College gives direct support to original investigation. We may reckon five major and eight minor posts as dedicated to biological research in this college. Altogether, then, we have 15 positions in London and 23 in the provinces (taking assistantships, and professorships, and curatorships together)—a total of 38 in all England with its 25,000,000 inhabitants, as against the 300 in Germany with its 45,000,000 inhabitants. In proportion to its population (leaving aside the consideration of its greater wealth), England has only about one-fourth of the provision for the advancement of biological research which exists in Germany.

It would not be fair to reckon in this comparison the various biological professorships in small colleges recently created, and paid to a small extent by stipends derived from endowments, in the provincial towns of England, for the holders of these chairs are called upon to teach a variety of subjects, for instance, zoology, botany, and geology combined; and not only is the devotion of the energies of their teaching staff to scientific discovery not contemplated in the arrangement of these institutions, but, as a matter of fact, the large demands made on the professors in the way of teaching must deprive them of the time necessary for any serious investigation. Such posts, in the fact that neither time, assistants, nor proper laboratories are provided to enable their holders to engage in scientific research, are schoolmasterships rather than professorships, as the word is used in German universities.

One result of the exceedingly small provision of positions in England similar to those furnished by the German university system, and of the irregular, uncertain character of many of those which do exist, is that there is an insufficient supply of young men willing to enter upon the career of zoologist, botanist, physiologist, or pathologist as a profession. The number of posts is too small to create a profession, *i.e.* an avenue of success; and consequently, whereas in Germany there is always a large body of new men ready to fill up the vacancies as they occur in the professorial organisation, in England it very naturally does not appear to our university students as a reasonable thing to enter upon research as a profession, when the chances of employment are so few and far between.

Before stating, as I propose to do, what appears to me a reasonable and proper method of removing to some extent the defect in our national life due to the want of provision for scientific research, I will endeavour to meet some of the objections which are usually raised to such views as those which I am advocating. The endowment of research by the State, or from public funds of any kind, is opposed on various grounds. One is that such action on the part of the Government is well enough in Continental States, but is contrary to the spirit of English statecraft, which leaves scientific as well as other *enterprise* to the individual initiative of the people. This objection is based on error, both as to fact and theory. It is well enough to leave to individual effort the conduct of such enterprises as are remunerative to the parties who conduct them; but it is a mistake to speak of scientific research as an "enterprise" at all. The mistake arises from the extraordinary pertinacity with which so-called "invention" is confounded with the discovery of scientific truth. New knowledge in biological or other branches of science cannot be sold; it has no marketable value. Koch could not have sold the discovery of the Bacterium of phthisis for as much as sixpence, had he wished to do so. Accordingly, we find that there is not, and never has been, any tendency among the citizens of this country to provide for themselves institutions for the manufacture of an article of so little pecuniary value to the individual who turns it out as is new knowledge. On the other hand, as a matter of fact, the providing of means for the manufacture of that article is not only not foreign to English statecraft, but is largely, though not largely enough, undertaken by the English State. The Royal Observatories, the British Museum, the Royal Gardens at Kew, the Geological Survey, the Government grant of 4,000*l.* a year to the Royal Society, the 300*l.* or 400*l.* a year (not a large sum) expended through the medical officer of the Privy Council upon the experimental investigation of disease, are ample evidence that such providing of means for creating new knowledge forms part of the natural and recognised responsibilities of the British Government. Such a responsibility clearly is recognised in this country, and does fall, according to the present arrangement of things, upon the central Government. What we have to regret is, that those who temporarily hold the reins of government fail to perceive the lamentable inadequacy of the mode in which this responsibility is met.

A second objection which is made to the endowment of research by public funds, or by other means, such as voluntary contributions, is this: it is stated that men engaged in scientific research ought to *teach*, and thus gain their livelihood. It is argued, in fact, that there is no need whatever to provide stipends or laboratories for researchers, since they have only to stand up and teach in order to make incomes sufficient to keep them and their families, and to provide themselves with laboratories. This is a very plausible statement, because it is the fact that some investigators have also been excellent lecturers, and have been able to make an income by teaching whilst carrying on a limited amount of scientific investigation. But neither by teaching in the form of popular lectures, nor by teaching university or professional students who desire as a result to pass some examination test, is it possible, where there is a fair field and no favour, for a man to gain a reasonable income and at the same time to leave himself time and energy to carry on original investigations in science.

In some universities, such as those of Scotland, the privilege of conferring degrees of pecuniary value to their possessors becomes a source of income to the professors of the university; they are, in fact, able to make considerable incomes, independently of endowment, by compelling the candidates for degrees to pay a fee to each professor in the faculty for the right of attending his lectures and of presentation to the degree. Consequently, teaching here appears to be producing an income which may support a researcher; in reality, it is the acquisition of the university degree, and not necessarily the teaching, for which the pupil pays his fee. Where the teacher is unprotected by any compulsory regulations (such as that which requires attendance on his lectures and fee-payment on the part of the pupils) it is *impossible* for him to obtain such an income by teaching for one hour a day as will enable him to devote the rest of the day to unremunerative study and investigation, for the following reason. Other teachers, equally satisfactory as teachers, will enter into competition with him, without having the same intention of teaching for one hour only, and of carrying on researches for the rest of the day. They will contemplate teaching for six hours a day, and they will accordingly offer to those who require to be taught either six hours' teaching for the same fee which the researcher charges for one, or one hour for a sixth part of that fee. Consequently the unprotected researcher will find his lecture-room deserted—pupils will naturally go to the equally good teacher who gives more teaching for the same fee, or the same teaching for a less cost. And no one can say that this is not as it should be. The university pupil requires a certain course of instruction, which he ought to be able to buy at the cheapest rate. It does not seem to be doing justice to the pupil to compel him to form one of a class consisting of some hundreds of hearers, where he can obtain but little personal supervision or attention from the teacher, whereas if he had the free disposal of his fee, he might obtain six times the amount of attention from another teacher. This arrangement does not seem to be justifiable, even for the purpose of providing the university professor with an income and leisure to pursue scientific research. The student's fee should pay for a given amount of teaching at the market value, and he has just cause of complaint if, by compulsory enactments, he is taxed to provide the country with scientific investigation.

Teaching must, in all fairness, ultimately be paid for as

teaching, and scientific research must be provided for out of other funds than those extracted from the pockets of needy students, who have a reasonable right to demand, in return for their fees, a full modicum of instruction and direction in study.

In the German universities, the professor receives a stipend which provides for him as an investigator. He also gives lectures, for which he charges a fee, but no student is compelled to attend those lectures as a condition of obtaining his degree. Accordingly, independent teachers can, and do, compete with the professor in providing for the students' requirements in the matter of instruction. As a consequence, the fees charged for teaching are exceeding small, and the student can feel assured that he is obtaining his money's worth for his money. He is not compelled to pay any fee to any teacher as a condition of his promotion to the university degree. In a German university, if the professor in a given subject is incompetent, or the class overcrowded, the student can take his fee to a private teacher, and get better teaching; all that is required of the candidate, as a condition of his promotion to the Doctor's degree, is that he shall satisfy the examination-tests imposed by the faculty, and produce an original thesis.

Unless there be some such compelling influence as that obtaining in the Scotch universities, enabling the would-be researcher to gather to him pupils and fees without fear of competition, it seems impossible that he should gain an income by teaching whilst reserving to himself time and energy for the pursuit of scientific inquiry. It is thus seen that the necessity of endowment, in some form or another, to make provision for scientific research, is a reality, in spite of the suggestion that teaching affords a means whereby the researcher may readily provide for himself. The simple fact is that a teacher can only make a sufficient income by teaching, on the condition that he devotes his whole time and energy to that occupation.

Whilst I feel called upon to emphatically distinguish the two functions—viz., that of *creating new knowledge*, and that of *distributing existing knowledge*—and to maintain that it is only by arbitrary and undesirable arrangements, not likely to be tolerated, or, at any rate, extended, at the present day, that the latter can be made to serve as the support of the former, I must be careful to point out that I agree most cordially with those who hold that it is an excellent thing for a man who is engaged in the one to give a certain amount of time to the other. It is a matter of experience that the best teachers of a subject are, *cæte is paribus*, those who are actually engaged in the advancement of that subject, and who have shown such a thorough understanding of that subject as is necessary for making new knowledge in connection with it. It is also, in most cases, a good thing for the man engaged in research to have a certain small amount of change of occupation, and to be called upon to take such a survey of the subject in connection with which his researches are made, as is involved in the delivery of a course of lectures and other details of teaching Though it is not a thing to be contemplated that the researcher shall sell his instruction at a price sufficiently high to enable him to live by teaching, yet it is a good thing to make teaching an additional and subsidiary part of his life's work. This end is effected in Germany by making it a duty of the professor, already supported by a stipend, to give some five or six lectures a week during the academical session, for which he is paid by the fees of his hearers. The fees are low, but are sufficient to be an inducement; and, inasmuch as the attendance of the students is not compulsory, the professor is stimulated to produce good and effective lectures at a reasonable charge, so as to attract pupils who would seek instruction from some one else if the lectures were not good or the fees too high. Indeed, in Germany this system works so much to the advantage of the students, that the private teachers of the universities at one time obtained the creation of a regulation forbidding the professors to reduce their fees below a certain minimum, since, with so low a fee as some professors were charging, it was impossible for a private teacher to compete! This state of things may be compared, with much advantage, with the condition of British universities. In these we hear, from one direction, complaints of the high fees charged and of the ineffective teaching given by the professoriate; and in other universities, where no adequate fees are allowed to the professors as a stimulus to them to offer useful and efficient teaching, we find that the teaching has passed entirely out of their hands into those of college tutors and lecturers. The fact is that a satisfactory relation between teaching and research is one which will not naturally and spontaneously arrange itself. It can hardly be said to exist in any British university or college, but the method has been thought out and carried into practice in Germany. It consists in giving a competent researcher a stipend and a laboratory for his research work, and then requiring him to do a small amount of teaching, remunerated by fees proportionate to his ability and the pains which he may take in his teaching. If you pay him a fixed sum as a teacher, or artifically insure the attendance of his class, instead of letting this part of his income vary simply and directly with the attractiveness of his teaching, you will find as the result that (with rare exceptions) he will not give effective and useful teaching. He will naturally tend to do the minimum required of him, in a perfunctory way. On the other hand, if you leave him without stipend as a researcher, dependent on the fees of pupils for an income, he will give all his time and energies to teaching, he will cease to do any research, and become, *pro tanto*, an inferior teacher.

A third objection which is sometimes made to the proposition that scientific research must be supported and paid for as such, is the following: It is believed by many persons that a man who occupies his best energies in scientific research can always, if he choose, make an income by writing popular books or newspaper articles in his spare hours; and, accordingly, it is gravely maintained that there is no need to provide stipends and the means of carrying on their work for researchers. To do so, according to this view, would be to encourage them in an exclusive reticence, and to remove from them the inducement to address the public on the subject of their researches, by which the public would lose valuable instruction.

This view has been seriously urged, or I should not here notice it Any one who is acquainted with the sale of scientific books, and the profits which either author or publisher makes by them, knows that the suggestion which I have quoted is ludicrous. The writing of a good book is not a thing to be done in leisure moments, and such as have been the result of original research have cost their authors often years of labour apart from the mere writing. Mr. Darwin's books, no doubt, have had a large sale; but that is due to the fact, apart from the exceptional genius of the man who wrote them, that they represent some thirty or more years of hard work, during which he was silent. There is not a sufficiently large public interested in the progress of science to enable a researcher to gain an income by writing books, however great his literary facility. A school-book or class-book may now and then add more or less to the income of a scientific investigator; but he who becomes the popular exponent of scientific ideas, except in a very moderate and limited degree, must abandon the work of creating new knowledge. The professional *littérateur* of science is as much removed by his occupation from all opportunity of serious investigation as is the professional teacher who has to consume all his time in teaching. Any other profession—such as the Bar, Medicine, or the Church—is more likely to leave one of its followers time and means for scientific research than is that of either the popular writer or the successful teacher.

We have, then, seen that there is no escape from the necessity of providing stipends and laboratories for the purpose of creating new knowledge, as is done in continental States, if we are agreed that more of this new knowledge is needed and is among the products which a civilised community is bound to turn out, both for its own benefit and for that of the community of States, which give to and take from one another in such matters.

There are some who would finally attack our contention by denying that new knowledge is a good thing, and by refusing to recognise any obligation on the part of England to contribute her share to that common stock of increasing knowledge by which she necessarily profits. Among such persons are those who would prohibit altogether the pursuit of experimental physiology in England, and yet would not and do not hesitate to avail themselves of the services of medical men, whose power of rendering those services depends on the fact that they have learnt the results obtained by the experiments of physiologists in other countries or in former times. In reference to this strange contempt and even hatred of science, which undoubtedly has an existence among some persons of consideration, even at the present day, I shall have a few words to say before concluding this address. I have now to ask you to listen to what seems to me to be the demand which we should make, as members of a British Association for the Advancement of Science, in respect of adequate provision for the creation of new knowledge in the field of biology in England.

Taking England alone, as distinct from Scotland and Ireland, we require in order to be approximately on a level with Germany, forty new biological institutes, distributed among the five

branches of physiology, zoology, anatomy, pathology, and botany—forty in addition to the fifteen which we may reckon (taking one place with another) as already existing. The average cost of the buildings required would be about 4,000*l.* for each, giving a total initial expenditure of 160,000*l.*; the average cost of stipends for the director, assistants, and maintenance we may calculate at 1,500*l.* annually for each, or 60,000*l.* for the forty—equal to a capital sum of 2,000,000*l.* These institutes should be distributed in groups of five—eight groups in all—throughout the country. One such group would be placed in London (which is, at present, almost totally destitute of such arrangements), one in Bristol, one in Birmingham, one in Nottingham, one in Leeds, one in Newcastle, one in Ipswich, one in Cardiff, one in Plymouth—in fact, one in each of the great towns of the kingdom where there is at present, or where there might be with advantage, a centre of professional education and higher study. The first and the most liberally arranged of these biological institutes—embracing its five branches, each with its special laboratory and staff—should be in London. If we can have nothing else, surely we may demand, with some hope that our request will eventually obtain compliance, the formation in London of a College of Scientific Research similar to that of Paris (the Collège de France). It is one of the misfortunes and disgraces of London that—alone amongst the capitals of Europe, with the exception of Constantinople—it is destitute of any institution corresponding to the universities and colleges of research which exist elsewhere.

Either in connection with a properly organised teaching university or as an independent institution, it seems to me a primary need of the day that the Government should establish in London laboratories for scientific research. Two hundred and fifty years ago Sir Thomas Gresham founded an institution for scientific research in the City of London. The property which he left for this purpose is now estimated to be worth three millions sterling. This property was deliberately appropriated to other uses by the Corporation of the City of London and the Mercers' Company about a hundred years since, with the consent of both Houses of Parliament. By this outrageous act of spoliation these Corporations, who were the trustees of Gresham, have incurred the curse which he quaintly inserted in his will in the hope of restraining them from attempts to divert his property from the uses to which he destined it. "Gresham's curse" runs as follows:—"And that I do require and charge the said Corporations and chief governors thereof, with circumspect Diligence and without long Delay, to procure and see to be done and obtained, as they will answer the same before Almighty God; (for if they or any of them should neglect the obtaining of such Licenses or Warrants, which I trust can not be difficult, nor so chargeable, but that the overplus of my Rents and Profits of the Premisses hereinbefore to them disposed, will soon recompense the same; because to soe good Purpose in the Commonwealth, no Prince nor Council in any Age, will deny or defeat the same. And if conveniently by my Will or other Convenience, I might assure it, I would not leave it to be done after my death, then the same shall revert to my heirs, whereas I do mean the same to the Commonwealth, and then THE DEFAULT THEREOF SHALL BE TO THE REPROACH AND CONDEMNATION OF THE SAID CORPORATIONS AFORE GOD)." I confess that I find it difficult to see how the present representatives of the Corporations who perverted Gresham's trust are to escape from justly deserving the curse pronounced against those Corporations, unless they conscientiously take steps to restore Gresham's money to its proper uses. Let us hope that Gresham's curse may be realised in no more deadly form than that of an Act of Parliament repealing the former one which sanctioned the perversion of Gresham's money. Such a sequel to the Report of the Commission which has recently inquired into the proceedings of the Corporation and Companies of the City of London is not unlikely.

Whilst we should, I think, especially press upon public attention the need for an institute of scientific research in London, and indicate the source from which its funds may be fitly derived, we must also urge the foundation of other institutes in the provinces upon the scale already sketched, because it is only by the existence of numerous posts, and of a series of such posts—some of greater and some of less value, the latter more numerous than the former—that anything like a professional career for scientific workers can be constructed. It is especially necessary to constitute what I have termed "assistantships," that is, junior posts in which younger men assist and are trained by more experienced men. Even in the few institutions which do already exist additional provision of this kind is what is wanted more than anything else, so that there may be a progressive career open to the young student, and a sufficient field of trained investigators from which to select in filling up the vacancies in more valuable positions.

I am well aware that it will be said that the scheme which I have proposed to you is gigantic and almost alarming in respect of the amount of money which it demands. One hundred and sixty thousand pounds a year for biology alone must seem, not to my hearers, but to those who regard biology as an amusing speculation—that is to say, who know little or nothing about it—an extravagant suggestion. Unfortunately it is also true that such persons are very numerous—in fact, constitute an overwhelming majority of the community; but they are becoming less numerous every day. The time will come, it seems possible, when there will be more than one member of the Government who will understand and appreciate the value of scientific research. There are already a few members of the House of Commons who are fully alive to its significance and importance.

We may have to wait for the expenditure of such a sum as I have named, and possibly it may be derived ultimately from local rather than imperial sources, though I do not see why it should be; yet I think it is a good thing to realise *now* that this is what we ought to expend in order to be on a level with Germany. This apparently extravagant and unheard of appropriation of public money is *actually made every year in Germany.*

I think it is well to put the matter before you in this definite manner, because I have reason to believe that even those whom we might expect to be well informed in regard to such matters, are not so, and as a consequence there is not that keen sense of the inferiority and inadequacy of English arrangements in these matters which one would gladly see actuating the conduct of English statesmen. For instance, only a few years ago, when speaking at Nottingham, the present Prime Minister, who has taken an active part in rearranging our universities, and has, it is well known, much interest in science and learning, stated that 27,000*l.*, the capital sum expended on the Nottingham College of Science, was a very important contribution to the support of learning in this country, amounting, as he said he was able to state, from the perusal of official documents, to as much as one-third of what was spent in Germany during the past year upon her numerous universities, which were so often held up to England as an example of a well supported academical system. Now, I do not think that Mr. Gladstone can have ever had the opportunity of considering the actual facts with regard to German universities, for he was in this instance misled by the official return of expenditure on a single university, namely, that of Strasburg; the total annual expenditure on the twenty-one German universities being, in reality, about 800,000*l.*, by the side of which a capital sum of 27,000*l.* looks very small indeed. I cannot but believe that if the facts were known to public men, in reference to the expenditure incurred by foreign States in support of scientific inquiry, they would be willing to do something in this country of a sufficient and statesmanlike character. As it is, the concessions which have been made in this direction appear to me to be in some instances not based upon a really comprehensive knowledge of the situation. Thus the tentative grant of 4,000*l.* a year from the Treasury to the Royal Society of London appears to me not to be a well-devised experiment in the promotion of scientific research by means of grants of money, because it is on too small a scale to produce any definite effect, and because the money cannot be relied upon from year to year as a permanent source of support to any serious undertaking.

The Royal Society most laboriously and conscientiously does its best to use this money to the satisfaction of the country, but the task thus assigned to it is one of almost insurmountable difficulty. In fact, no such miniature experiments are needed. The experiment has been made on a large scale in Germany, and satisfactory results have been obtained. The reasonable course to pursue is to benefit by the experience, as to details and methods of administration, obtained in the course of the last sixty years in Germany, and to apply that experience to our own case.

It is quite clear that "the voluntary principle" can do little towards the adequate endowment of scientific research. Ancient endowments belonging to the country must be applied thereto, or else local or imperial taxes must be the source of the necessary support. Seeing that the results of research are distinctly of imperial, and not of local value—it would seem appropria

that a portion of the imperial revenue should be devoted to their achievement. In fact, as I have before mentioned, the principle of such an application of public money has long been admitted, and is in operation.

Whilst voluntary donations on the part of private persons can do little to constitute a fund which shall provide the requisite endowment for the scheme of biological institutes which I have sketched (not to mention those required for other branches of science), yet those who are interested in the progress of scientific investigation may by individual effort do something, however little, towards placing research in a more advantageous position in this country. Supposing it were possible, as I am sanguine enough to believe that it is, to collect in the course of a year or two from private sources a sum of 20,000*l*. for the maintenance of a biological laboratory and staff, it would be necessary, in expending so limited a sum, to aim at the provision of something which would be likely to produce the largest and most obvious results in return for the outlay, and to benefit the largest number of scientific observers in this department.

I believe that it is the general opinion among biologists that there could be no more generally useful institution thus set in operation than a biological laboratory upon the sea-coast, which, besides its own permanent staff of officers, would throw open its resources to such naturalists as might from time to time be able to devote themselves to researches within its precincts. There is no such laboratory on the whole of the long line of British coast. At Naples there is Dr. Dohrn's celebrated and invaluable laboratory, which is frequented by naturalists from all parts of the world; at Trieste the Austrian Government supports such a laboratory; at Concarneau, Roscoff, and Villefranche, the French Government has such institutions; at Beaufort, in North Carolina, the Johns Hopkins University has its marine laboratory; and at Newport, Professor Alexander Agassiz has arranged a very perfect institution also for the study of marine life. In spite of the great interest which English naturalists have always taken in the exploration of the sea and marine organisms—in spite of the fact that the success and even the existence of our fisheries-industries to a large extent depends upon our gaining the knowledge which a well-organised laboratory of marine biology would help us to gain, there is actually no such institution in existence.

This is not the occasion on which to explain precisely how and to what extent a laboratory of marine zoology might be of national importance. I hope to see that matter brought before the Section during the course of our meeting. But I may point out now, that though it appears to me that the great need for biological institutes, to which I have drawn your attention, can *not* be met by private munificence, and must in the end be arranged for by the continued action of the Government in carrying out a policy to which it has for many years been committed, and which has been approved by Conservatives and Liberals alike—yet such a special institution as a laboratory of marine biology, serving as a temporary workshop to any and all of our numerous students of the important problems connected with the life of marine plants and animals, might very well be undertaken from private funds. Should it be possible, on the occasion of this meeting of the British Association in Southport, to obtain some promise of assistance towards the realisation of this project, I think we shall be able to congratulate ourselves on having done something, though small perhaps in amount, towards making better provision for biological research, and therefore something towards the advancement of science.

In conclusion, let me say that, in advocating to-day the claim of biological science to a far greater measure of support than it receives at present from the public funds, I have endeavoured to press that claim chiefly on the ground of the obvious utility to the community of that kind of knowledge which is called biology. I have endeavoured to meet the opposition of those who object to the interference of the State wherever it may be possible to attain the end in view without such interference, but who profess themselves willing to see public money expended in promoting objects which are of real importance to the country, and which cannot be trusted to the voluntary enterprise arising from the operation of the laws of self-preservation and the struggle for wealth. There are, however, it seems to me, further reasons for desiring a thorough and practical recognition by the State of the value of scientific research. There are not wanting persons of some cultivation who have perceived and fully realised the value of that knowledge which is called science, and of its methods, and yet are anxious to restrain rather than to aid the growth of that knowledge. They find in science something inimical to their own interests, and accordingly either condemn it as dangerous and untrustworthy, or encourage themselves to treat it with contempt by asserting that "after all, science counts for very little"—a statement which is unhappily true in one sense, though totally untrue when it is intended to signify that the progress of science is not a matter which profoundly influences every factor in the well-being of the community. Amongst such people there is a positive hatred of science, which finds expression in their exclusion of it, even at this day, from the ordinary curriculum of public school education, and in the baseless though oft-repeated calumny that science is hostile to art, and is responsible for all that is harsh, ugly, and repulsive in modern life. To such opponents of the advancement of science, it is of little use to offer explanations and arguments. But we may, when we reflect on their instinctive hostility and the misrepresentations of science and the scientific spirit which it leads them to disseminate, console ourselves by bringing to mind what science really is, and what truly is the nature of that calling in which a man who makes new knowledge is engaged.

They mock at the botanist as a pedant, and the zoologist as a monomaniac; they execrate the physiologist as a monster of cruelty, and brand the geologist as a blasphemer; chemistry is held responsible for the abomination of aniline dyes and the pollution of rivers, and physics for the dirt and misery of great factory towns. By these unbelievers science is declared responsible for individual eccentricities of character, as well as for the sins of the commercial utilisers of new knowledge. The pursuit of science is said to produce a dearth of imagination, incapability of enjoying the beauty either of nature or of art, scorn of literary culture, arrogance, irreverence, vanity, and the ambition of personal glorification.

Such are the charges from time to time made by those who dislike science, and for such reasons they would withhold, and persuade others to withhold, the fair measure of support for scientific research which this country owes to the community of civilised States. Not in reply to these misrepresentations, but by way of contrast, I would here state what science seems to be to those who are on the other side, and how, therefore, it seems to them wrong to delay in doing all that the wealth and power of the State can do to promote its progress.

Science is not a name applicable to any one branch of knowledge, but includes all knowledge which is of a certain order or scale of completeness. All knowledge which is deep enough to touch the causes of things, is Science; all inquiry into the causes of things is scientific inquiry. It is not only co-extensive with the area of human knowledge, but no branch of it can advance far without reacting upon other branches; no department of Science can be neglected without sooner or later causing a check to other departments. No man can truly say this branch of Science is useful and shall be cultivated, whilst this is worthless and shall be let alone; for all are necessary, and one grows by the aid of another, and in turn furnishes methods and results assisting in the progress of that from which it lately borrowed.

We desire the increase and the support and the acceptance of Science, not only because it has a certain material value and enables men to battle with the forces of nature and to turn them to account, so as to increase both the intensity and the extension of healthy human life: that is a good reason, and for some persons, it may be, the only reason. But there is something to be said beyond this.

The pursuit of scientific discovery, the making of new knowledge, gratifies an appetite which, from whatever cause it may arise, is deeply seated in man's nature, and indeed is the most distinctive of his properties. Man owes this intense desire to know the nature of things, smothered though it often be by other cravings which he shares with the brutes, to an inherited race-perception stronger than the reasoning faculty of the individual. When once aroused and in a measure gratified, this desire becomes a guiding passion. The instinctive tendency to search out the causes of things, gradually strengthening as generation after generation of men have stumbled and struggled in ignorance, has at last become an active and widely extending force: it has given rise to a new faith.

To obey this instinct—that is, to aid in the production of new knowledge—is the keenest and the purest pleasure of which man is capable, greater than that derived from the exercise of his animal faculties, in proportion as man's mind is something greater and further developed than the mind of brutes. It is in itself an unmixed good, the one thing which commends itself as still "worth while" when all other employments and delights prove themselves stale and unprofitable.

Arrogant and foolish as those men have appeared who, in

times of persecution and in the midst of a contemptuous society, have, with an ardour proportioned to the prevailing neglect, pursued some special line of scientific inquiry, it is nevertheless true that in itself, apart from special social conditions, Science must develop in a community which honours and desires it before all things, qualities and characteristics which are the highest, the most human of human attributes. These are, firstly, the fearless love and unflinching acceptance of truth; hopeful patience; that true humility which is content not to know what cannot be known, yet labours and waits; love of Nature, who is not less, but more, worshipped by those who know her best; love of the human brotherhood for whom and with whom the growth of Science is desired and effected.

No one can trace the limits of Science, nor the possibilities of happiness both of mind and body which it may bring in the future to mankind. Boundless though the prospect is yet the minutest contribution to the onward growth has its absolute and unassailable value; once made it can never be lost; its effect is for ever in the history of man

Arts perish, and the noblest works which artists give to the world. Art (though the source of great and noble delights) cannot create nor perpetuate; it embodies only that which already exists in human experience, whilst the results of its highest flights are doomed to decay and sterility. A vain regret, a constant effort to emulate or to imitate the past, is the fitting and laudable characteristic of Art at the present day. There is, indeed, no truth in the popular partition of human affairs between Science and Art as between two antagonistic or even comparable interests; but the contrast which they present in points such as those just mentioned is forcible. Science is essentially creative; new knowledge—the experience and understanding of things which were *previously non-existent for man's intelligence*, is its constant achievement. And these creations never perish; the new is built on and incorporates the old; there is no turning back to recover what has lapsed through age; the oldest discovery is even fresher than the new, yielding in ever increasing number new results, in which it is itself reproduced and perpetuated, as the parent in the child.

This, then, is the faith which has taken shape in proportion as the innate desire of man for more knowledge has asserted itself—namely, that there is no greater good than the increase of Science; that through it all other good will follow. Good as Science is in itself, the desire and search for it is even better, raising men above vile things and worthless competitions to a fuller life and keener enjoyments. Through it we believe that man will be saved from misery and degradation, not merely acquiring new material powers, but learning to use and to guide his life with understanding. Through Science he will be freed from the fetters of superstition; through faith in Science he will acquire a new and enduring delight in the exercise of his capacities; he will gain a zest and interest in life such as the present phase of culture fails to supply.

In opposition to the view that the pursuit of Science can obtain a strong hold upon human life, it may be argued, that on no reasonable ground can it appear a necessary or advantageous thing to the individual man to concern himself with the growth and progress of that which is merely likely to benefit the distant posterity of the human race. Our reply is: Let those who contend for the reasonableness of human motives develop, if they can, any theory of human conduct in which reasonable self-interest shall be man's guide. We do not contend for any such theory. By reasoning we may explain and trace the development of human nature, but we cannot change it by any such process. It is demonstrably unreasonable for the individual man, guided by self-interest, to share the dangers and privations of his brother-man, and yet, in common with many lower animals, he has an inherited quality which makes it a pleasure to him to do so; it is unreasonable for the mother to protect her offspring, and yet it is the natural and inherited quality of mothers to derive pleasure from doing so; it is unreasonable for the half-starved poor to aid their wholly starving brethren, and yet such compassion is natural and pleasurable to those who show it, and is the constant rule of life. Unreasonable though these things are from the point of view of individual self-interest, yet they are done because to do them is pleasurable, to leave them undone a pain. The race has, as it were, in these respects befooled the individual, and in the course of evolution has planted in him, in its own interests, an irrational capacity for taking pleasure in doing that which no reasoning in regard to self-interest could justify. As with these lower and more widely distributed instincts, shared by man with some lower social animals, so is it with this higher and more peculiar instinct—the tendency to pursue new knowledge. Whether reasonable or not, it has by the laws of heredity and selection become part of us and exists: its operation is beneficial to the race: its gratification is a source of keen pleasure to the individual—an end in itself. We may safely count upon it as a factor in human nature; it is in our power to cultivate and develop it, or, on the other hand, to starve and distort it for a while, though to do so is to waste time in opposing the irresistible.

As day by day the old-fashioned stimulus to the higher life loses the dread control which it once exercised over the thoughts of men, the pursuit of wealth and the indulgence in fruitless gratifications of sense become to an increasing number the chief concerns of their mental life. Such occupations fail to satisfy the deep desires of humanity; they become wearisome and meaningless, so that we hear men questioning whether life be worth living. When the dreams and aspirations of the youthful world have lost their old significance and their strong power to raise men's lives, it will be well for that community which has organised in time a following of and a reverence for an ideal Good, which may serve to lift the national mind above the level of sensuality and to insure a belief in the hopefulness and worth of life. The faith in Science can fill this place—the progress of Science is an ideal Good, sufficient to exert this great influence.

It is for this reason more than any other, as it seems to those who hold this faith, that the progress and diffusion of scientific research, its encouragement and reverential nurture, should be a chief business of the community, whether collectively or individually, at the present day.

Department of Anthropology

ADDRESS BY WILLIAM PENGELLY, F.R.S., F.G.S., VICE-PRESIDENT OF THE SECTION.

ANTHROPOLOGY, on one of its numerous sides, marches with geology; and hence it is, no doubt, that I, for many years a labourer very near this somewhat ill-defined border, have been invited to assist my friends and neighbours in the work which lies before them during the Association week. I have the more cheerfully accepted the invitation from a vivid recollection that, when on a few occasions I have come uninvited into this Department, my reception has been so very cordial as to lead me to ask myself whether the reports which for many years (1864 to 1880) I laid annually before my geological brethren did not derive their chief interest from their anthropological bearings and teachings.

In 1858—a quarter of a century ago—I had the pleasure of reading to the Geological Section of the Association the first public communication on the exploration, then in progress, of Brixham Cavern (more correctly, Brixham Windmill Hill Cavern); and as any interest connected with that paper lay entirely in the evidence it contained of the inosculation and contemporaneity of human industrial relics, of a rude character, with remains of certain extinct mammals, I purpose on this occasion to lay before the Department a few thoughts, retrospective and prospective, which may be said to radiate from that exploration, confining myself mainly to South Devon.

Probably nothing will better show the apparent apathy and scepticism with which, up to 1858, all geological evidence of the antiquity of man was received by British geologists generally, than the following statement of facts:—

About the beginning of the second quarter of the present century the late Rev. J. MacEnery made Kent's Cavern, or Kent's Hole, near Torquay, famous by his researches and discoveries there. He not only found flint implements beneath a thick continuous sheet of stalagmite, but, after a most careful and painstaking investigation in the presence of witnesses, arrived at the conclusion that the flints "were deposited in their deep position before the creation of the stalagmite" (*Trans. Devon. Assoc.* iii. 330); and when it was suggested by the Rev. Dr. Buckland, to whom he at once and without reservation communicated all his discoveries, that "the ancient Britons had scooped out ovens in the stalagmite, and that through them the knives got admission to the 'diluvium,'" he replied, "I am bold to say that in no instance have I discovered evidence of breaches or ovens in the floor, but one continuous plate of stalagmite diffused uniformly over the loam" (*Ibid.* p. 334).

He added, "It is painful to dissent from so high an authority, and more particularly so from my concurrence generally in his views of the phenomena of these caves, which three years' personal observation has in almost every instance enabled me to verify" (*Ibid.* p. 338).

It is, perhaps, not surprising that Dr. Buckland, one of the leading geologists of his day, should be too tenacious of his opinion, and feel too secure in his position to yield to the statements and arguments of his comparatively young friend MacEnery, then scarcely known to the scientific world.

That the position taken by Buckland retarded the progress of truth, and was calculated to check the ardour of research, is apparently certain, and much to be regretted; but it should be remembered that, at least, as early as 1819 he taught that "the two great points . . . of the low antiquity of the human race, and the universality of a recent deluge, are most satisfactorily confirmed by everything that has yet been brought to light by geological investigations" ("Vindiciæ Geologicæ," p. 24); that early in 1822 he reiterated and emphasised these opinions in his famous Kirkdale paper (*Phil. Trans.* for 1822, pp. 171-236), which the Royal Society "crowned with the Copley medal" (*Quart. Journ. Geol. Soc.* vol. xiii. p. xxxiii.); that in 1823, having amplified and revised this paper, he published it as an independent quarto volume under the attractive title of "Reliquiæ Diluvianæ," of which he issued a second edition in 1824; and that, though his acquaintance with Kent's Cavern was much less intimate than that of MacEnery, he, nevertheless, was, of the two, the earlier worker there, and in fact had discovered a flint implement in it before MacEnery had ever seen that or any other cavern—the first tool of the kind found in any cavern, it is believed, and which in all probability was met with under circumstances not in conflict with his published opinion on the low antiquity of man. I confess that under such circumstances, human nature being what it is, the line followed by Dr. Buckland seems to me to have been that which most men would have pursued.

It was, at any rate, the line to which he adhered as late, at least, as 1837, for in his well-known "Bridgewater Treatise," published that year, after describing his visit to the caverns near Liége, famous through the discoveries of Dr. Schmerling, he said, "The human bones found in these caverns are in a state of less decay than those of the extinct species of beasts; they are accompanied by rude flint knives and other instruments of flint and bone, and are probably derived from uncivilised tribes that inhabited the caves. Some of the human bones may also be the remains of individuals who, in more recent times, have been buried in such convenient repositories. M. Schmerling . . . expresses his opinion that these human bones are coeval with those of the quadrupeds, of extinct species, found with them; an opinion from which the author, after a careful examination of M. Schmerling's collection, entirely dissents" (*op. cit.* i. 602).

It may be doubted, however, whether his faith in these, his early, convictions remained unshaken to the end. I have frequently been told by one of his contemporary professors at Oxford, who knew him intimately, that Buckland shrank from the task of preparing for the press new editions of his "Reliquiæ Diluvianæ" and his "Bridgewater Treatise." "The work," he said, "would be not editing, but re-writing."

Mr. MacEnery intended to publish his "Cavern Researches" in one volume quarto, illustrated with thirty plates. In what appears to have been his second prospectus, unfortunately not dated, he said, "The limited circulation of works of this nature, being by no means equal to the expenses attendant on the execution of so large a series" [of plates], "the author is obliged to depart from his original plan, and to solicit the support of those who may feel an interest in the result of his researches."

There is reason to believe that at least twenty-one of the plates were ready, and that the rough copy of much of his manuscript was written; but that, the support he solicited not being forthcoming, the idea of publishing had to be abandoned (see *Trans. Devon. Assoc.* iii. 198–201).

In 1840 Mr. R. A. C. Austen (now Godwin-Austen), F.G S., read to the Geological Society of London a paper on the Bone Caves of Devonshire, which, with some amplifications, was incorporated in his memoir on the geology of the south-east of Devonshire, printed in the *Transactions* of the Society in 1842 (2nd ser. vi. 433-489). Speaking of his own researches in Kent's Cavern he said, "Human remains and works of art, such as arrow-heads and knives of flint, occur in all parts of the cave and throughout the entire thickness of the clay: and no distinction founded on condition, distribution, or relative position can be observed whereby the human can be separated from the other reliquiæ" (*Ibid.* p. 444).

He added, "My own researches were constantly conducted in parts of the cave which had never been disturbed, and in every instance the bones were procured from beneath a thick covering of stalagmite; so far, then, the bones and works of man must have been introduced into the cave before the flooring of stalagmite had been formed" (*Ibid.* p. 446).

Though these important and emphatic statements were so fortunate as to be committed to the safe keeping of print with but little delay, and under the most favourable circumstances, they appear neither to have excited any interest, nor indeed to have received much, if any, attention.

In 1846, the Torquay Natural History Society appointed a Committee, consisting of Dr. Battersby, Mr. Vivian, and myself—all tolerably familiar with the statements of Mr. MacEnery and Mr. Austen—to make a few diggings in Kent's Cavern for the purpose of obtaining specimens for their museum. The work, though more or less desultory and unsystematic, was by no means carelessly done, and the Committee were unanimously and perfectly satisfied that the objects they met with had been deposited at the same time as the matrix in which they were inhumed. At the close of their investigation they drew up a report which was printed in the *Torquay Directory* for November 6, 1846 (see *Trans. Devon. Assoc.* x. 162). Its substance, embodied in a paper by Mr. Vivian, was read to the Geological Society of London on May 12, 1847, as well as to the British Association in the succeeding June, and the following abstract was printed in the Report of the Association for that year (p. 73):—

"The important point that we have established is, that relics of human art are found *beneath* the unbroken floor of stalagmite. After taking every precaution, by sweeping the surface, and examining most minutely whether there were any traces of the floor having been previously disturbed, we broke through the solid stalagmite in three different parts of the cavern, and in each instance found flint knives. . . . In the spot where the most highly finished specimen was found, the passage was so low that it was extremely difficult, with quarrymen's tools and good workmen, to break through the crust; and the supposition that it had been previously disturbed is impossible."

It will be borne in mind that the same paper was read the month before to the Geological Society. The Council of that body, being apparently unprepared to print in their *Quarterly Journal* the statements it contained, contented themselves with the following notice, given here in its entirety (*op. cit.* iii. 353):—

"On Kent's Cavern, near Torquay," by Mr. Edward Vivian.—"In this paper an account was given of some recent researches in that cavern by a committee of the Torquay Natural History Society, during which the bones of various extinct species of animals were found in several situations."

It will be observed that the "flint knives" were utterly ignored, a fact rendered the more significant by the following announcement on the wrapper of the journal:—"The Editor of the *Quarterly Journal* is directed to make it known to the public that the authors alone are responsible for the facts and opinions contained in their respective papers."

Such, briefly, were the principal researches in Kent's Cavern, at intervals from 1825 to 1847. Their reception was by no means encouraging: Mr. MacEnery, after incurring very considerable expense, was under the necessity of abandoning the intention of publishing his "Cavern Researches;" Mr. Austen's paper, though printed unabridged, was given to an apathetic, unbelieving world, and was apparently without effect; and Mr. Vivian's paper, virtually the report by a committee of which he was a member, was cut down to four lines of a harmless, unexciting character.

For some years nothing occurred to break the quietude, which but for an unexpected discovery on the southern shore of Torbay would probably have remained to this day.

Early in 1858 the workmen engaged in a limestone quarry on Windmill Hill, overhanging the fishing town of Brixham in South Devon, broke unexpectedly a hole through what proved to be the roof of an unknown and unsuspected cavern. I visited it very soon after the discovery, and secured to myself the refusal of a lease to include the right of exploration. As the story of this cavern has been told at some length elsewhere (see *Phil. Trans.* clxiii. 471–572; or *Trans. Devon. Assoc.* vi. 775–856). it will here suffice to say that at the instance of the late Dr. H. Falconer, the eminent palæontologist, the subject

was taken up very cordially by the Royal and Geological Societies of London, a Committee was appointed by the latter body, the exploration was placed under the superintendence of Mr. (now Prof.) Prestwich and myself, and, being the only resident member of the Committee, the actual superintendence fell of necessity to me.

The following facts connected with this cavern were no doubt influential in leading to the decision to have it explored :—

1. It was a virgin cave which had been hermetically sealed during an incalculably long period, the last previous event in its history being the introduction of a reindeer antler, found attached to the upper surface of the stalagmitic floor. It was therefore free from the objection urged sometimes against Kent's Cavern, that, having been known from time immemorial, and up to 1825 always open to all comers, it had perhaps been ransacked again and again.

2. It was believed, and it proved, to be a comparatively very small cavern, so that its complete exploration was not likely to require a large expenditure of time or of money.

It will be seen that the exploration was placed under circumstances much more likely to command attention than any of those which had preceded it. It was to be carried on under the auspices of the Royal and Geological Societies, by a Committee consisting of Mr. S. H. Beckles, Mr. G. Busk, Rev. R. Everest, Dr. H. Falconer, Mr. Godwin-Austen, Sir C. Lyell, Prof. Owen, Dr. J. Percy, Mr. J. Prestwich, Prof. (now Sir A. C.) Ramsay, and myself—all Fellows of the Geological Society, and almost all of them of the Royal Society also.

It was impossible not to feel, however, that the mode of exploration must be such as would not merely satisfy those actually engaged in the work, but such as would command for the results which might be obtained the acceptance of the scientific world generally. Hence I resolved to have nothing whatever to do with "trial pits" here and there, or with shafts to be sunk in selected places; but, first, to examine and remove the stalagmite floor; then the entire bed immediately below (if not of inconvenient depth) horizontally throughout the entire length of the cavern, or so far as practicable; this accomplished, to proceed in like manner with the next lower bed; and so on until all the deposits had been removed.

This method, uniformly followed, was preferable to any other, because it would reveal the general stratigraphical order of the deposits, with the amount and direction of such "dip" as they might have, as well as any variations in the thickness of the beds; it would afford the only chance of securing all the fossils, and of thus ascertaining, not only the different kinds of animals represented in the cave, but also the ratios which the numbers of individuals of the various species bore to one another, as well as all peculiar or noteworthy collocations; it would disclose the extent, character, and general features of the cavern itself; it was undoubtedly the least expensive mode of exploration; and it would render it almost impossible to refer bones or indications of human existence to wrong beds, depths, or associations.

The work was begun in July, 1858, and closed at the end of twelve months, when the cavern had practically been completely emptied; an official report was printed in the *Philosophical Transactions* for 1873, and all the specimens have been handed over to the British Museum.

The paper on the subject mentioned at the beginning of this address was read in September, 1858, during the meeting of the Association at Leeds, when I had the pleasure of stating that eight flint tools had already been found in various parts of the cavern, all of them inosculating with bones of mammalia, at depths varying from nine to forty-two inches in the cave-earth, on which lay a sheet of stalagmite from three to eight inches thick, and having *within* it and *on* it relics of lion, hyæna, bear, mammoth, rhinoceros, and reindeer.

It soon became obvious that the geological apathy previously spoken of had been rather apparent than real. In fact, geologists were found to have been not so much disinclined to entertain the question of human antiquity as to doubt the trustworthiness of the evidence which had previously been offered to them on the subject. It was felt, moreover, that the Brixham evidence made it worth while, and indeed a duty, to re-examine that from Kent's Cavern, as well as that said to have been met with in river deposits in the valley of the Somme and elsewhere.

The first fruits, I believe, of this awakening was a paper by Mr. Prestwich, read to the Royal Society, May 26, 1859, on the occurrence of flint implements, associated with the remains of animals of extinct species in beds of a late geological period, in France at Amiens and Abbeville, and in England at Hoxne (*Phil. Trans.* for 1860, pp. 277–317). This paper contains explicit evidence that Brixham Cavern had had no small share in disposing its author to undertake the investigation, which added to his own great reputation and rescued M. Boucher de Perthes from undeserved neglect. "It was not," says Mr. Prestwich, "until I had myself witnessed the conditions under which these flint implements had been found at Brixham, that I became fully impressed with the validity of the doubts thrown upon the previously prevailing opinions with respect to such remains in caves" (*op. cit.* p. 280).

Sir C. Lyell, too, in his address to the Geological Section of the British Association, at Aberdeen, in September, 1859, said, "The facts recently brought to light during the systematic investigation, as reported on by Dr. Falconer, of the Brixham Cave, must, I think, have prepared you to admit that scepticism in regard to the cave evidence in favour of the antiquity of man had previously been pushed to an extreme" (*Report Brit. Assoc.* 1859, *Trans. Sects.* p. 93).

It is probably unnecessary to quote further to show how very large a share the exploration at Brixham had in impressing the scientific world generally with the value and importance of the geological evidence of man's antiquity. That impression, begun as we have seen in 1858, has not only lasted to the present day, but has probably not yet culminated. It has produced numerous volumes, crowds of papers, countless articles in reviews and magazines, in various countries; and, perhaps in order to show how very popular the subject became almost immediately, it is only necessary to state that Sir C. Lyell's great work on the "Antiquity of Man" was published in February, 1863; the second edition appeared in the following April, and the third followed in the succeeding November—three editions of a bulky scientific work in less than ten months! A fourth edition was published in May, 1873.

Few, it may be presumed, can now doubt that those who before 1858 believed that our fathers had under-estimated human antiquity, and fought for their belief, have at length obtained a victory. Nevertheless, every anthropologist has doubtless from time to time

> "Heard the distant and random gun
> That the foe was sullenly firing."

The "foe," to speak metaphorically, seems to consist of very irregular forces, occasionally unfair but never dangerous, sometimes very amusing, and frequently but badly armed or without *any* real armour. The Spartan law which fined a citizen heavily for going into battle unarmed was probably a very wise one.

For example, and dropping a metaphor, a pamphlet published in 1877 contains the following passage:—"With regard to all these supposed flint implements and spear- and arrow-heads found in various places, it may be well to mention here the frank confession of Dr. Carpenter. He has told us from the presidential chair of the Royal Academy that 'No logical proof can be adduced that the peculiar shapes of these flints were given them by human hands'" (see 'Is the Book Wrong? A Question for Sceptics,' by Hely H. A. Smith, p. 26). The words ascribed to Dr. Carpenter are put within inverted commas, and are the whole of the quotation from him. I was a good deal mystified on first reading them, for while it seemed likely that the president spoken of was the well known member of this Association —Dr. W. B. Carpenter—it was difficult to account for his being in the presidential chair of the Royal Academy, and not easy to understand what the Royal Academy had to do with flint implements. A little search, however, showed that the address which Dr. W. B. Carpenter delivered in 1872 from the presidential chair of, not the Royal Academy, but the British Association, contained the actual words quoted, followed immediately by others which the author of the pamphlet found it inconvenient to include in his quotation. Dr. Carpenter, speaking of "common sense," referred, by way of illustration, to the "flint implements" of the Abbeville and Amiens gravel beds, and remarked, "No logical proof can be adduced that the peculiar shapes of these flints were given to them by human hands; but does any unprejudiced person now doubt it?" (*Report Brit. Assoc.* 1872, p. lxxv.). Dr. Carpenter, after some further remarks on the "flint implements," concluded his paragraph respecting them with the following words:—"Thus what was in the first instance a matter of discussion, has now become one of those 'self-evident' propositions which claim the unhesitating assent of all whose opinion on the subject is entitled to the least weight."

It cannot be doubted that, taken in its entirety, that is to say, taken as every lover of truth and fairness should and would take

it, Dr. Carpenter's paragraph would produce on the mind of the reader a very different effect to that likely, and no doubt intended, to be produced by the mutilated version of it given in the pamphlet.

A second edition of the pamphlet has been given to the world. Dr. Carpenter is still in the presidential chair of the Royal Academy, and the quotation from his address is as conveniently short as before.

It would be easy to bring together a large number of similar modes of "defending the cause of truth"—to use the words of the pamphlet just noticed—but space and time forbid.

I cannot, however, forego the pleasure of introducing the following recent and probably novel explanation of cavern phenomena. In 1882 my attention was directed to two articles, by one and the same writer, on "Bone-Cave Phenomena." The writer's theme was professedly the Victoria Cave, near Settle, Yorkshire, which he says was an old Roman lead mine, but his remarks are intended to apply to bone-caves in general. He takes a very early opportunity in the second article of stating that "We shall have to take care to distinguish between what is truly indicated in the 'science' view from what are purely imaginary exaggerations of its natural and historical phenomena"; and he no doubt believes that he has taken this care.

"We have now," he says, "to present our own view of the Victoria Cave and the phenomena connected with it, premising that a great many of the old mines in Europe were opened by Phœnician colonists and metal workers, a thousand years before the Romans had set foot in Britain, which accounts for the various floors of stalagmite found in most caves, and also for the variety of groups of bones embedded in them. The animals represented by them when living were not running wild about the hills devouring each other, as science men suppose, but the useful auxiliaries and trained drudges of the miners in their work. Some of them, as the bear, had simply been hunted and used for food, and others of a fierce character, as the hyæna, to frighten and keep in awe the native Britons. The larger species of mammalia, as the elephant, the rhinoceros and hippopotamus, and beasts foreign to the country, the Romans, no less than the Phœnicians, had every facility in bringing with them in their ships of commerce from Carthage, or other of the African ports. These, with the native horse, ox, and stag, which are always found in larger numbers in the caves than the remains of foreign animals, all worked peacefully together in the various operations of the mines. . . . The hippopotamus, although amphibious, is a grand beast for heavy work, such as mining, quarrying, or road-making, and his keeper would take care that he was comfortably lodged in a tank of water during the night. . . . The phenomena of the Victoria Cave Lead Mine differ in no material respect from those of hundreds of others, whether of lead, copper, silver, or iron, worked in Roman and pre-Roman times in all parts of Europe. Its tunnels have all been regularly quarried and mined, *not by ancient seas*, but by the hands of historic man. Double openings have been made in every case for convenient ingress and egress, during the process of excavation. Its roadways had been levelled, and holes made up with breccia, gravel, sand, and bones of beasts that had succumbed to toil, on which sledges, trolleys, and waggons could glide or run. . . . Near the entrance inside Victoria Cave were found the usual beds of charcoal and the hearths for refining the metal, while close by on the hillside may still be seen the old kilns in which the men 'roasted' the metallic ores and burned lime."

Should any one be disposed to ascribe these articles to some master of the art of joking, it need only be replied that they appeared in a religious journal (*The Champion of the Faith against Current Infidelity* for April 20, and May 11, 1882, vol. i. pp. 5 and 26), with the writer's name appended; and that I have reason to believe they were written seriously and in earnest.

It has been already intimated that Brixham Cavern has secured a somewhat prominent place in literature; and it can scarcely be needful to add that some of the printed statements respecting it are not quite correct. The following instances of inaccuracy may be taken as samples:—

The late Prof. Ansted, describing Brixham Cavern in 1861, said, "In the middle of the cavern, under stalagmite itself, and actually entangled with an antler of a reindeer and the bones of the great cavern bear, were found rude sculptured flints, such as are known to have been used by savages in most parts of the world" ("Geological Gossip," p. 209).

To be "entangled" with one another, the antler, the bones of the cave bear, and the flints must have been all lying together As a matter of fact, however, the antler was *on* the upper surface of the sheet of stalagmite, while all the relics of the cave bear and all the flints were in detrital beds below that sheet. Again, the flints nearest the bear's bones in question were two in number; they were twelve feet south of the bones, and fifteen inches less deep in the bed. There was no approach to entanglement.

Should it be suggested that it is scarcely necessary to correct errors on scientific questions in works, like "Geological Gossip," professedly popular and intended for the million, I should venture to express the opinion that the strictest accuracy is specially required in such books, as the great majority of their readers are entirely at the mercy of the compilers. Those who read scientific books of a higher class are much more capable of taking care of themselves.

Prof. Ansted's slip found its way into a scientific journal, where it was made the basis of a speculation (see *Geologist*, 1861, p. 246).

The most recent noteworthy inaccuracies connected with this famous cavern are, so far as I am aware, two in the English edition of Prof. N. Joly's "Man before Metals" (1883).

According to the first, "An entire left hind leg of *Ursus spelæus* was found lying above the incrustation of stalagmite which covered the bones of other extinct species and the carved flints" (p. 52).

It is only necessary in reply to this to repeat what has been already stated: all the bones of cave-bear found in the cavern were in beds *below* the stalagmite.

The following quotation from the same work contains the second inaccuracy, or, more correctly, group of inaccuracies, mentioned above: "We may mention among others the cave at Brixham, where, associated with fragments of rude pottery and bones of extinct species, heaps of oyster shells and other salt-water mollusks occur, as well as fish-bones of the genus *scarus*" (p. 104).

I am afraid there is no way of dealing with this paragraph except that of meeting all its statements with unqualified denials. In short, Brixham Windmill Hill Cavern contained no pottery of any kind whatever, not a single oyster-shell, nor even a solitary bone of any species of fish. One common limpet shell was the only relic of a marine organism met with in the cavern.

As already intimated, the result of the researches at Brixham quickened a desire to re-examine the Kent's Cavern evidence, and this received a considerable stimulus from the publication of Sir C. Lyell's "Antiquity of Man" in 1863. Having in the meantime made a careful survey of the cavern, and ascertained that there was a very large area in which the deposits were certainly intact, to say nothing of unsuspected branches which in all probability would be discovered during a thorough and systematic exploration, I had arrived at the conclusion that, taking the cavern at its known dimensions merely, the cost of an investigation as complete as that at Brixham would not be less than 1000*l*.

Early in 1864 I suggested to Sir C. Lyell that an application should be made to the British Association, during the meeting to be held at Bath that year, for the appointment of a Committee, with a grant of money, to make an exploration of Kent's Cavern; and it was decided that I should take the necessary steps in the matter. The proposal being cordially received by the Committee of the Geological Section, and well supported in the Committee of Recommendations, a Committee—consisting of Sir C. Lyell, Mr. J. Evans, Mr. (now Sir) J. Lubbock, Prof. J. Phillips, Mr. E. Vivian, and myself (Hon. Secretary and Reporter)—was appointed, with 100*l*. placed at their disposal. Mr. G. Busk was added to the Committee in 1866, Mr. W. Boyd Dawkins in 1868, Mr. W. Ayshford Sanford in 1869, and Mr. J. E. Lee in 1873. The late Sir L. Palk (afterwards Lord Haldon), the proprietor, placed the cavern entirely under the control of the Committee during the continuance of the work; the investigation was begun on March 28, 1865, and continued without intermission to June 19, 1880, the Committee being annually reappointed with fresh grants of money, which in the aggregrate amounted to 1900*l*., besides 63*l*. received from various private sources.

The mode of exploration was essentially the same as that followed at Windmill Hill, Brixham, but as Kent's Cavern, instead of being a series of narrow galleries, contained a considerable number of capacious chambers, and as the aim of the explorers was to ascertain not merely what objects the deposits contained, but their exact position, their distribution, their condition, their collocation, and their relative abundance, the details had to be

considerably more elaborate, while they remained so perfectly simple that the workmen had not the least difficulty in carrying them out under my daily superintendence. The process being fully described in the First Annual Report by the Committee (see *Report. Brit. Assoc.* 1865, pp. 19, 20), it is unnecessary to repeat it here.

Mr. Godwin-Austen, while agreeing with Mr. MacEnery that flint implements occurred under the stalagmite, contended that they were found throughout the entire thickness of the cave earth. MacEnery, on the other hand, was of opinion that in most cases their situation was intermediate between the bottom of the stalagmite and the upper surface of the cave earth; and, while admitting that occasionally, though rarely, they had been met with somewhat lower, he stated that the greatest depth to which he had been able to trace them was not more than a few inches below the surface of the cave earth (*Trans. Devon. Assoc.* iii. 326–327). The Committee soon found themselves in a position to confirm Mr. Godwin-Austen's statement, and to say with him that "no distinction founded on condition, distribution, or relative position can be observed whereby the human can be separated from the other reliquiæ" (*Trans. Geol. Soc.* 2nd ser. vi. 444).

Mr. MacEnery's "Plate F" contains seven figures of three remarkable canine teeth, and the following statement respecting them:—"Teeth of *Ursus cultridens*, found in the cave of Kent's Hole, near Torquay, Devon, by Rev. Mr. McEnery, January, 1826, in Diluvial Mud mix'd with Teeth and Gnaw'd Bones of Rhinoceros, Elephant, Horse, Ox, Elk, and Deer, with Teeth and Bones of Hyænas, Bears, Wolves, Foxes, &c."

It is worthy of note that no other plate in the entire series names the date on which the specimens were found, or the mammals with whose remains they were commingled. This arose probably from the fact, well known to MacEnery, that no such specimens had been found elsewhere in Britain; and possibly also to emphasise the statements in his text, should any doubt be thrown on his discovery.

It is, no doubt, unnecessary to say here that the teeth belonged to a large species of carnivore to which, in 1846, Prof. Owen gave the name of *Machairodus latidens*. MacEnery states that the total number of teeth he found were five upper canines and one incisor, and the six museums in which they are now lodged are well known.

A considerable amount of scepticism existed for many years in some minds as to whether the relics just mentioned were really found in Kent's Cavern, it being contended that from its zoological affinities *Machairodus latidens* must have belonged to an earlier fauna than that represented by the ordinary cave mammals; and various hypotheses were invented to explain away the difficulty, most of them, at least, being more ingenious than ingenuous. Be this as it may, it was naturally hoped that the re-exploration of the cavern would set the question at rest for ever; and it was not without a feeling of disappointment that I had to write seven successive annual reports without being able to announce the discovery of a single relic of *Machairodus*. Indeed, the greater part of the Eighth Report was written with no better prospect; when, while engaged in washing a "find" met with on July 29, 1872, I found that it consisted of a well-marked incisor of *Machairodus latidens*, with a left ramus of lower jaw of bear, in which was one molar tooth. They were lying together in the first or uppermost foot-level of cave earth, having over it a continuous sheet of granular stalagmite 2·5 feet thick. There was no longer any doubt of MacEnery's accuracy; no doubt that *Machairodus latidens* was a member of the cave earth fauna, whatever the zoological affinities might say to the contrary; nor was there any doubt that man and *Machairodus* were contemporaries in Devonshire.

I cannot pass from this case without directing attention to its bearing on negative evidence: had the exploration ceased on July 28, 1872—the day before the discovery—those who had always declined to believe that *Machairodus* had ever been found in the cavern would have been able to urge, as an additional and apparently conclusive argument, that the consecutive, systematic, and careful daily labour of seven years and four months had failed to show that their scepticism was unwarranted. Nay, more, had the incisor been overlooked—and, being but a small object, this might very easily have occurred—they might finally have said "15·25 years' labour"; for, so far as is known, no other relic of the species was met with during the entire investigation. In all probability had either of these by no means improbable hypotheses occurred, geologists and palæontologists generally would have joined the sceptics; MacEnery's reputation would have been held in very light esteem; and—to say the least—his researches regarded with suspicion.

When their exploration began, and for some time after, the Committee had no reason to believe or to suspect that the cavern contained anything older than the cave earth; but at the end of five months, facts, pointing apparently to earlier deposits, began to present themselves; and, at intervals more or less protracted, additional phenomena, requiring apparently the same interpretation, were observed and recorded; but it was not until the end of three full years that a vertical section was cut, showing, in undisturbed and clear succession, not only the cave earth with the granular stalagmite lying on it, but, under and supporting the cave earth, another, thicker, and continuous sheet of stalagmite—appropriately termed crystalline, and below this again an older detrital accumulation, known as the breccia, made up of materials utterly unlike those of the cave earth.

The breccia was just as rich as the cave earth in osseous remains; but the lists of species represented by the two deposits were very different. It will be sufficient to state here that, while remains of the hyæna prevailed numerically very far above those of any other mammal in the cave earth, and while his presence there was also attested by his teeth-marks on a vast number of bones, by lower jaws—including those of his own kith and kin—of which he had eaten off the lower borders as well as the condyles, by long bones broken obliquely just as hyænas of the present day break them, and by surprising quantities of his coprolites, there was not a single indication of any kind of his presence in the breccia, where the crowd of bones and teeth belonged almost entirely to bears.

No trace of the existence of man was found in the breccia until March, 1869, that is about twelve months after the discovery of the deposit itself, when a flint flake was met with in the third foot-level, and was believed to be not only a tool, but to bear evidence of having been used as such (see *Report Brit. Assoc.* 1869, pp. 201, 202). Two massive flint implements were discovered in the same deposit in May, 1872, and at various subsequent times other tools were found, until at the close of the exploration the breccia had yielded upwards of seventy implements of flint and chert.

While all the stone tools of both the cave earth and the breccia were Palæolithic and were found inosculating with remains of extinct mammals, a mere inspection shows that they belong to two distinct categories. Those found in the breccia—that is, the more ancient series—were formed by chipping a flint nodule or pebble into a tool, while those from the cave earth—the less ancient series—were fashioned by first detaching a suitable flake from the nodule or pebble, and then trimming the flake—not the nodule—into a tool.

It must be unnecessary to say that the making of nodule tools necessitated the production of flakes and chips, some of which were no doubt utilised. Such flakes, however, must be regarded as accidents, and not the final objects the workers had in view.

It is worthy of remark that in one part of the cavern, upwards of 130 feet in length, the excavation was carried to a depth of nine feet, instead of the usual four feet, below the bottom of the stalagmite; and that, while no bone of any kind occurred in the breccia below the seventh foot-level, three fine flint nodule tools were found in the eighth, and several flint chips in the ninth, or lowest foot-level.

It may be added that the same fact presented itself in the lowest or corresponding bed in Brixham Windmill Hill Cavern. In short, in each of the two famous Devonshire caverns, the archæological zone reached a lower level than the palæontological.

That the breccia is of higher antiquity than the cave earth is proved by the unquestionable evidence of clear undisturbed superposition; that they represent two distinct chapters and eras in the cavern history is shown by the decided dissimilarity of the materials composing them, the marked difference in the osseous remains they contained, and the strongly contrasted characters of the stone implements they yielded; and that they were separated by a wide interval of time may be safely inferred from the thickness of the bed of stalagmite between them.

It is probable, however, that the fact most significant of time and physical change is the presence of the hyæna in the cave earth or less ancient, but not in the breccia or more ancient, of the two deposits. I called attention to this fact in a paper read to this Department ten years ago (see *Report Brit. Assoc.* 1873, pp. 209–214), and at greater length elsewhere in 1875 (see *Trans. Plym. Inst.* v. 360–375). Bearing in mind the cave-haunting

habits of the hyæna, the great preponderance of his remains in the cave earth, and their absence in the breccia, it seems impossible to avoid the conclusion that he was not an occupant of Britain during the earlier period.

The acceptance of this conclusion, however, necessitates the belief (1) that man was resident in Britain long before the hyæna was.

(2) That it was possible for the hyæna to reach Britain between the deposition of the breccia and the deposition of the cave earth. In other words, that Britain was a part of the Continent during this interval.

Sir C. Lyell, it will be remembered, recognised the following geographical changes within the British area between the Newer Pliocene and historical times (see "Antiquity of Man," edition 1873, pp. 331, 332).

Firstly, a pre-Glacial Continental period, towards the close of which the Forest of Cromer flourished, and the climate was somewhat milder than at present.

Secondly, a period of submergence, when the land north of the Thames and Bristol Channel, and that of Ireland, was reduced to an archipelago. This was a part of the Glacial age, and icebergs floated in our waters.

Thirdly, a second Continental period, when there were glaciers in the higher mountains of Scotland and Wales.

Fourthly, the breaking up of the land through submergence, and a gradual change of temperature, resulting in the present geographical and climatal conditions.

It is obvious that if, as I venture to think, the Kent's Cavern breccia was deposited during the first Continental period, the list of mammalian remains found in it should not clash with the list of such remains from the Forest of Cromer, which, as we have just seen, flourished at that time. I called attention to these lists in 1874, pointing out that according to Prof. Boyd Dawkins ("Cave-Hunting," p. 418) the forest bed had at that time yielded twenty-six species of mammals, sixteen of them being extinct, and ten recent; that both the breccia and the forest bed had yielded remains of the cave-bear, but that in neither of them had any relic or trace of hyæna been found. A monograph on the "Vertebrata of the Forest Bed Series" was published in 1882 by Mr. E. T. Newton, F.G.S., who, including many additional species found somewhat recently, but eliminating all those about which there was any uncertainty, said: "We still have forty nine species left, of which thirty are still living, and nineteen are extinct" (p. 135). Though the number of the species has thus been almost doubled, and the presence of the cave-bear remains undoubted, it continues to be the fact that no trace of the hyæna has been found in the forest bed, and no suspicion exists as to his probable presence amongst the eliminated uncertain species.

It should be added that no relic or indication of hyæna was met with in the "Fourth Bed" of Brixham Windmill Hill Cavern, believed to be the equivalent of the Kent's Hole breccia.

I am not unmindful of the fact that my evidence is negative only, and that raising a structure on it may be building on a sandy foundation. Nevertheless, it appears to me, as it did ten years ago, strong enough to bear the following inferences:—

1. That the hyæna did not reach Britain until its last Continental period.

2. That the men who made the Palæolithic nodule-tools found in the oldest known deposit in Kent's Cavern arrived during the previous great submergence, or, what is more probable—indeed, what alone seems possible unless they were navigators—during the first Continental period. In short, I have little or no doubt that the earliest Devonians we have sighted were either of Glacial, or, more probably, of pre-Glacial age.

It cannot be necessary to add that while the discovery of remains of hyæna in the forest bed of Cromer, or any other contemporary deposit, would be utterly fatal to my argument, it would leave intact all other evidence in support of the doctrine of British Glacial or pre-Glacial man.

Some of my friends accepted the foregoing inferences in 1873, while others, whose judgment I value, declined them. Since that date no adverse fact or thought has presented itself to me; but through the researches and discoveries of others in comparatively distant parts of our island, and especially in East Anglia, the belief in British pre-Glacial man appears to have risen above the stage of ridicule, and to have a decided prospect of general scientific acceptance at no distant time.

I must, before closing, devote a few words to a class of workers who are "more plague than profit."

The exuberant enthusiasm of some would-be pioneers in the question of human antiquity results occasionally in supposed "discoveries" having an amusing side; and not unfrequently some of the pioneers, though utter strangers, are so good as to send me descriptions of their "finds," and of their views respecting them. The following case may be taken as a sample:—In 1881, a gentleman, of whom I had never heard, wrote, stating that he was one of those who felt deeply interested in the antiquity of man, and that he had read all the books he could command on the subject. He was aware that it had been said by one palæontologist to be "unreasonable to suppose that man had lived during the Eocene and Miocene periods," but he had an indistinct recollection that another eminent man had somewhere said that "man had probably existed in England during a tropical Carboniferous flora and fauna." He then went on to say, "I have got that which I cannot but look upon as a fossil human skull. I have endeavoured to examine it from every conceivable standpoint, and it seems to stand the test. The angles seem perfect, the contour the same but smaller in size than the average human head; but that, in my opinion, is only what should be expected if we assume that man lived during the Carboniferous period, in spite of what Herodotus says about the body of Orestes." Finally, he requested to be allowed to send me the specimen. On its arrival it proved, of course, to be merely a stone; and nothing but a strong "unscientific use of the imagination" could lead any one to believe that it had ever been a skull, human or infrahuman.

It may be added that a few years ago a gentleman brought me what he called, and believed to be, "three human skulls and as many elephants' teeth," found from time to time, during his researches in a limestone quarry. They proved to be nothing more than six oddly shaped lumps of Devonian limestone.

So far as Britain is concerned, cave-hunting is a science of Devonshire birth. The limestone caverns of Oreston, near Plymouth, were examined with some care in the interests of palæontology as early as 1816, and subsequently as they were successively discovered. The two most famous caverns of the same county—one on the northern, the other on the southern shore of Torbay—have been anthropological as well as palæontological studies; and, as we have seen, have had the lion's share in enlarging our estimate of human antiquity. The researches have, no doubt, absorbed a great amount of time and of labour, and demanded the exercise of much care and patience; but they have been replete with interest of a high order, which would be greatly enhanced if I could feel sure that your time has not been wasted nor your patience exhausted in listening to this address respecting them.

JOSEPH-ANTOINE-FERDINAND PLATEAU.

THE career of this indefatigable investigator, as we announced last week, has just closed. Born in the second year of the present century, he has occupied a notable position in the scientific world for more than fifty years. Before he reached middle age he met with the terrible misfortune of losing his eyesight while trying venturesome experiments on the physiological effects of light. His scientific career seems to have become only more active in consequence! When we think of the ease and success with which certain chess-players can, blind-fold, carry on some dozen or two simultaneous games, there seems little to surprise us in the mathematical career of Euler after he became blind. But the difficulties which stood in the way of the physicist, and which he successfully overcame, were of a far more formidable character. Had his chief investigations related to sound, the loss of eyesight might have but little interfered with them. But to carry out by the help of others' eyes a long series of investigations connected with Physiological Optics was a triumphal feat with which we know nothing to compare, except, perhaps, the composition of those marvellous master-works which Beethoven elaborated after he had become stone deaf.

Plateau's really great contributions to physical science were, however, not optical, but molecular. They were collectively republished in 1873 in two volumes, with the title, *Statique expérimentale et théorique des Liquides soumis aux seules Forces moléculaires.* This work was

appreciatively reviewed in our columns (vol. x., p. 119) by the then greatest authority on the subject, the late Prof. Clerk Maxwell, so that it is unnecessary for us to analyse it here. Few of the readers of the recently published biography of Maxwell can have forgotten the humorous but accurately expressive lines in which he alludes to this work:—

"And just as that living Plato, whom foreigners nickname Plateau,
Drops oil in his whiskey and water—for foreigners sweeten it so:—
Each drop keeps apart from the other, inclosed in a flexible skin,
Till touched by the gentle emotion evolved by the prick of a pin," &c.

When we look at the Royal Society's Catalogue, we find that up to 1873 Plateau is credited with fifty-three papers on subjects of the most varied character. One large section of these, of course, forms the matter of the volumes already mentioned. Another large section is devoted to the persistence of visual impressions, subjective impressions of colour, irradiation, and other questions of physiological optics. In connection with these, there are several controversies and reclamations, with and against authorities such as Chevreul and von Helmholtz. In these contests, it must be confessed that Plateau usually has the worse. In fact, he appears very much in the same light as did Brewster a little earlier. He furnished to others, who knew how to interpret and to use them, a great array of novel facts: but his strength lay mainly in the patience and ingenuity which led him to these facts; not in the power of interpreting, explaining, or generalising them.

Besides the two main subjects above mentioned, we find in Plateau's *répertoire* a number of curiosities taken from widely different branches of science. Thus we have a chemical analysis of the mineral waters of Spa; the geometrical problem of describing an equilateral triangle whose several corners shall be on three given circles in one plane; arithmetical recreations; photometry; the "ghosts" produced by various series of rotating spokes; and a centrifugal air-pump.

Plateau occupied with success, until practically disabled, the Chair of Physics in the University of Ghent; and, if he did not attain to the foremost rank among experimental physicists, he at least did much good and useful work under circumstances which would have effectually closed the career of many men who have been more successful than he. He was occupied in his later years in compiling a valuable catalogue of all the papers he could meet with which bore on his special optical inquiries. It is to be hoped that the as yet unpublished part of this collection has been left in a state approaching completion.

OFFICIAL REPORTS ON CHOLERA IN EGYPT

SURGEON-GENERAL HUNTER, who was commissioned by the Government to make inquiry as to the circumstances attending the cholera epidemic in Egypt, has sent two reports to the Foreign Office. Neither pretends to afford full information on the subject which has been under investigation, but the more recent one, which gives information up to August 19, supplies some indication as to the opinion Dr. Hunter has formed with regard to the etiology of the epidemic. In his first report Dr. Hunter gives the cholera deaths registered up to July 31 as 12,600, but he adds that, owing to defective registration, the total mortality will probably be found to have been nearly double that number. Since that date some 15,000 more deaths have been registered, and if the same faulty system of registration has been maintained, the total mortality up to the present date cannot have fallen far short of some 55,000. The inquiry undertaken by Dr. Hunter relates therefore to a matter of the greatest magnitude, the more so as Egypt has apparently been free from cholera ever since 1865. It is however precisely this question of immunity from cholera that will be raised by Dr. Hunter, and already we are able to gather what opinion will be expressed on this point.

Thus, the possibility of the importation of the disease into Egypt from India is discussed, and it is stated that even some of those who originally were firmly convinced of this method of origin have been forced to a different conclusion. The spontaneous origin of the contagium is also regarded as not being supported by facts; and Dr. Sierra, in a communication which is appended to Dr. Hunter's, distinctly asserts that such a generation of the infection in the Nile Delta cannot be regarded as proved merely because the choleraic germ is often produced at the mouth of the Ganges. Prominence is, however, given to the fact that Egypt has been visited by five epidemics since that of 1831, namely, in 1848, 1850, 1855, 1865, and 1883, and independent testimony is brought forward to show that during the early part of the present year, as also at occasional intervals since 1865, there have been cases of a disease known as "cholerine," which have been characterised by some of the symptoms of true cholera. And further, Dr. Hunter, in expressing an opinion as to these cases, says that he has arrived at the conclusion that many of them were "what in India we should call cholera."

A further step in the argument is embodied in a description of the filthy conditions under which the Egyptians live, and especially of the foul state of the Nile at Damietta and other places, both owing to the floating carcasses of animals who had died of bovine typhus and otherwise. Having regard to all these points, the report implies that a number of cases, which for the moment we may describe as sporadic cholera, have formed a somewhat continuous series of attacks ever since the 1865 outbreak, and that the potency of the infection for spread in an epidemic form was developed under the influence of the foul conditions which obtained immediately antecedent to the date of the last epidemic. This view is by no means a new one; it was specially dealt with in a series of papers which were brought before the Epidemiological Society in 1878, when the possibility of a "progressive development of the property of infectiveness" under favourable conditions was insisted on; and it is more than probable that, as regards some of the infectious diseases, it may turn out to be a true explanation of their origin.

It must, however, be borne in mind that in England, and indeed in all thickly peopled countries, cases which are clinically of a similar character constantly occur during the warmer months of the year; indeed, the term "English cholera" is of by no means infrequent occurrence in our mortality tables. And not only so, but Dr. Sierra, in arguing against the spontaneous development of the contagium under the conditions which were found at Damietta, says that the same "cosmo-telluric conditions" have appeared often enough at the mouth of the Nile, that the same accumulation of carcasses in the river has before now taken place, and yet that no cholera has broken out in Egypt. The evidence mainly needed with a view to support the theory which is foreshadowed in Dr. Hunter's reports, should go to point out what were the peculiar conditions which, during the past summer, led to the development of a special potency for mischief in a disease which is always more or less present. The subject is one of the greatest scientific interest, and we trust it will be fully dealt with in the final report.

NORDENSKJÖLD'S GREENLAND EXPEDITION

BARON NORDENSKJÖLD telegraphed as follows to the *Times* from Thurso on Friday night:—"An inland ice party started on July 4 from Auleitswik Fjord. When they were 140 kilometres east of the glacier border and 5000 feet above the sea level they were prevented by soft snow from proceeding with sledges. They sent the Laplanders further on snowshoes. These advanced 230

kilometres eastwards over a continual snow desert to a height of 7000 feet. The conditions for a snow-free interior consequently did not exist here; but this expedition, during which men have reached for the first time the interior of Greenland, has given important results as to the nature of the interior of an ice-covered continent. Over the whole inland there is ice. There occur masses of fine dust, partly of cosmical origin, with the ice. The rest of the expedition, under the command of Dr. Nathorst, visited the north-western coast between Waigattel (?) and Cape York. The Esquimaux told our Esquimaux interpreter (Hans Christian, formerly of Capt. Hall's expedition) that two members of the American Polar Expedition had died, and the rest had returned to Littleton Island (Sofia). On August 16 the expedition sailed from Egedesmunde for the south, with rich collections, zoological, botanical, and geological. Short stays were made at Iviktit, Julianshaab, and Frederiksdal. We tried to proceed eastwards thrice through the sounds north of Cape Farewell and once along the coast, but were hindered by ice. We then went outside the ice field to 66° latitude, remaining constantly in sight of land, having twice in vain tried to find an ice-free shore more to the south. The band of drift ice was forced south of Cape Dan. On September 4 we anchored in a fjord which had been newly visited by Esquimaux, and where we found some remains from the Norse period. It was the first time since the fifteenth century that a vessel had succeeded in anchoring on the east coast of Greenland south of the Polar Circle. We tried in vain to anchor in another fjord more to the north, and returned. The expedition arrived at Reikiavik (Iceland) on September 9. Our observations on the temperature of the sea prove that the cold current which packs the ice along the east coast of Greenland is very insignificant; that the glaciers of the east coast are few and of no great size; and that the fjords are free from ice. Probably the coast may be reached by suitable steamers in the autumn of most years."

It will thus be seen that for once Baron Nordenskjöld has failed to fulfil his predictions. But his expedition must be regarded as in all respects successful. He has succeeded in penetrating into the very heart of Greenland, and the idea of taking Lapps with him to skate their way over the rough ice-bound land was a happy one. Greenland thus appears to be what has always been conjectured, a land everywhere covered with a thick ice-sheet. We cannot gather from the telegram whether Nordenskjöld's theory as to the position of the old Norse settlements has been confirmed, but he has, at all events, succeeded in bringing back remains of the old colonies. The analysis of the cosmical dust which has been collected will be eagerly looked for, and the detailed account of the collections made in Northern Greenland.

NOTES

Mr. J. Y. Buchanan has been invited to accompany the expedition which sailed last week from the Thames to survey the route and lay the cables connecting Cadiz and the Canary Islands, and these islands with Senegal, on the west coast of Africa. During the laying of the cable from Lisbon to Madeira, over a route that had been carefully sounded, into what was believed to be close on 2000 fathoms, it suddenly parted. Soundings taken immediately revealed the existence of a bank with no more than 110 fathoms of water on it, which had been missed while surveying the route. Again, quite recently—indeed, during the last two weeks—the French exploring vessel *Talisman*, which has been investigating this part of the ocean with a numerous scientific staff, under the direction of the veteran Milne-Edwards, discovered another bank to the southward of the "Seine Bank," with as little as 70 fathoms on it. This bank was found to be about thirty miles long from east to west, and six miles broad from north to south. Apart from the special investigation of these banks, the survey of the line o route, which is carried out by two ships working in concert along a zigzag course, sounding every seven miles, must necessarily furnish much important information. Between Madeira and the Canary Islands lies the small group of the Salvage Islands, which may be said to be almost unknown. It is intended to carry the soundings round them, so as to determine whether they are connected with any of the new banks or with known land. It is also intended to land on the islands, from which interesting collections may be expected. In addition to the instruments ordinarily carried in the ships, Mr. Buchanan takes out a new sounding tube, constructed for use with the ordinary wire sounding apparatus. With it it will be possible at every station to secure a good sample of the mud and of the water from the bottom without altering the routine work of the ship. As the route crosses the mouth of the Mediterranean it will thus be possible to determine the extent to which the dense warm water which leaves that sea as a bottom current affects the density and temperature of the deep water of the North Atlantic in its neighbourhood. The ships to be used are the *Dacia* and the *International*, both belonging to the Telegraph Construction and Maritime Company.

In a communication to the *Sonntags-Beilage zur Norddeutschen Allgemeinen Zeitung* for September 16 Dr. Reichenow, the well-known ornithologist of the Berlin Museum, describes a new ostrich under the name of *Struthio Molybdophanes*. A living example is in the Zoological Gardens at Berlin, and others are expected at Cologne and Paris. The habitat of this species is stated to be the deserts of Somali Land and the Western Galla country, extending on the east coast of Africa from 10° N. lat. to the Equator.

The United States steamer *Yantic* has, we regret to learn, failed to reach and rescue Lieut. Greeley and his observing party, who have been stationed at Lady Franklin Bay, in Kennedy Channel, about 81° N., since the summer of 1881. This at first sight looks as if there were little hope of the safety of the party, as they had only two years' provisions with them. But Sir George Nares, who knows the region intimately, writes to the *Times* to show that there is no reason for despair. He gives in detail his reasons for believing that Lieut. Greeley, when the relief ship failed to reach him in 1882, would, like a prudent leader, prepare for the worst and husband his provisions to the utmost. Moreover, he would probably be able to add considerably to his supply by hunting, and on the route southwards there are depots at various accessible points. So, even if another year should have to be spent in the north, there is good reason to hope for the ultimate safety of the party.

According to the *Izvestia* of the Russian Geographical Society, the young Tashkend Observatory carries on very useful scientific work. Col. Pomerantseff and his assistants are not only engaged in the verification of those instruments which are used every year for determinations of latitudes and longitudes in Turkestan, and in the computation of the results; they also pursue independent work, such as the observation of the small planets Juno and Pallas; the observation of the last solar eclipse at Penjakent; of the stars eclipsed by the moon, which are given in the *Nautical Almanac*; magnetical and meteorological observations. These last were made in 1882, at twenty stations, out of which eight are first-rank stations, and that of Tashkend makes observations every hour.

The prize of 500 francs presented by Prof. A. P. de Candolle for the best monograph on a genus or family of plants is announced as open for competition for 1884. Papers in Latin, French, German, English, or Italian should be sent to Prof. Alph. de Candolle, Geneva, before October 1, 1884.

THE *China Mail*, in referring to the Hong Kong Observatory, says that Dr. Doberck will first be instructed to draw up a report for His Excellency the Governor, on the minor stations now in existence. He will examine past records, and, if these are found fairly accurate, will endeavour to furnish certain data as to the climatic conditions prevalent throughout the colony during the different months of the year. When this is done, it will probably be found feasible to make these stations cooperate with the central station at Kowloon, especially in observations connected with typhoons. Another important item will be the determination of the magnetic elements, and the investigation of the magnetic attraction of the various mountains and hills in the colony and its neighbourhood. It is also possible that, under instructions from the Governor, Dr. Doberck will proceed to Manilla, Shanghai, and other places on the coast of China, to inspect the observatories there, and put himself into communication with the directors of those institutions, with the view of having their reports sent regularly to the Hong Kong Observatory to receive careful discussion here with the object of eventually furnishing trustworthy weather forecasts.

MESSRS. ALLEN, COUES, AND BREWSTER, according to *Science*, sign a call for a convention of American ornithologists, to be held in New York City, beginning on September 26, 1883, for the purpose of founding an American Ornithologists' Union, upon a basis similar to that of the "British Ornithologists' Union." The object of the Union will be the promotion of social and scientific intercourse between American ornithologists and their co-operation in whatever may tend to the advancement of ornithology in North America. A special object, which it is expected will at once engage the attention of the Union, will be the revision of the current lists of North American birds, to the end of adopting a uniform system of classification and nomenclature, based on the views of a majority of the Union, and carrying the authority of the Union. It is proposed to hold meetings at least annually, at such times and places as may be hereafter determined, for the reading of papers, and the discussion of such matters as may be brought before the Union. Those who attend the first meeting will be considered *ipso facto* founders. Active and corresponding members may be elected in due course after organisation of the Union, under such rules as may be established for increase of membership. Details of organisation will be considered at the first meeting.

AN enthusiastic meeting of 3000 working men was held in Nottingham recently, at which resolutions were passed maintaining the great importance of sound technical instruction for the manufactures of the country. In connection therewith we may say that it is expected that the technical schools which are attached to the University College, Nottingham, will be opened some time in October next. It is intended in these schools to provide a complete course of instruction in mechanical and electrical engineering, and in the sciences most intimately connected with these professions; also to give instruction to artisan classes in mechanics, and in the details and history of the machinery employed in the lace and hosiery manufactures. The students attending the school will be divided into day and evening classes. It is expected that the day students will consist of young men who intend taking up engineering as a profession, or, being the sons of manufacturers, and looking forward to the management of a manufacturing business, consider it desirable to gain some knowledge of the construction of machinery. For these students the College provides chemical and physical laboratories, and lecture theatres, and class rooms for drawing, mathematics, theoretical mechanics, &c. The workshops now added will comprise tools and mechanism in all departments of work. The shops will be supplied with steam power, and lighted by the electric light. In the evenings classes will be held for artisans. On these occasions opportunities will be given to engineers' apprentices and others to prepare themselves for the annual competition for Whitworth Scholarships. The Mechanical Museum will form a very important feature in the means of instruction provided for lace-makers and hosiers. In this museum will be exhibited models of all the mechanical movements which are generally recognised, with short printed or written descriptions pointing out the special features of each, and their function in lace and hosiery machinery when so employed. Specimens of lace and hosiery machines which can be set in motion, will also be shown, their moving parts being labelled in such a way as to point out their relation to the models above mentioned. The workshops are under the general direction of Prof. Garnett.

DURING the ensuing winter session of the Liverpool Science and Art Classes there will be conducted by Miss Helen Fryer a class for the study of Hygiene. The lectures will follow the course of the syllabus lately published by the Government Science and Art Department. Miss Fryer will also give a course of lectures on Animal Physiology.

THE Directors of the Crystal Palace have completed arrangements for holding an International Exhibition of Arts, Manufactures, Science, and Industry during 1884. It is intended that the Exhibition should open on April 3 and close at the end of October. All the arrangements will be under the control of Mr. G. C. Levey.

BY the kindness of the Trustees of the Gilchrist Fund, the Committee of the Victoria Coffee Hall have been able to arrange for the delivery of six Penny Science Lectures by eminent lecturers on Tuesdays, beginning on October 2. The Committee are anxious that lectures such as these, which are rarely within the reach of the London working men, should be made widely known beyond the circle of the usual frequenters of the Hall, and the Hon. Secretary would be glad to hear from any one willing to help by getting a poster hung up, or distributing handbills among working men in districts within reach of the Victoria. The following are the lecturers and subjects:—October 2 and 9: Lecture by Mr. Wm Lant Carpenter, F.C.S., on "Ice, Water, and Steam." October 16: Mr. P. H. Carpenter, on "Life under the Ocean Wave." October 23: Mr. E. B. Knobel, Sec.R.A.S., on "Comets." October 30: Mr. C. A. V. Conybeare, on "The Rights and Feelings of an Animal." November 6: Dr. B. W. Richardson, M.D., LL.D., F.R.S., on "Food and Feeding."

WE are glad to see that science has a place in the first number of *The English Illustrated Magazine* (Macmillan and Co.), which contains Prof. Huxley's Royal Institution lecture on the oyster. Mr. Grant Allen contributes an interesting article with some beautiful illustrations on "The Dormouse at Home."

TWO strong shocks of earthquake were felt on Sunday at Casamicciola. A house situated in the upper town was wrecked and fell in ruins. No lives were lost.

CAPT. EDWARD ASHDOWN, Commander of the P. and O. steamer *Siam*, writes as follows to the *Times*:—"It may be interesting to some of your scientific readers to know that the steamship *Siam*, on her voyage from King George's Sound to Colombo, on August 1, when in lat. 6° S., long. 89° E., passed, for upwards of four hours, through large quantities of lava, which extended as far as could be seen (the ship was going 11 knots at the time). The lava was floating in a succession of lanes of from five to ten yards wide, and trending in a direction north-west to south-east. The nearest land was the coast of Sumatra (distant 700 miles), but as there was a current of fifteen to thirty miles a day, setting to the eastward, the lava could not

have come from there, and I can only imagine it must have been an upheaval from somewhere near the spot. I may mention the soundings on the chart show over 2000 fathoms. There was a submarine volcano near the spot in 1789."

EXPERIMENTS on the liquefaction of oxygen and nitrogen are described by Wroblewski and Olszewski (*Compt. Rend.* xcvi. 1140 and 1225). At $-136°$ oxygen liquefies under a pressure of $22\frac{1}{2}$ atmospheres; nitrogen at the same temperature does not liquefy, even under a pressure of 150 atmospheres, but if the pressure is somewhat slowly diminished, care being taken that it does not become less than 50 atmospheres, the nitrogen becomes liquid. Carbon disulphide solidifies at about $-116°$, and alcohol at $-130°.5$.

FROM the annual report on the mineral statistics of Victoria, we see that the quantity of gold raised in 1882 was 864,609 oz., as against 833,378 oz. in 1881. The deepest shaft in the colony is the Magdala, at Slawell, 2409 feet deep.

THE additions to the Zoological Society's Gardens during the past week include a White-fronted Capuchin (*Cebus albifrons* ♂) from Brazil, presented by Capt. Harrison; a Puma (*Felis concolor*) from South America, presented by Mr. B. M. Whithard; a Grey Ichneumon (*Herpestes griseus*) from India, presented by Mr. Murray Dickinson; a Ruddy Ichneumon (*Herpestes smithi*) from South Africa, presented by Col. J. H. Bowker, F.Z.S.; a Fallow Deer (*Cervus dama* ♀), European, presented by Sir Henry Bessemer; a Persian Gazelle (*Gazella subgutterosa* ♂), two Persian Sheep (*Ovis aries*, var. ♂ ♂) from Persia, presented by Lady Brassey; a Grey Seal (*Halichœrus gryphus*) from Wales, presented by Mr. J. J. Dodgshon; two Rufous Tinamous (*Rhynchotus rufescens*) from Uruguay, presented by Mr. J. Brown; a Spanish Terrapin (*Clemmys leprosa*), South European, presented by Mr. Aitchison; a Yellow-billed Sheathbill (*Chionis alba*) from Antarctic America, two Yarrell's Curassows (*Crax carunculata* ♂ ♀) from South-east Brazil, purchased; a Macaque Monkey (*Macacus cynomolgus* ♂) from India, a Greater Sulphur-crested Cockatoo (*Cacatua galerita*) from Australia, two Small Hill Mynahs (*Gracula religiosa*) from Southern India, deposited.

THE IRON AND STEEL INSTITUTE

THE Iron and Steel Institute has this year resolved to revisit the place of its birth—in other words, the young and flourishing town of Middlesbrough-on-Tees, where the association was founded some fifteen years ago. The arrangements for its reception and for visits to different works in the neighbourhood (though marred in practice by a grievous disaster) left nothing to be desired; but the papers, though sufficient in number and value for practical metallurgists, offer very little that is of interest to the student of science generally. Hence our notice will be brief. It is somewhat to be regretted (especially seeing that the Eston Works formed the first day's excursion) that no paper was devoted to the development of the Thomas-Gilchrist or "basic" process of steel-making. This process has been widely and successfully adopted in Germany, but has made little progress as yet in the Cleveland district, for which it may be said to have been specially designed, and where it was first put in practice. On this disappointment, however, it is useless to dwell. Passing over three adjourned discussions—on tin-plate making, coal-washing machinery, and the manufacture of anthracite pig iron respectively—we come to the new papers prepared for the meeting. There were two dealing with the important manufacture of coke: one by Mr. R. Dixon, on the Simon-Carve's process, and one by Mr. Jameson, on the process which bears his name. We hail these as a further assurance that the barbarous, costly, and offensive beehive oven, which still continues to disgrace our English coking districts, is far on the high road to extinction. In Belgium it has altogether ceased to exist, being superseded by more rational methods; and the same will soon be the case for the rest of the Continent. The two papers before us do not, however, contribute very much to our knowledge. Mr. Dixon's deals simply with the cost of erecting ovens on the particular system described, which cost is unfortunately high, and on the yield and quantity of coke produced, which are both satisfactory. Some difficulty is experienced with the bituminous coal of Durham in keeping the valve-boxes and mains free from pitch; but this, it is hoped, will shortly be overcome. He also describes a method just introduced of heating the air required for combustion by the waste gases passing away from the ovens, by which the time needed for coking is expected to be largely reduced. Mr. Jameson's system, as our readers will remember, consists in burning the coal from the top in a closed oven, and withdrawing the gases, as they form, from the bottom, by means of an exhausting apparatus. These waste gases are condensed, and give valuable results in ammonia, tar, &c. The amount of this yield has been largely increased, since former papers were read on the subject, by new extracting and condensing appliances, and the percentage of coke made appears also to have improved. One great advantage of the system is that any beehive oven can be adapted to it at a cost of some 10*l.* or 15*l.* The oils extracted, the value of which had been questioned, find a ready sale at 2*l.* to 3*l.* per ton.

A paper on raw coal in the blast furnace, prepared by Mr. I. Lowthian Bell, F.R.S., was postponed, in consequence of its author's serious illness—an illness from which we are glad to hear that he is recovering. We pass on to a paper by Mr. E. A. Cowper, Past President of the Institute of Mechanical Engineers, on the results obtained with the hot-blast stove which bears his name. This, as is well known, is an application to the blast furnace of the fire-brick "regenerator" invented by Sir Wm. Siemens for gas furnaces. In the earlier days of the hot-blast process, the best known means of heating the air was to pass it through a sort of coil of cast-iron pipes, inclosed within a tall furnace. The limit of endurance with such pipes is, however, reached at about 1000° F.; whereas by employing two inclosed stacks of fire-bricks, one of which is always being heated from below, while the other is being cooled from above by air passing through it to the furnace, temperatures of 1500° are attainable. The advantages of so far increasing the temperature were hotly contested, from a theoretical point of view; but "the proof of the pudding is in the eating," and Mr. Cowper has proved beyond doubt that a blast of 1500°, combined with a very large and slowly-working furnace, will realise an economy (in fuel consumed per ton of iron made) which, in these days of competition, means just the difference between a fair profit and a heavy loss. The chief element of success in these stoves appears to be the making of the bricks as thin as possible, so that there may be but little depth for the heat to soak into or soak out of; and the author describes a form of brick, making what he calls "honeycomb filling," with which there is nowhere a greater thickness than two inches, and this is always heated from both sides.

Two papers on hydraulic cranes for steel works, by Mr. R. M. Daelen and Mr. T. Wrightson, and another by Mr. J. E. Stead, on a new form of gas sampler, do not require any special comment. Finally we have a paper on blast furnace economy in relation to design, by Mr. R. Howson, which is of a somewhat more suggestive character. The almost universal form of the interior of a blast furnace is as follows:—From the throat, where the materials are charged and the gases collected, it widens slowly to a point about two-thirds of the way down, called the boshes. From thence it narrows again, but more rapidly, and ends in a shallow circular pit called the hearth. Mr. Howson asks whether this form has not, from beginning to end, been a "rule-of-thumb business" with English engineers; and whether the rapid narrowing below the boshes does not in fact favour the lodgment of half-melted cinder, and the consequent building up of "scaffolds," which are known to be the most serious of all impediments to the successful working of a blast furnace. It is supposed that the hearth needs "relief from pressure;" but as a matter of fact the difficulty is to get the materials down quickly enough, and the easier their descent is made the better. He proposes a barrel-shaped form, having a regular curve at the boshes, instead of a sharp angle—a form actually adopted by the late Mr. Menelaus at Treforest, and with great success as to economy of fuel. With the same object he advocates the charging of the coke towards the sides of the furnace, and the stone towards the middle, and the preserving of this distribution throughout, so as to have as much combustible material as possible above and near to the tuyeres.

UNIVERSITY AND EDUCATIONAL INTELLIGENCE

UNIVERSITY COLLEGE, LONDON.—The Department of Applied Science and Technology in this college opens on October 2, along with the rest of the college. The instruction in this department includes (1) lectures on different branches of civil and mechanical engineering and surveying and levelling, drawing and practical experimental work in the engineering laboratory; (2) lectures and practical laboratory work in electricity and allied branches of physics; (3) lectures in architecture and architectural construction; and (4) lectures and practical laboratory work in different branches of chemical technology, including brewing, heating and lighting, metallurgy, chemistry of the alkali trade, and agricultural chemistry. Besides these technical and professional lectures, the Faculty of Science provides very complete courses of lectures in mathematics, physics, chemistry, and geology, the sciences upon which the professional knowledge must be based.

ON Tuesday last Mr. F. J. M. Page, B.Sc., F.C.S., was elected Demonstrator of Practical Chemistry at the London Hospital Medical College.

SCIENTIFIC SERIALS

THE *Journal of Anatomy and Physiology*, vol. xvii. part 4, July, 1883, contains: On the action of saline cathartics, by Dr. Matthew Hay.—On the anatomy and physiology of the urinary bladder and of the sphincters of the rectum, by F. Le Gros Clark, F.R.S.—On ten cases of congenital contraction of the stomach, with remarks, by W. Roger Williams (plate 17).—A new rule of epiphyses of long bones, and on the ossification of the temporal bone, by J. B. Sutton (plate 18).—On three cases of cerebellar disease, by Dr. Thomas Oliver.—A contribution to the anatomy of the Indian elephant, by Dr. R. J. Anderson.—On a case of semi-agnatha or synotia in a lamb, by Frederic Eve.—On a case of primary epithelioma of the lung with secondary deposits in the kidney, vertebræ, and ribs, by W. E. Hoyle, M.A. (plate 19).—Researches into the histology of the central gray substance of the spinal cord and medulla oblongata, by Dr. W. A. Hollis (plate 20).—On the membrana tympani, by Dr. J. M. Crombie.—An account of an obturator hernia, and of a fibrous body attached to the hydatid of Morgagni, by W. S. Richmond.

THE *Quarterly Journal of Microscopical Science* for July contains:—On the ancestral form of the Chordata, by Prof. W. Hubrecht (plate 23).—On the renal organs of Patella, by J. T. Cunningham.—On a rare form of the blastoderm of the chick, and its bearing on the question of the formation of the vertebrate embryo, by Dr. C. O. Whitman (plates 24 and 25).—On the development of the pelvic girdle and skeleton of the hind limb in the chick, by Alice Johnson (plates 26 and 27).—On the development of the mole (*Talpa europea*), by Walter Heape (plates 28 to 31).—On the tongue of *Ornithorhynchus paradoxus*: the origin of taste bulbs and the parts upon which they occur, by Edward B. Poulton, M.A. (plate 32).—Observations upon the fœtal membranes of the opossum and other marsupials, by Dr. H. F. Osborn (plate 33).

THE *Journal of the Royal Microscopical Society* for August contains:—On the red mould of barley, by C. G. Matthews (plates 5 and 6).—On the spicules of *Cucumarea hyndmanni*, *C. calcigera*, and two allied forms, by Prof. F. Jeffrey Bell, M.A. (plate 8).—On a method of preserving the freshwater medusa, by Peter Squire (four grains of bichloride of mercury to a pint of distilled water).—The usual summary of current researches and *Proceedings* of the Society.

THE *American Journal of Science*, September.—On the existence in both hemispheres of a dry zone and its cause, by Arnold Guyot. The presence is determined of two nearly rainless belts on both sides of the tropics, extending round the globe, and embracing most of the so-called deserts of both hemispheres. It is argued that the atmospheric currents, which are the great regulators of aqueous precipitation, are the primary cause of these subtropical dry zones.—On the relations of temperature to glaciation, by George F. Becker. Assuming the correctness of the generally received opinion that the sun is a gradually cooling body, it is concluded that the absolute maximum in the development of glaciers is past, and that the Glacial period was not one of general cold, but one of higher mean temperature at sea-level than the present.—Analysis of two varieties of lithiophilite (manganese triphilite) from Tubbs Farms, Maine, and Branchville, Connecticut, by S. L. Penfield.—On the intensity of sound. I. The energy and coefficient of damping of a tuning-fork, by Charles K. Wead.—The decay of rocks geologically considered, by Dr. T. Sterry Hunt. In this comprehensive memoir the author insists (*a*) on the evidence afforded by recent geological studies of the universality and antiquity of subaërial decay both of silicated crystalline rocks and of limestones, and of its great extent in pre-Cambrian times; (*b*) on the preservation of the disintegrated materials *in situ*, wherever they have been protected from denudation by overlying strata, or by their position in places sheltered from erosion, as in the Appalachian and St. Lawrence valleys; (*c*) on the insignificant results of this process of decay since the Glacial period owing to the relatively short duration of that period, and probably also to changed atmospheric conditions in recent times; (*d*) on the fact that the process has furnished the materials both for the clays, sands, and iron-oxides from the beginning of the Palæozoic to the present time, and for the corresponding Eozoic rocks formed from the older feldspath rocks by the partial loss of protoxide bases. The decay of sulphuretted ores in the Eozoic rocks has also given rise to oxidised iron ores and to deposits of rich copper ores in various geological regions; (*e*) that the rounded masses of crystalline rocks left in the process of decay constitute not only the boulders of the drift, but, judging from analogy, the similar masses in conglomerates of various ages from Eozoic times.—On Mr. Glazebrook's paper on the aberration of concave gratings, by H. A. Rowland.—On the stibnite from Japan, by Edward S. Dana. The author fully describes and illustrates the remarkable series of specimens of crystallised stibnite from Mount Kosang in the Island of Shikoku, South Japan, which have recently come into the possession of the Yale Museum.—Notes on the volcanoes of Northern California, Oregon, and Washington Territory, by Arnold Hague and Joseph P. Iddings.—Cassiterite, spodumene, and beryl in the Black Hills, Dakota, by William P. Blake.—Discovery of a new planetoid on the night of August 12, by C. H. F. Peters.

THE *American Naturalist* for June, 1883, contains:—Pearls and pearl fisheries, part i., by W. H. Dall.—Aboriginal quarries: soapstone bowls and the tools used in their manufacture, by J. D. McGuire.—Annelid messmates with a coral, by J. W. Fewkes.—Progress of invertebrate palæontology in the United States for the year 1882, by Dr. C. A. White.—Notes on the genus Campeloma of Rafinesque, by R. E. Call.—Mosses, by W. W. Bailey.—Emotional expression, by A. T. Bruce.—The developmental significance of human physiognomy, by C. D. Cope.

July, 1883, contains:—The Naturalist Brazilian expedition, No. 2: the lower Jacuby and Sao Jeronymo, by H. S. Smith.—Growth and development, by C. Morris.—Pearls and pearl fisheries, part 2, by W. H. Dall.—Catlinite: its antiquity as a material for tobacco pipes, by E. A. Barber.

August, 1883, contains:—Means of plant dispersion, by E. J. Hill.—On the classification of the Linnean orders of Orthoptera and Neuroptera, by A. S. Packard, jun.—On the power of scent in the turkey vulture, by S. N. Rhoads.—The Siphonophores (illustrated), by T. Walter Fewkes.

Annalen der Physik und Chemie, July 15.—Theory of dispersion, by L. Lorenz.—On the elliptical polarisation by reflection from the surfaces of transparent bodies, by A. C. van Ryn van Alkemade.—The coefficient of refraction of some mixtures of alcohol and aniline, by W. Johst (with tables).—Remarks on E. Lommel's treatise "Concerning Newton's Rings," by Karl Exner.—On a method of comparing electrical resistances independent of the resistance of the leads, by F. Kohlrausch.—Some determinations of the absolute resistance of a chain by means of an earth inductor and a galvanometer.—Concerning the effect of polarisation with alternating currents, by A. Winkelman.—Quantitative determination of the influence of the changes of temperature produced by extension upon the measurement of the former, by Dr. A. Miller of München.—On the admissability of the acceptance of an electric sun potential and the effect of its interpretation on terrestrial phenomena, by Werner Siemens.—Researches in gaseous constitution of heavenly bodies, by A. Ritter of Aachen.—On the reduction of the fundamental units of mechanics to their elements, by E. Budde.—On a new fluid of high specific weight, of high refractive index and great dispersion, by Carl Rohrbach (with tables).—On the correct writing of some expressions of Arabic origin used in the art of measuring, by K. Zöppritz.

Bulletin of the Belgian Royal Academy of Sciences, July 27.—On the influence exercised by the respiratory process on the

circulation of the blood, by Messrs. Em. Legros and Griffé. From experiments made on the dog, cat, horse, pig, sheep, rabbit, and other animals, Magendie's dictum that pressure is diminished during inspiration and increased during expiration appears to be normally true in the case of the pig alone.—On the existence and cause of a monthly periodicity of the aurora borealis, by M. Terby. The paper is accompanied by a table of magnetic disturbances at Brussels during the years 1870-82 arranged in monthly decades. The existence of a monthly periodicity is demonstrated, and from a series of remarkable coincidences it is suggested that in this periodicity is reflected the duration of the rotation of the sun round its axis. It is further argued that the magnetic perturbations accompanying the aurora borealis, which are closely associated with the appearance of solar spots, are probably subject to the same vicissitudes as the auroras, and to the same periodicity.—Two memoirs on steam-engines, locomotives, breaks, and railway rolling stock, by M. Delacy.—Remarks on the force of the word *discovery* as applied to the Iguanodons of Bernissart, by M. P. J. van Beneden. The discovery of the large specimen recently exposed to public view in the court of the Brussels Natural History Museum, a full account of which appeared in NATURE, September 6 (p. 439), is referred to M. Fagès. But M. van Beneden shows that he was the first to determine the connection of these gigantic fossils with the Iguanodon family.—On some remains of fossil Cetacea collected in the phosphorated rocks between the Elbe and Weser, by M. P. J. van Beneden.—The following theorem is communicated by M. Catalan: a, x, y being integers, every value of x satisfying the equation $(a^2 + 1) x^2 = y^2 + 1$, is the sum of three positive squares, with the exception of $x_1 = 1$ and $x_2 = 4a^2 + 1$.—On some autographs of Grétry, the famous composer of Liége, by M. Ed. Fetis.—On some desiderata in the history of art in Belgium, by M. Ed. Mailly.

Archives of Physical and Natural Sciences, Geneva, August 15. —On some remarkable movements occasionally accompanying the fall of hailstones, by M. Daniel Colladon.—Memoir on earthquakes and volcanoes, by Prof. F. Cordenons. In this first part of a comprehensive study of underground phenomena the author gives a general classification of seismic disturbances, and examines the various hypotheses hitherto proposed to account for them.—On the nomenclature of fossils in connection with the recent discussions on botanic nomenclature, by M. Alph. de Candolle.—On the American ants (concluded), by M. H. de Saussure.—On the movements of the ground recorded at the Neuchatel Observatory, by Dr. Hirsch.—Meteorological observations with tables of temperature and barometric pressure made at the Observatory of Geneva and on the Great Saint Bernard during the month of July.

Rendiconti of the Reale Istituto Lombardo di Scienze e Lettere, July 26, 1883.—Experimental studies on the parasite of tuberculosis (Robert Koch's *bacillus*), by Prof. G. Sormani and Dr. E. Brugnatelli. The conclusions of Charnley Smith (*Brit. Med. Jour.*, January, 1883) regarding the detection of the bacilli of tubercle in the breath of consumptive patients are not confirmed. Hence consumption would not appear to be infectious.—Cure of pneumonitis effected by the cold water method of treatment, by Prof. C. Golgi.—On the quaternary vegetable fossils recently discovered by G. B. Dell' Angelo in the Re district, Val Vegezzo, by Prof. F. Sordelli.—Remarks on the various methods of distributing the current to a system of electric lamps, by Prof. R. Ferrini.—On the Institution of International Law and its operations during the years 1879-83, by C. C. Norsa.—Meteorological tables for the month of July prepared at the Royal Brera Observatory, Milan.

SOCIETIES AND ACADEMIES

London

Royal Society, June 21.—"Contributions to our Knowledge of the Connection between Chemical Constitution, Physiological Action, and Antagonism." By T. Lauder Brunton, M.D., F.R.S., and J. Theodore Cash, M.D.

In this paper the authors show that the physiological action of salts of ammonia varies considerably according to the acid with which the ammonia is combined. They all affect the spinal cord, motor nerves, and muscles, and tend finally to paralyse these structures. The course of poisoning varies: the chloride has at first a stimulant action on the cord while with the iodide this is less marked, and the paralysing action is more distinct. The iodide, sulphate, and phosphate paralyse motor nerves more powerfully than other salts, the iodide being the most powerful of all.

Nineteen salts of the compound ammonias were investigated. They affect the spinal cord, motor nerves, and muscles.

There is a marked difference in action between ammonia and the compound ammonias; while ammonia causes well marked tetanus, compound ammonias as a rule produce symptoms of motor paralysis, with the exception of those in which only one atom of hydrogen is substituted by an alcohol radical. This paralysis appears to be partly due to their action on the spinal cord and nerve centres, and partly to a curara-like action on the motor nerves.

Some of them apparently increase somewhat the excitability of the spinal cord at first, but this is temporary, and is shown rather by hyperæsthesia or tremor than by convulsion; and tetra-methyl and ethyl-ammonium salts differ from the di- or tri-methyl or ethyl-ammonias in having a much greater tendency to cause convulsions.

The effect of the acid radical on the physiological action is less marked in the case of the compound ammonias than in the salts of ammonia itself. The iodides of the compound ammonias paralyse motor nerves more quickly than either chlorides or sulphates.

Salts of methyl, ethyl, amyl ammonium are more active than the corresponding ones of the di- and tri-compounds, but the tetra-compounds are most active of all.

In the next part of the paper the effect of the salts of alkalies on muscle and nerve are considered. The substances investigated were the chlorides of lithium, sodium, potassium, rubidium, and cæsium. These differ from ammonia in having very little tendency to stimulate the spinal cord, and the chief symptom of poisoning by them is increasing torpor. Slight excitement of reflex action is noted at first in the case of potassium and rubidium.

The motor nerves are not paralysed by cæsium or rubidium, except in very large doses, but the other substances of this group paralyse them to a greater or less extent. Lithium and potassium are the most powerful.

The contractile power of muscle (as shown by the height of curve) is increased by rubidium, ammonium, potassium, and cæsium. It is unaffected by sodium excepting in large doses, and is almost invariably diminished by lithium.

The action of substances belonging to the alkaline earths and earths is discussed in the next section. The substances investigated were the chlorides of calcium, strontium, barium, beryllium, didymium, erbium, and lanthanum. In regard to their action upon the nervous system, these substances fall into two groups: (a) containing beryllium, calcium, strontium, and barium; and (b) containing yttrium, didymium, erbium, and lanthanum. Group a has a tendency to increase reflex action, as evidenced by spasm or tremor. Group b, reflex action in the cord appears to be little affected, but they appear to have a tendency to paralyse motor centres of the brain in the frog. Group a all paralyse motor nerves to some extent. Lanthanum has also a slight paralysing action, but the other members of group b have not, agreeing in this respect with sodium and rubidium, and differing from all the others. The *contracture* produced by barium is enormous, resembling that produced by veratria, as the authors have shown in a former paper. It is like that of veratria diminished by heat, cold and potash, and may be abolished by these agents. It is not so well marked when the drug is injected into the circulation, as when locally applied to the muscle.

The action of some of the more important of those drugs can be graphically represented by a spiral, the terminal members of which are potassium and barium, and these two are to a certain extent connected by ammonium as an intermediate link.

The alterations effected in the action of the different members of these groups on muscle by the subsequent application of another is next discussed, and it is shown that the effect of one substance upon muscle may be increased or diminished by the application of another. One of the most curious points is that two substances having a similar action may, instead of increasing, neutralise each other's effect.

Barium, calcium, strontium, yttrium, and beryllium cause a great prolongation of the muscular curve or *contracture*. Some relations are pointed out between the atomic weights of antagonising elements of which the data are too limited to draw from them any general rule, but the authors think that they may possibly lead by and by to some useful result. Thus rubidium in large doses has the same effect as barium in causing a veratria-like curve, but barium destroys the effect of rubidium before producing its own effect.

$$Rb\ 85{\cdot}4 \times 8 = 683{\cdot}2$$
$$Ba\ 137 \times 5 = 685.$$

In the next division the authors show that by alternate application of acids and alkalies the muscle of the frog may be made to describe, on a slowly revolving cylinder, curves which almost exactly resemble those described on a quick cylinder by the normal contraction of a muscle on stimulation; and also those which the muscle describes on irritation after it has been poisoned by barium. They consider that the contraction of muscle may be possibly due in some measure at least to alterations in acid or neutral salts which the muscle contains.

Entomological Society, September 5.—Mr. J. W. Dunning, F.L.S., president, in the chair.—Baron Osten-Sacken of Heidelberg was elected a member of the Society.—Sir S. S. Saunders exhibited *Idarnella cariae*, Hasselq., which had been lost sight of for more than a century; and other interesting fig-insects.—Mr. F. Enock exhibited an hermaphrodite specimen of *Macropis labiata*, Panz.—Mr. J. Coverdale exhibited specimens of *Grapholitha cæcana*, Schläger, a *Tortrix* new to Britain.—The Rev. H. S. Gorham read a revision of the genera and species of Malacoderm *Coleoptera* of the Japanese fauna, part i., *Lycidæ* and *Lampyridæ*.

SYDNEY

Linnean Society of New South Wales, July 25.—Prof. W. J. Stephens, M.A., in the chair.—The following papers were read:—On the myology of the Frilled Lizard (*Chlamydosaurus Kingii*), by Charles De Vis, B.A. The author does not find there is any special muscular mechanism connected with the reptile's habit of elevating the frill and of occasionally assuming the erect attitude. The function of the frill he regards as being partly to frighten assailants, partly to aid in the collection and concentration of the waves of sound.—Descriptions of Australian Microlepidoptera, No. 9, by E. Meyrick, B.A.—Some remarks on the action of tannin on Infusoria, by Harry Gilliatt.

PARIS

Academy of Sciences, September 10.—M. Blanchard, president, in the chair.—On certain predictions relative to seismic disturbances, by M. Faye. The author exposes the groundless character of the theory recently advanced by M. Delauney and others, regarding the connection of earthquakes with the planetary movements, and more particularly with the supposed transit of Jupiter through the August meteors.—Separation of gallium (continued). Separation from titanic acid, by M. Lecoq de Boisbaudran.—A new method of filtration for highly diluted precipitates, by M. Lecoq de Boisbaudran.—Memoir on induction, by M. P. Le Cordier. In this paper the author adopts the theory of a continuous and incompressible medium, by the translations and pressures of which are produced electric currents and electrostatic phenomena. Electromotor and electrostatic effects of induction are calculated approximately for a hollow sphere forming an insulated conductor, homogeneous, isotropic, and non-magnetic, turning with a constant angular velocity round a fixed axis in a uniform and permanent magnetic field.—Experiments made at Grenoble, by M. Marcel Deprez, on the transmission of force by electricity. Note communicated by M. Boulanger on behalf of the Committee appointed by the city of Grenoble to follow these experiments.—Cholera from the standpoint of chemistry, by M. Ramon de Luna. From his chemical and physiological studies in Madrid and the Philippines the author concludes that cholera is propagated exclusively through the respiratory organs, and that the only safe treatment is the inhaling of hypoazotic vapour mixed with air. The best prophylactic is also found in hypoazotic fumigations of rooms, utensils, &c., twice a day. During the terrible outbreak at Manilla, in 1882, this treatment was adopted with complete success in the case of three hundred artisans employed in the mint.—Observations of the new comet discovered by Mr. Brooks on September 2, and of the planet 234 made at the Paris Observatory (equatorial of the West Tower), by M. G. Bigourdan.—Proposition on a question of mechanics touching the figure of the earth, by M. E. Brassinne.—Laws of induction due to the variation of intensity in currents of diverse forms; circular current, by M. Quet.—On the absorption of the ultra-violet rays by albuminoid substances, by M. J. L. Soret. From his experiments, in which he was assisted by MM. Danilewsky and Denis Monnier, the author concludes that all albuminoid substances hitherto studied contain a common principle, to which is due their characteristic absorptive band. Gelatine, which in so many other respects differs from albumen, acts quite differently. It is much more transparent, and gives rise to no bands.—On the proportion of food consumed by dogs under various temperatures, by M. Guimaraes. In the normal state the average daily consumption varied from one-tenth to one-sixteenth of the weight of the body; in a temperature of 10° to 12° C. from one-ninth to one-twelfth.—On the division of the cellular nucleus in plants, by M. L. Guignard.—On the structure of the leaf of the fossil genus *Sphenophyllum*, ranging from the Lower Carboniferous to the Upper Permian systems, by M. B. Renault.—General conclusions on the causes of chemical change in wheaten flour, and on the best conditions for preserving it for long periods in a sound state, by M. Balland.

September 17.—M. Blanchard, president, in the chair.—Allusion was made by the president to the loss sustained by the Academy in the person of M. Puiseux, member of the Geometrical Section, who died at Frontenay on September 9.—On the destructive fires caused by lightning, with some suggested improvements in lightning conductors (one illustration), by M. D. Colladon.—On the possibility of increasing the irrigating waters derived from the Rhone by regulating the discharge from the Lake of Geneva, by M. Ar. Dumont. The author dwells on the great benefits likely to be conferred on the southern departments of France by the project recommended by the Geneva Commission. This project, which might be carried out at an expenditure of about 180,000*l*., involves the creation of a hydraulic force of 7000 horse-power, by which the level of the lake at high water might be reduced by at least 0·60 m., and the minimum discharge of the Rhone at the outlet increased by 80 mc. per second.—Elements and ephemerides of the Pons-Brooks comet of 1812, by MM. Schulhof and Bossert.—Search for the red star observed during the total eclipse of the sun on May 6, 1883, by M. E. L. Trouvelot. The subsequent disappearance of this object might perhaps justify the supposition that it was an intra-Mercurial planet. But pending more accurate observations the author suspends his judgment on this point.—On the double star Σ 2400 of the Dorpat Catalogue, by M. Perrotin.—Electric law of the conservation of energy under all forms at entrance and issue of any material system traversed by the electric current, by M. G. Cabanellas.—On a new capillary electrometer, by M. A. Chervet.—Note on Hall's electric phenomenon, by M. Aug. Righi.—Qualitative research of manganese in the zinc of commerce, in zinc ashes and zinc spar, and search for bismuth in the lead of commerce by means of electrolysis, by M. A. Guyard.—New observations on the microbes of fishes, by MM. L. Olivier and Ch. Richet.—On the olfactory apparatus in the antennæ of Vanessa Io, by M. J. Chatin.—On the venomous properties of the jequirity, by MM. Cornil and Berlioz.—On the microbes found in the liver and kidneys of victims to yellow fever (three illustrations), by M. Babes.

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THURSDAY, OCTOBER 4, 1883

PHYSIOLOGICAL CRUELTY

Physiological Cruelty, or Fact versus Fancy; an Inquiry into the Vivisection Question. By Philanthropos. (London: Tinsley Brothers, 1883.)

WE desire to draw the most marked attention to this little book. The author says that his "aim has been to place in the hands both of professional and unprofessional readers a sort of compendium of the principal facts and most obvious reasonings on the question of experiment on living animals." Such being his "aim," he has hit his mark with singular force and precision. For our own part, after having read the literature on both sides of the vivisection question *ad nauseam*, we feel that no essay which has yet appeared upon the subject is better worthy of perusal both by lay and professional readers, and therefore we can have no doubt that, again to quote the words of its preface, "such a work may prove useful to medical men who have not time to consult books of reference, and examine into the details of the subject for themselves, without its being too technical to interest those of the general public who are willing to give thought and attention to a most important matter."

Whoever "Philanthropos" may be, he is clearly a man having an accurate and extensive knowledge of physiology, combined with a sound and careful judgment, remarkable literary ability, and a wise moderation of speech. His conclusions are everywhere reasoned conclusions, and being well equipped with the armour of fact, both on the right hand and on the left—ethics and physiology—we do not know a more hopeless engagement than it would be for any one who has arrived at different conclusions to meet this writer with these weapons of undeniable fact and dispassionate reason. But with all this he is something more (and may we not add, something better?) than a man of science and a logician. He is clearly a man of large and generous heart, of finely strung feelings, and a lover of animals as well as "a lover of men." "Philanthropos," indeed, while giving us step by step the reason of the faith that is in him, shows us by many indications that he is not a man to have joined sides with the physiologists merely from his love of science (supposing, as we feel entitled to suppose, that he is a working physiologist), but has been led to do so chiefly on the grounds, in its largest sense, of humanitarianism.

The treatise begins with an introductory chapter on the "Duty of Unprejudiced Investigation," and then goes on to consider in successive chapters, "What is Pain?" "What is Cruelty?" "Our Rights over Animals;" "What is Vivisection?"

Thus far it may be said that the writer is concerned with questions of definition, but in the vivisection controversy so much turns upon these questions that it is most desirable to begin with a clear exposition of all the above-mentioned points. In our opinion this exposition is the best that has so far been published. It only covers fifty pages, but these are so well packed that, while they read like the simple flowing of common sense, it is evident that they must have consumed a large amount of thought and labour in their production.

The work proceeds to consider "The Relation of Experiment to Physiology" and "The Relation of Medicine to Experiment." These are admirable chapters, full of clear and competent teaching, which it is most desirable that every one who ventures to express an opinion upon either side of the vivisection controversy should have assimilated. Next follows an important chapter on "Legislation, Past, Present, and Possible," which contains many judicious remarks on the administration of the present Act. A list of the more important researches which have been already prevented by the Home Office is given, and it is then urged: "The defenders of medical research ask for no such sweeping measure on their side. They have not demanded the repeal of the Act 39 and 40 Vict. ch. 77, whose short and insulting title is 'The Cruelty to Animals Act, 1876.' They would be content if it were administered in a spirit which takes for granted more humanity in experimenters, less omniscience in Home Secretaries, and more trustworthiness in their advisers. . . . It is surely absurd that an unqualified person should have the power of going behind the opinions of these high authorities, and contradicting them upon their own ground. On the contrary, the Home Secretary's professional advisers ought to be, like the Queen's, responsible for all technical points. . . . The Act is at present worked upon the principle that medical men are not to be trusted, their leaders' certificates not to be depended upon, and that cruelty would be the rule if it were not made impossible. But the profession was tried for cruelty before the Royal Commission and was acquitted. It would only be fair, therefore, to act on the basis of that acquittal, and admit that the abuse of their very restricted liberty is to be looked for as the exception, and not the rule. Therefore let the determination of who is to be licensed, and for what, rest [as was intended by the Royal Commissioners] with those who understand the subject matter of the decision. They are the best judges of the value of what is proposed to be done; and the sense of responsibility to the nation, and the public opinion of their own profession, will be amply sufficient safeguards against too great laxity. Probably the members of the 'anti-vivisectionist' societies do not believe that there is any such professional public opinion; but there is, and it is an effectual though quiet check on the few who need it. *But if any influence from without could injure it, it would be the constant ignoring and denying of its existence.*"

After a concluding chapter there are several very interesting appendices. Of these we have only space to notice the one on "The Medical Minority." Here it is first pointed out that of the forty-seven skilled witnesses who were examined before the Royal Commission only two were of the opinion that experiments are not necessary for original research in physiology. These are Mr. George Macilmain, M.R.C.S., 1818 (retired from practice), and Dr. W. B. A. Scott. "If persons of repute existed in the ranks of the medical profession willing to give adverse evidence, we may fairly suppose that they were called for on that occasion." But "a third exception to the unanimity of medical men is furnished by the author of a pamphlet which has lately been widely circulated by an anti-vivisectionist society." This pamphlet is on the "Uselessness of Vivisection upon Animals as a

Method of Scientific Research," by Lawson Tait, F.R.C.S., &c. Our regret that Mr. Lawson Tait should have destroyed any reputation he may have had as a man who ought to have some acquaintance with physiology, by making so public a display of his astonishing want of knowledge, prohibits us from saying much upon this painful episode in the vivisection controversy. "Philanthropos," indeed, has treated him with a leniency which is suggestive of compassion; but while it was necessary for our author to show by a few quotations in what a quagmire of ignorance and inaccuracy Mr. Lawson Tait has here immersed himself, we have no heart to look at so sorry a spectacle.

Having thus stated the aim and scope of the work before us, we may conclude this notice by quoting a passage or two as fair samples of its literary merit and argumentative tone:—

"This then is the sum total of the pain-giving experiments upon animals performed in England during three years. Less than 100 cases, of which the great majority consist of inoculations, followed—not by torture, but—by illness, form the contributions of our country to the 'systematic torturing of thousands of beasts all over the world,' referred to by a writer on the subject. It is a pity that ninety-five animals should have been put to any discomfort at all; and if illness and pain could be abolished from the world at one blow, the happiness of the lower creatures would be no small ingredient in the general joy. As it is, however, physiologists must aim at something humbler; they must try to decrease what they cannot destroy, and to alleviate where they cannot heal. And those who wish to narrow the means at their command for doing so, by totally prohibiting experiments on living animals, had better be quite sure that they know what the state of things is which they propose to alter. The same writer says that 'experimentation on living animals is a system of long protracted agonies, the very recollection of which is enough to make the soul sick as with a whiff and an aftertaste from a moral sewer.' The degree of correspondence between this phrase and the facts of English physiological practice will be apparent to the reader of the foregoing pages. And it is with facts alone that we wish to deal."

Again, after narrating a long list of cases in which medical and surgical practice, both on men and animals, has been directly indebted to physiological experiments, it is said:—

"The part of experiment in the progress of medicine is not confined to such results as can be catalogued. At every turn it controls observations, corrects deductions, verifies discoveries, suggests inquiries, always (as Prof. Sharpey so well said before the Royal Commission) 'putting a lamp in the hand of the physician.' This lamp has been turned down rather low in England, but it still burns. Will the world be better if it is altogether extinguished, and the task of shedding light upon the onward path of medicine left to the torch-bearers of other countries? For it is inevitable that—if the present anti-experimental agitation should prove successful—its history must tend to force all physiologists into identifying tenderness to animals with unscientific sentimentalism, and unreasoning disregard of the sufferings of men. And that injury to their finer feelings which is now supposed to have resulted from the free exercise of their profession must in truth come to pass in some measure from its enslavement in England."

Unfortunately on the subject of vivisection the great majority of persons seem to think that they are entitled to hold strong opinions without waiting to consider or inquire. Were this not the case, we should predict that this little book would have an enormous sale, for it is one that costs very little, either in time or money, to read, and it is written by some thoroughly competent and very judicious author. But although we are afraid that it will not meet with the recognition which it unquestionably deserves, we are not without hope that many who read this review, and who desire to form their opinions touching the vivisection controversy on a basis of sound information and consecutive reasoning, may be induced to see how much ado about nothing that controversy has raised.

GEORGE J. ROMANES

OUR BOOK SHELF

The Transactions of the New Zealand Institute. Vol. xv. for 1882. Edited by James Hector, M.D., F.R.S. Issued May, 1883.

THIS volume of nearly 600 pages contains a number of interesting and valuable papers on zoology, botany, and geology. As might naturally be expected, most of these relate to the fauna, flora, or geological structure of New Zealand, and in this way we can note from year to year the excellent progress that is being made in the scientific exploration of this country. While the paper, type, and general appearance of this royal octavo volume are excellent, we would venture to hint that it is possible that sufficient time is hardly given to the authors, no doubt widely scattered from Wellington, to properly correct their proofs, and that the general artistic finish of the numerous plates might be greatly improved.

Among the more important contributions to this volume may be mentioned the following:—Zoology: E. Meyrick, descriptions of New Zealand Microlepidoptera. Alluding to the descriptions of Walker and Butler as unreliable, the author in the first part of his memoir describes twenty-nine species of Crambidæ, sixteen as new. In a second part a list of Tortricina is given; of thirty-eight species, eleven are described as new. C. Chilton, several papers on new or rare species of Crustacea; several subterranean forms are described. G. M. Thomson, on New Zealand Copepoda; Prof. Hutton, several papers on the structure of Gasteropods, and on additions to the Molluscan fauna; W. Colenso, on some new Arachnida; W. T. L. Travers, on the distribution of birds; R. W. Fereday, descriptions of many new butterflies; W. Arthur, notes on fishes; T. F. Cheeseman, on two new Planarians; Prof. von Haast, on a skeleton of *Megaptera lalandii*; Prof. J. Jeffery Parker, several anatomical memoirs. Botany: W. M. Maskell, on new Desmids; T. F. Cheeseman, additions to the flora; W. Colenso, new ferns and flowering plants; John Inglis, accounts of some diatomaceous deposits; Charles Knight, on the lichens of New Zealand; D. Petrie, on two new species of Carex. Geology: S. Herbert Cox, on the minerals of New Zealand; Prof. F. W. Hutton, on some Tertiary shells, and on a silt deposit; W. S. Hamilton, on the formation of the quartz pebbles of the Southland Plains; J. A. Pond, on the occurrence of platinum in quartz lodes at the Thames Gold Fields. Several miscellaneous papers are added, one of the most interesting being an account of a visit to Macquarie Island, the most southerly island of the New Zealand group; it lies considerably to the south of Kerguelen Land or the Crozets. It is about eighteen miles long, and five miles broad. A list of the plants collected is given; their affinities are all towards the New Zealand flora, and no new species were discovered.

Electricity and its Uses. By J. Munro. (London: The Religious Tract Society, 1883.)

IN attempting to give the general reader "an understanding of all the essential parts" of the more wonderful and

recent of the electrical inventions, Mr. Munro has tried to accomplish a well nigh impossible task. That he should have been perfectly successful in his endeavour is hardly to be expected; nevertheless he has produced a book which a person unacquainted with electrical science may read with pleasure, and from which such a person may learn what wonders are accomplished by the aid of electricity, and in a general way how this powerful and subtle agent does its work.

The first thirty-one pages, in which the author gives so much of the theory of electricity as may be necessary to enable any one to understand his descriptions of the inventions which follow, form without doubt the weak part of this book. In describing the effect of grouping thermopiles and elements in series, the author seems to have confused electromotive force with current strength, for he says that with such a combination a powerful current equal to the sum of the elementary ones will circulate in the connecting wire. The short chapter on induction is likely to cause in the mind of a person unacquainted with electrical science some confusion between statical and current induction.

With the fourth chapter a description of the inventions begins, and here it may be said that the book proper begins. The chapters on the telegraph and telephone and all the inventions which depend on the telephone are excellent, the general principles being so clearly given as to be readily understood. The theory of the dynamo is too difficult for an essentially popular book, and here, as in one or two other places, the author has wisely refrained from leading his readers into a sea of complexity from which they could with difficulty have escaped, but has carried them over with an agility worthy of a conjurer. The remaining chapters, which deal with lighting, transmission of power, heating, and plating, are written in a popular style, and will no doubt be read with interest.

C. V. B.

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications.*

[*The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to insure the appearance even of communications containing interesting and novel facts.*]

Professor Henrici's Address at Southport

Mr. J. J. Walker, in his letter printed in your last number (p. 515), draws attention to the works of Chasles, and I am glad that he has thus given me an opportunity of saying a few words about the relation in which the writings of this great geometer stand to those of the German geometers mentioned in my address.

I am fully aware that his works are well known in England, and so I believe are those of Poncelet and others. But the study of the works of Chasles does not give as complete a view of the variety of methods invented and results obtained on the Continent as might be expected from the author of the "Aperçu Historique" (1837). In that brilliant work he regrets himself that he does not understand German, and does not therefore give an account of what was done in Germany by "Steiner, Plücker, Möbius, &c.," and it seems that he always remained ignorant of it. At all events he did not fill up those gaps in his important "Rapport" of 1870. In much of his own work contained in his "Aperçu" he had been anticipated by Möbius and Steiner, and his "Géométrie Supérieure," which appeared twenty years later than Steiner's "Entwickelungen," and twenty-six years later than the "Bary-centrische Calcul," by Möbius, and which is, I believe, in England considered the chief book from which to learn modern Continental geometry, must not be taken as a standard of what at the time of its publication was known in Germany.

With regard to the arithmetic, I beg to point out that I only judge from my experience as an examiner. The methods of abbreviating calculations with decimals must have been known long before De Morgan, I fancy, but that is a very different thing from having them introduced as an important part of the teaching of arithmetic in schools.

The manner in which a large number of candidates worked examples at the London University Matriculation Examination startled me considerably, especially as I noticed that the process of working decimals described in my address was used by candidates who otherwise gave evidence of really sound knowledge and good teaching.

O. Henrici

The New Comet

The new comet (Pons) was seen here last night in the 6-inch equatorial, its place closely corresponding to that of the ephemeris contained in the recent *Dun Echt Circular*.

In the comet eyepiece it was large, round, and faint, with no tail and but little trace of central condensation. It might, in sweeping have been taken for a nebula, having very much that look. I could not see it in the 2-inch finder, and though fairly visible in the comet eyepiece (power 35), a very little mist that came up rapidly obscured it.

J. Rand Capron

Guildown Observatory, Guildford, October 2

The Genus "Simotes" of Snakes

In the report of the Committee appointed by the British Association for the investigation of Timor Laut, given in at Southport during the recent meeting, I find that among the snakes discovered by me one has been described as a new species of Simotes, and is noted as being of "special interest, as no species of the genus had hitherto been previously known to occur eastward of Java." In the *Proceedings of the Zoological Society* for the year 1864, p. 180, a species of Simotes, *S. australis*, was described by Gerard Krefft, from Port Curtis, Australia, as being "the first *Simotes* discovered in Australia."

Aberdeen, October 1 Henry O. Forbes

Floating Pumice

Referring to a note in the last number of Nature (p. 532), giving an account of a steamer's having encountered vast quantities of pumice in the Indian Ocean, it may be of interest to record that after passing, in the R.M.S. *Quetta*, the Straits of Sunda on July 9 last (having sailed close under the then active Krakatoa), we traversed a continuous field, unbroken as far as the eye could reach, of pumice, every day till the evening of the 12th, when our position must have been six hours (± 60 miles) to the west of our noon position, 93° 54′ E. long. and 5° 53′ S. lat. Capt. Templeton assured me that there was, singularly enough a current against us all the way from the Straits of one-third of a mile per hour. There can be no doubt that this pumice came from Krakatoa, and possibly also that mentioned by the steamer in your note last week. The pumice knobs were all water-worn, and a few had barnacles of about one inch in length growing on them. It will be recollected that the eruption first broke out on May 22 and 23.

Aberdeen, October 1 Henry O. Forbes

"Elevation and Subsidence"

In the number of Nature for September 20, Mr. W. F. Stanley (p. 488) requests references to where it has been considered that the sinking on the coast of Greenland is due to the weight of inland accumulation of ice. If Mr. Stanley has only so lately as the present year advocated this opinion, though doubtless the idea has arisen independently with him, he certainly has no right to consider himself the originator of it, which he seems disposed to do. So far as I am aware the priority is due to Mr. T. F. Jamieson (*Quart. Journ. Geol. Soc.* vol. xxi. 1865), who attributed the subsidence, which is universally conceded to have occurred during the Glacial period, to the enormous weight of snow resting on the land, considering that if the interior of the earth on which the crust rests is in a state of fusion, a depression might take place from a cause of this kind; and then the melting of the ice would account for the rising of the land which seems to have followed upon the retreat of the glaciers.

Unaware of this proposition, in 1871 (President's Address, *Proc. Liverpool Geol. Soc.* 1871–72; *Geol. Mag.* vol. ix. 1872) I in the same way ascribed the subsidence of the land during the Glacial period to the combined weight of snow and the boulder

clay, that is of the mud, &c., which then issued in streams from beneath the glaciers, and contains the pebbles derived from distant localities which had drifted in icebergs and coast-ice, and been dropped into it, the land being raised to a considerable extent when, upon the return of a more genial climate, it was relieved of its load of ice and snow.

Prof. N. S. Shaler, of Harvard College, U.S.A., in 1874 (*Mem. Boston Soc. Nat. Hist.* vol. ii.), considering that by the hypothesis of Adhemar the conditions which would have resulted were not fulfilled during the last Glacial period, concluded that we may more reasonably look to the weight of ice then accumulated on the continents for the depression of the land areas it occupied.

In a paper on the "Cause of the Glacial Period," read before Section C at the meeting of the British Association at Bristol, 1875 (*Report*, p. 79; also *Geol. Mag.*, Decade II., vol. ii.) I adduced evidence tending to prove that such a subsidence of the Isthmus of Panama has taken place as would allow a diversion of the equatorial waters of the Atlantic into the Pacific; as a consequence of which similar effects to those which occurred during the Glacial period might have been produced. The formation of the Canal ought to afford to competent observers absolute proof whether such has been the case or not. In the course of the essay I ascribed not only the subsidence during the Glacial period, but also that now rapidly progressing in Greenland, to the weight of the greatly increased accumulation of snow; and that the rise of the land in Norway is dependent on the removal of pressure by the melting or diminution of the glaciers. It does not appear unlikely that to a great extent the rising of the Andes may be due to the dissolution of the snow which once covered these mountains in a greatly increased degree, it may have been contemporaneously with the Glacial period in Europe and North America; and in part to the transfer of pressure, by the materials derived from its flanks and brought down by the Amazon and its tributaries forming at its delta the "measures" in this great coal-field of South America now in process of formation.

Mr. J. Starkie Gardner, at a much later period (*Geol. Mag.*, June, 1881) stated that great accumulations of ice in the Glacial period seem to have been accompanied by subsidence, and even Greenland at the present day may be sinking under its ice-cap. In the same year the Rev. O. Fisher, in "Physics of the Earth's Crust" (p. 223), accounted for the raised shell beds found in Scandinavia at an altitude of 700 feet, by the country having been formerly depressed owing to its being loaded with heavy ice fields, and that its gradual subsequent rise may have been caused by the ice having melted off. He remarks that similar movements have occurred and are now going on in Greenland; and that the subsidence of 6 or 8 feet in a century may possibly be accounted for by the snow-fall being at present greater than is carried off by the glaciers and evaporation.

During the present year Mr. W. F. Stanley (NATURE, vol. xxvii. p. 523) held that the cause of the coast of Greenland sinking is the weight of the present accumulation of ice upon that continent. Quite recently Mr. Searles V. Wood (*Geol. Mag.*, July, 1883, footnote) thinks that the overwhelming of reindeer pastures by the ice during the centuries of Danish occupation, and the indications of subsidence afforded by the position of ancient dwellings, may show that the ice is now augmenting, and the land sinking under its weight.

But the great question is not to whom belongs the priority of attributing the depression of the land during the Glacial period, and at present in Greenland, to the weight of accumulated snow, and the reelevation to its removal; even this explanation of the phenomena under consideration, important though it may be, is but an item in the still greater one, namely, whether the depression which has taken place and is still in progress at the mouths of great rivers, in their deltas, in the estuaries and bays into which, emptying themselves, they carry mud, sand, pebbles, and other debris, is caused by the weight of these accumulated deposits pressing down the crust of the earth beneath them, thus permitting further accumulation to any extent; and also whether the subsidence, which by every one is conceded to have occurred during the deposition of all stratified rocks, from the earliest of which we can read the record in the "great stone book" to those now in progress, is due to the same cause—the weight of the materials of which they have been formed.

The converse has also to be inquired into, whether the elevation of the land and the formation of hills and mountains is the result of the abrasion of the land and the transfer of the disintegrated materials to a distance by rain and rivers; thus relieving by so much the locality from which they have been removed of the weight pressing on the crust of the earth. The highest hills in a district are those from which the greatest amount has been removed by denudation, their summits not unfrequently consisting of the lowest rocks in the geological series of the neighbourhood.

Birkenhead, September 22 CHARLES RICKETTS

I QUITE agree with Mr. Mackie in believing that "the connection between sedimentation and subsidence on the one hand, and between denudation and elevation on the other," are "simply concomitant effects of the same cause;" that, in fact, depressions in the earth's crust are the cause of sedimentary deposits, and not the deposits the cause of the depressions, and, further, that the elevations and depressions are caused by lateral pressure developed by the shrinking of the earth's crust; but is it necessary that certain parts of a depressed area should be "strengthened by volcanic outbursts, &c."? I do not think so.

If a magazine or book with a paper cover be held close, and pressed from back to front, the mass of the leaves is thrown into anticlinal and synclinal curves though the book is at no point stronger than at any other; the pressure is brought to bear upon the book, and as it cannot "telescope," it is necessarily bent upwards and downwards. Is this not something like what happens to the rock?

Take, for example, the Old Red Sandstone between the base of the Grampians and the Carboniferous rocks of Fife. This is a plain partly composed of sandstones, partly of sandstone with interbedded volcanic rocks, and partly of solid masses of volcanic ejecta. The plain has been bent into two anticlinal and two synclinal curves.

In such a varied area, if anywhere, one would expect to find evidence of the influence of the relative strength of the rocks in modifying their curvature.

The syncline nearest to the Grampians is mainly composed of sandstone and conglomerates; as these rocks bend up towards the anticlinal axis to the south, the Sidlaw Hills (composed of hard sandstones and interbedded porphyrite, &c.) present a very striking example of strengthening of the beds; still they are neither on the anticlinal nor the synclinal axis, for though near to and towering high above the former, they lie on the slope of the beds dipping towards the north. The rocks of the second syncline are sandstones with intrusive and interbedded lavas, the volcanic rocks greatly increasing in proportion to the sedimentary towards the synclinal axis near the estuary of the Tay, towards which the rocks are gently bent up, while across the estuary, which occupies the position of the denuded arch of the anticline, the rocks are almost entirely sheets of lava, with volcanic breccias, &c.

Thus we have a synclinal and anticlinal curve, both of sandstone, while the hard and thickly-bedded volcanic rocks form part of the slope between them, and again we have a syncline partly composed of interbedded lavas and sandstones, while the almost entirely volcanic rocks are bent up into an anticline.

It would therefore seem that the quality and thickness of the rock masses have very little influence upon the form of the curves into which they are bent. JAS. DURHAM

Newport, Fife

Photograpby and Still Life

I HAVE been assured, by a gentleman to whose opinion all dabblers in science photography must bow, that the following method of photographing objects of still life was unknown to him, and that its publication might prove useful to others.

Having some years ago to photograph a series of implements to illustrate a paper on the Borness Cave, I was met at the outset by the difficulty of avoiding cast shadows and such accessories as were needful for posing the objects to be copied. It occurred to me that a pane of glass, a white cloth, and some beeswax would meet the difficulty; as objects fixed to the glass by beeswax with a white cloth behind them would "come out" on a white ground free from the shadows and accessories I wished to avoid. Having been recently asked to photograph some important bones, teeth, and flint implements, necessity, "the mother of invention," has much improved on the original rough process, and I can confidently recommend the following cheap apparatus as extremely efficient, viz., a *square* pane of plate glass with a hole drilled in the centre (for fastening such

objects as may be too heavy for the beeswax). The pane to slide between two grooves into any convenient movable stand. The advantage of this form and arrangement is obvious, as after the object or objects are fixed to the glass they can be inverted or placed sideways, as may best suit the light, without moving the camera. Moreover, the stand can be tilted or set obliquely at the operator's pleasure, the object being thus adjusted to the camera instead of the camera to the object. The backgrounds can of course be changed at will to any shade between black and white—a most important power, as a background that will set off one object will often be unsuitable to another.

Torquay, September 15 ARTHUR R. HUNT

Animal Intelligence

AT the north side of Dublin there is at Clontarf a sea inlet where the water at certain times of the tide is very shallow. A little stream flows under the road into the sea at this place. The bridge beneath which it passes has pretty high parapets. A huge dog, a frequent companion during my student days, used to mount one of these parapets, employing it as a lookout when he happened for the moment to lose sight of me. Mrs. Comerford, widow of a distinguished barrister, was my landlady. This dog, aided by an accomplice named Bran, slew Mrs. Comerford's red cat, a great favourite, and buried him, all but the point of his tail, in the garden. The accomplices demeaned themselves in the most innocent manner, but betrayed considerable confusion when their delinquency was detected. It did not seem to occur to their canine minds that the mere tip of the poor cat's tail, when the body itself was out of sight, could possibly incriminate them. But to return to Clontarf. It was the practice among the lads about, when the depth of water suited, to wade out and catch little flatfish. These abound in great numbers, and lie commonly on the seabed. The waders went in barelegged, and when they happened to tread upon a fish, kept the foot in position until they could stoop down and secure their prey. One of the fisherboys was one day attended by his dog, and when the intelligent creature saw the work in which his master was engaged, proceeded to help him by plunging about, and whenever he felt a fish, kept his paw upon it until his master should come up and place it in his creel. This curious method of catching flatfish is not confined to Clontarf. I was walking one day along Con's Water, called after the old chieftan of the name, Con or Constantine O'Neil, when I observed a barefooted lad wading in the shallow water, for the tide was out, and from time to time casting something on the bank. He was catching flatfish with his feet. I did not detect his occupation, in which he seemed pretty successful, until I went close up in order to see what he was about.

Belfast, September 22 HENRY MACCORMAC

Meteor

IT may interest some of your readers to know that a meteor was seen here this evening during a thunderstorm, and immediately after a flash of lightning. It appeared about the size of an ordinary cricket ball, and was of a brilliant yellow colour, and moved very slowly in an upward northerly direction from about east-south-east. As it moved along, it gradually decreased to the size of an ordinary star, and was then lost to my view. The storm began about 7 o'clock, and lasted about half an hour, during which time the lightning was very vivid. A very thick fog (that arose suddenly) preceded the storm, but disappeared before its commencement. The weather during the day had been close, with heavy showers at intervals. C. FORTESCUE

11, Oxford Road, Banbury, September 20

A Remarkable Rainbow

ON Monday, September 24, I saw at Chertsey, in Surrey, a remarkable rainbow. Beyond the blue of the inner bow the colours repeated themselves three times, so that there appeared four contiguous spectra; the three extraordinary ones being narrower and less bright than the ordinary. The outer bow appeared as usual. I am not aware that this phenomenon has been noticed before, and being quite unable to account for its appearance would be greatly obliged to any one who would enlighten me.

Firfield, Weybridge Heath, September 25 L. C.

Professor Cayley

WITH reference to Dr. Salmon's account of Dr. Cayley's undergraduate career it may be worth while to call the attention of some of the readers of NATURE to a contemporary description in C. A. Bristed's "Five Years in an English University," vol. i. pp. 130–132 (1852). In this volume are also to be found many notices of other Senior Wranglers and Senior Classics of about the above date. R. T.

THE NORDENSKJÖLD GREENLAND EXPEDITION

THE following is an abstract of two communications received from Dr. A. G. Nathorst, dated Upernivik, in Greenland, July 22 and August 2, in which the eminent Swedish naturalist gives an account of the work of the Nordenskjöld expedition up to the latter date:—

Having left Reikiavik on June 10, we sighted the coast of Greenland in lat. 65° 50′ on the 12th, but were unable, on account of the pack-ice, to reach the shore. During the following day we steamed along the ice, dredging and making hydrographical measurements with great success, and on the 14th we came very close to the shore in lat. 62° 40′, but, as it was impossible to land even here, we made for Julianshaab, *viâ* Cape Farewell. From there Nordenskjöld, Herr Kolthoff, and myself made an excursion to Nunasernansak, in the Kongerdluarsuk Fjord, the only spot on the earth where the remarkable mineral "endialyt" is found, and from which the metal known as zirconium is produced. Of this, as well as of other minerals found here, we made an excellent harvest.

Having called at Godhavn, we arrived, on June 29, at Ujaragsugsuk, where Herr Hamberg and I landed in order to examine the fossil plant-bearing strata here, while the vessel proceeded to the Auleitsivik Fjord, whence the ice journey was to commence. On the way north the *Sophia* called at Egedesminde, and on July 1 anchored at Tessiursarsoak, where a splendid harbour was discovered, which was afterwards charted by Sergeant Kjellman under the name of "Sophia Harbour." July 2 and 3 were spent in bringing the baggage for the ice journey up on the ice, and on July 4 Nordenskjöld started in the company of Dr. Berlin, in the finest weather, on his inland excursion.

On July 8 the ship was to have left the harbour to take us on board again, but it was not until four days after that she succeeded in getting out on account of ice. These days were occupied by Dr. Forsstrand and Herr Kolthoff in dredging and in making ornithological, entomological, and botanical collections, a labour which was attended with remarkable success. On the 14th the *Sophia* arrived at Godhavn, where the *Yantic* and *Proteus*, the two American vessels on the way to Smith's Sound for the relief of Lieut. Greely's expedition at Lady Franklin Bay, were lying. Here the well-known Esquimaux interpreter, Hans Hendrik—generally called Hans Christian—who has participated in Arctic expeditions ever since Kane's voyage, joined the vessel, and on the 7th Herr Hamberg and I were taken on board.

The results of our researches at Ujaragsugsuk are *exceedingly* good, and many new discoveries, both geological and palæontological, have been made. The finds made at Atonekerdluk, on the other side of the Waigat, were especially very remarkable and valuable, as a number of hitherto unknown strata bearing fossil plants were discovered, from which magnificent leaves of *Araliæ*, *Magnoliæ*, *Lycasartæ*, *Platanæ*, and others were extracted. An idea of the size of the collection made may be gathered from the fact that they fill five large barrels, five boxes, and a firkin, which will all be despatched by a sailing vessel to Copenhagen. On July 22 the *Sophia* left Upernivik for Cape York, where Hans Hendrik says that the iron blocks we desire to examine are really lying. On the way north we found little ice, most of it being "calved" from the glaciers; we encountered, however, much fog, and were often compelled to "lay to," but such time has always been spent in dredging and studying the sea.

Between June 24 and 27 we were cruising in the pack-ice from 74° to 76° 5′ lat., where we sighted Conical Rock. It was, however, impossible to penetrate towards Cape York, but only to the north-west. As we saw a fjord north-east of Conical Rock, which was, however, not marked on the chart, we steered into it and cast anchor. Seeing some human beings on the shore, we landed, and found them to be a couple of Esquimaux families, rude and uncivilised, but obliging. They only stay here in the summer for catching the rotges which breed here in large numbers, while during the winter they sojourn on an island in Wolstenholme Sound, where they hunt the walrus. They possessed, however, no boats. We purchased some of their tools, &c., clothes of birds' skin, and some bear and fox hides.

On July 27 we sent two Esquimaux to examine the ice towards Cape York, who came back and reported that it was still lying along the south-east coast. For four days we attempted to penetrate northwards, running the ship in every direction where we saw a lead, but, as we everywhere encountered the ice barrier and were several times in great danger of being crushed, we "stood about," and arrived at Upernivik on the night of August 1.

It appears thus that the last severe winter in Greenland has also extended up Smith's Sound, as an example of which I may mention that Nares, who on the same day as we, in 1875, steamed up Smith's Sound towards Cape York, found the sea entirely free from ice.

That the chief object of this part of the expedition, while in my command, should not have been realised, I extremely regret; but I console myself with the fact that every effort in human power was done in order to carry it out.

I may say, however, that the exceedingly rich, zoological, botanical, and hydrographical fruits of the expedition towards Cape York and back, fully repay the cost and labour of the voyage. We leave here (August 2) for the Waigat, where I intend continuing my geological researches, while the rest of the expedition on board start on a four day's zoological and hydrographic excursion towards America. When that is over we start for Egedesminde, to take Nordenskjöld on board.

Dr. Berlin, who accompanied Nordenskjöld on his journey on the ice in Greenland, writes as follows:—

On July 3 the march began from the Auleitsivik Fjord. The party consisted of Nordenskjöld, myself, Sergeant Kjellström, the second mate Herr Johannesen, two hunters, Sevalsen and Kræmmer, two sailors, and the Lapps, Anders and Lars. We reached on sleighs, according to solar observations, eighty miles (English) inland, reckoned from the ice border, when the Lapps were sent forward 130 miles further, a distance fixed by their own judgment, which may be fully relied on. This was done because the deep, loose snow prevented us proceeding on sleighs, while it was eminently suitable for the "skidor," or snow "runners" (they are not "shoes") of the Lapps. We found no "ice-free" country, in fact the latter may, by this expedition, be fully proved not to exist, neither in this nor in any other latitude in Greenland. By the above-mentioned calculation, and estimating the shore-line at seventy miles inland, we have succeeded in reaching 280 miles into Greenland, which is more than half its width, while the Lapps, from their point of return, saw the land a good distance further east. The ice rose at the furthest spot reached to 7000 feet above the sea, and was still seen to rise to the east. The journey lasted a month.

THE PRESENT CONDITION OF FISH CULTURE

WITHIN the past few years the science of fish culture has made rapid progress, and radical changes have been made both in the apparatus and methods employed. Experience has enabled the fish-culturists to improve upon the old forms of hatching-boxes and troughs, while the propagation of additional species has necessitated the invention of new forms. The International Fisheries Exhibition, now in progress in London, has brought together valuable collections from the leading specialists of all parts of the world. A study of these enables one to form a very correct idea of the present condition of the science.

The subject is now sufficiently understood to warrant a division of the hatching apparatus into four classes: (1) apparatus for heavy eggs; (2) for semi-buoyant eggs; (3) for floating eggs; (4) for adhesive eggs.

Heavy eggs like those of the salmon and trout are hatched with little difficulty. An almost endless variety of apparatus intended for eggs of this class is exhibited, but it may all be referred to one of three divisions depending upon the direction of the current of water, namely, that with a horizontal current, that with an upward current, and that with a downward current. Apparatus of the kind first mentioned is most commonly employed, but that with an upward current has many points of superiority. Chief among these are economy of space, saving of water, and the prevention of injurious sedimentary deposits. In the United States, where, owing to the enormous quantity of eggs handled, economy of space is a necessity, the upward current is quite generally employed. A number of the American forms are provided with from ten to fifteen wire trays; these, when filled with eggs, are placed one above another, so that the entire volume of water must pass through each of them on its way through the compartment.

Semi-buoyant eggs, like those of the shad (*Alosa sapidissima*) and whitefish (*Coregonus clupeiformis*) require a treatment entirely different from those already mentioned, as their specific gravity is but slightly greater than that of fresh water, and they are easily carried about by the currents. The best results are obtained by directing an upward current against the eggs, thus producing a gentle but constant motion, and keeping them partially or wholly suspended in the water. As little attention is now given to hatching semi-buoyant eggs outside of the United States and Canada, the collections of these countries contain nearly everything of interest in this class. We find here various forms of floating boxes adapted to river currents, apparatus fed by water which is introduced under pressure through closed pipes, and mechanical apparatus requiring motive power. The first-named is admissible where the work is limited or where rigid economy is a necessity. The second is preferable in any city where hydrant water can be obtained or when the work is sufficiently extensive to warrant the use of a pumping-engine. The third is occasionally employed where large quantities of eggs are hatched, but it is more expensive than the one last named, and the results are usually less satisfactory. Apparatus of the second kind is ordinarily made of glass, its efficiency depending largely upon the motion imparted to the eggs and the position of the outflow through which the waste water and dead eggs escape. This opening in nearly all of the apparatus exhibited is placed at the top of the jar, and a current strong enough to carry off all of the dead eggs frequently carries many of the good ones with it, while the motion of those that remain is often so violent as to cause serious injury. An improvement in apparatus of this class has recently been made by Marshall McDonald of Washington, D.C. His apparatus consists of a closed jar having an outflow through a glass tube which passes into the interior of the jar, and can be raised or lowered at will. With this apparatus the dead eggs are easily removed by the slightest currents, and excellent results are obtained.

More difficulty is experienced in finding suitable apparatus for floating eggs, like those of the cod (*Gadus morrhua*), than for any other class. Only five forms intended for floating eggs are exhibited. None of these

are entirely satisfactory, but two of them are used with moderate results. The first is a rectangular wooden box with a wire-cloth bottom, and lateral openings even with the water-line covered with the same material. Around the outside of the box, just below the openings, is a strip of wood four inches wide which rests upon the surface of the water and serves as a float to keep it in position. This float forms an inclined plane leading to the lateral openings, and the waves striking against it run up the slight incline passing through the wire covering into the interior of the box, thus giving a constant circulation from above, the surplus water passing out through the bottom. The other form consists of an ordinary hatching trough divided into compartments by means of transverse partitions. The trough is placed at a slight incline, and the water passes from one compartment to another through a shallow tin spout placed in a notch at the top centre of the partition. In these compartments are smaller wooden boxes with wire-cloth bottoms. These are so placed in the compartments that their forward ends shall rest under the spouts that conduct the waste water from the compartment above, the free ends being thrown slightly upward by their own buoyancy. With a box thus placed in a compartment filled with water, the stream that is kept constantly running falls into its deepest part, creating a circular current, the waste water passing out through the bottom and up around the sides on its way to the next compartment, the wire-cloth preventing the escape of the eggs.

Much of the apparatus for adhesive eggs, like those of the herring (*Clupea harengus*), is very primitive, consisting simply of boxes lined with pine and spruce boughs or twigs, in which the parent fish are kept during the spawning period, the eggs adhering to the pine boughs. In some, the pine boughs are fastened to movable frames to admit of their transportation to other waters, but in most they are stationary, the fry being intended for the waters in which they are hatched. A decided improvement on the above are the more modern forms intended for artificially impregnated eggs. Unquestionably the best apparatus exhibited is a wooden trough with plates of glass placed at right angles to its length, invented by Frank N. Clark, of Northville, Michigan. The eggs are taken and impregnated upon these glass plates, and at once spread evenly over the surface by means of a feather. They soon adhere to the plates, which are then placed in grooves which have been cut into the side of the trough, three-fourths of an inch from each other. The grooves are so cut that every alternate glass shall rest on the bottom of the trough, with the others half an inch above, so that the water shall pass over the top of the first, beneath the lower edge of the second, over the third, &c., on its way through the trough, thus supplying a constant current to the eggs. Other apparatus made of muslin is exhibited, but this, for several reasons, chief among which is the tendency of the cloth to retain any sedimentary matter that may be in the water, is less effective.

Formerly the material of which hatching apparatus was composed was a matter of much importance to the fish-culturist; but the introduction of asphalt varnish has rendered the choice of materials a secondary consideration: the fish-culturist has now to consider the adaptability and cost of materials only, for almost any substance, whether metal or wood, if properly coated with asphalt, can be successfully employed.

Illustrations of recent methods of securing and retaining the adult fish until the eggs have been secured, are exhibited. At one of the hatching stations in Canada the fish taken along the shores are transported to saltwater ponds, where they are kept until the eggs and milt have fully developed. A decided advantage is claimed in salt over fresh water, as in it there is a much smaller percentage of loss among the fish, the presence of fungus (*Saprolegnia ferax*), that dreaded foe of all fish-cultural operations, being entirely unknown. In the McCloud River, California, a dam is placed across the stream directly opposite the hatchery, and the fish, finding further progress impossible, drop back into the deeper portions of the channel, a few rods below, where they can be easily caught by the aid of a haul-seine. In Grand Lake Stream, Maine, nets are stretched across the mouth of the river to direct the fish into basins of netting, where they are retained till the spawning season arrives.

In localities where the supply of eggs is obtained from the ripe fish taken in the nets of the fishermen, the United States Fish Commission some time since introduced steam launches for visiting the fishing grounds and distributing the spawn-takers among the more important fishing stations, and again bringing them with their take of eggs to the hatchery. This plan worked well, and enabled the Commission to obtain a much larger number of eggs than formerly, with little or no increase in expense. In 1882 the plan of stationing professional spawn-takers at the larger fishing shores, to remain during the height of the spawning season, was adopted. These are expected to examine every fish landed, and to secure all ripe eggs, which, after impregnation, are placed upon wire trays covered with damp cloths, and set in a cool place to await the arrival of the steam launch, which usually makes daily trips to collect the eggs and carry them to the hatchery. In the absence of the launch the eggs are often shipped by the ordinary river steamers.

The improvements in hatching apparatus, with a view to economy of space were important steps in the progress of fish culture, as the introduction of new forms greatly increased the capacity of the hatcheries in which such apparatus was employed. But even with these improvements fish-culturists have often found it difficult to handle as many eggs as they desired, owing to the limited duration of the spawning period. Their hatcheries were crowded for a short time, and the simultaneous hatching of the eggs required a large force of messengers to distribute the fry. As the spawning season had usually passed by the time the first fish were hatched, no more eggs could be secured, and the hatchery had to be closed till the following year. This difficulty is now practically overcome in several ways. It is found by Sir James Maitland that the spawning season for fish kept in artificial ponds is considerably affected by food. An abundance of hearty food at the time when the ovaries are beginning to enlarge hastens their development, while a scanty supply of coarse food considerably retards their growth. By judicious feeding one can arrange to have the fish of different ponds spawn at different dates, so that two crops instead of one can be produced at his hatchery. Refrigerators are now successfully employed in cases of wild fish, when the spawning time cannot be controlled by food. The surplus eggs, after the hatchery has been filled, are placed on cloth trays and corded up in the refrigerator until such time as there is room for them in the hatchery. They should be examined at intervals, and if in poor condition should be washed in ice water and again returned to the trays. Trout ova three, five, and seven months old respectively were exhibited in the German department, the eggs being apparently in excellent condition. The use of refrigerators, however, is at present limited to winter-spawning fishes. With summer-spawners it has thus far resulted disastrously.

Those interested in the acclimatisation of fishes have much to stimulate and encourage them. From all quarters come encouraging reports of the successful introduction of fishes into new waters. The shad of the Atlantic coast of America (*Alosa sapidissima*) is now taken in such quantities in the rivers of California as to claim a place among the food fishes of the Pacific. The California trout (*Salmo irideus*) is now grown in the waters of Australia, Japan, and Germany: with the fish-culturists

of the last-named country it is a special favourite. The German carp (*Cyprinus carpio*) has, through the efforts of Prof. Baird, been scattered broadcast over the United States, and in the warmer regions grows with surprising rapidity, attaining a weight more than double that of fish of the same age in their native waters. Eggs of the California salmon (*Salmo quinnat*) have been successfully transported from the United States to nearly all of the European countries. Though the species has always been considered a sea-going salmon, the fry hatched from the eggs sent to the Netherlands have been kept in freshwater ponds, where they have matured and spawned, the young produced from the eggs being as healthy as those hatched in California. These thrive better when confined in freshwater ponds than the native salmon (*Salmo salar*) under the same conditions, and fish-culturists of that country consider them better suited to their inland waters than any of the native Salmonidæ. It seems probable, from these experiments and from those with other species, that it will be possible to raise any of the Salmonidæ in fresh water.

The exhibits show the fish-cultural apparatus of many countries to be very primitive, and the hatching operations to be limited to a few species of Salmonidæ. The Exhibition cannot fail to advance the interests of fish culture in general, for it will open the eyes of the people to the fact that other species besides salmon and trout are worthy of attention, and can be hatched in enormous quantities without difficulty. Another important result of the Exhibition will doubtless be to convince the people of the value of fish culture as a means for increasing the food supply. It is indeed fortunate for the future of the science that so deep and widespread an interest has been awakened in the subject, for several papers calling in question the results of fish culture have recently made their appearance, and it requires the testimony of the leading authorities to counteract their influence. The testimony is just what might have been expected, and shows that in those countries where the operations have been most extensive confidence is strongest, and in those where work has been limited many question its practicability. Thus in both Sweden and Norway, where only a few thousand fry at most are placed in any stream, many of the hatcheries that formerly existed have ceased to operate, as no immediate results of their work could be seen. The same is true of other localities where the work has been carried on, to a limited extent only, by private parties; but where extensive hatching operations have been continued through a long period, the beneficial results are invariably acknowledged. Some, who are obliged to acknowledge the value of fish culture in cases where the fry are retained in ponds from which they cannot escape, question its result when the fish are turned out into waters tributary to the sea; these forget that the number of fry turned out by the wash-tub fish-culturists of many countries is so limited when compared with the entire number of fish in a stream, as to have no appreciable effect upon the fisheries. They also forget that, though owing to extensive operations considerable increase in the fisheries of a given locality may be noticeable in three or four years, the full results of artificial propagation cannot be expected until the fry of the artificially-hatched fish have developed into full-grown specimens and returned to the rivers to deposit their eggs. This would require from seven to eight years, as the second generation of any sea-going species can scarcely be expected to return before that period has elapsed. Canada and the United States are the only countries where public fish culture has been conducted on a large scale for a sufficient period to warrant a reliable verdict as to the importance of the work; and in both of these countries public opinion is decidedly in favour of continuing the work, and on a larger scale than ever before.

R. EDWARD EARLL

NOTES

WE regret to learn that M. Dumas has been confined to his bed for the last ten days, although his illness is not in itself serious.

PROF. CUNNINGHAM, of the Royal College of Surgeons, Ireland, has just been appointed to the Professorship of Anatomy in the University of Dublin, in succession to Prof. Macalister. The Professorship of Comparative Anatomy, also held by Dr. Macalister, is vacant, but will not be filled up until the meeting of the Academic Council in November or December next.

AN exhibition of electricity and electrical appliances will be held in Philadelphia, United States, commencing on Tuesday, September 2, 1884, under the auspices of the Franklin Institute of the State of Pennsylvania for the Promotion of the Mechanic Arts. From the eminent reputation of this institution, coupled with the fact that the projected exhibition will be the first in America exclusively devoted to this important and progressing branch of science, this announcement has attracted unusual interest throughout the United States, and the exhibition will undoubtedly afford an admirable opportunity of witnessing a representative display of American discovery and invention in electricity. To increase its scientific and industrial importance, as well as to add to its attractiveness, it was determined shortly after its inception to give it an international character. The importance of the project having been properly represented to the Congress of the United States, an Act was passed to this effect, and articles intended for the exhibition will be admitted to the States free of duty. All applications should be made to the Secretary, Franklin Institute, Philadelphia, U.S.A.

THE Russian frigate *Mininch* has just started from the Baltic on a scientific voyage round the world. She has on board a number of Russian *savants* of every branch of science.

THE Prince of Wales is about to try the acclimatisation of the Norwegian ptarmigan at Abergeldie. Of sixty birds taken at Langöen in Nordland, twenty-two have just arrived at Bergen, the rest having died on the way.

THE Electric Railway from Portrush to the Giant's Causeway was opened last Friday by Earl Spencer, and among others present were Sir William Thomson, Sir William Siemens, and Sir Frederick Bramwell. It is over six miles long, and has cost 45,000*l.* The line, after passing through the principal street of Portrush, follows the seaside road, a portion of a footpath six feet broad being reserved for the railway. The gauge is only three feet, and the gradients are very steep—in places as much as one in thirty-five—and in parts of its course the curves are sharper than might have been desirable had the route which it takes been chosen by the engineers. The force to work it is generated by a waterfall in the River Bush, with an available head of twenty-four feet, the electric current being conveyed by an underground cable to the end of the tramway. The water power passing through turbine water-wheels, which utilise the whole force of the fall, is said to amount to ninety horse.

THE electrical omnibus devised by M. Philippart travelled last Sunday from the Place des Nations to Versailles. The distance is more than 20 kilometres. The experiment was successful, the only incident being a short stoppage occasioned, it appears, by the heating of a coil owing to an excess of current.

ADVICES from Colombo, under date of August 30, state that on the evening of August 27, at about 5.30, an extraordinary occurrence took place in Colombo Harbour. The sea suddenly subsided about six feet, receding from ten feet to fifteen feet, and owing to the velocity of the outward current the stern moorings of several large vessels gave way. The tide continued to rise and

fall until about six o'clock, when everything resumed its normal condition. The occurrence is attributed to the volcanic eruption at the Straits of Sunda.

THE Municipal Council of Paris having passed a resolution to lower the price of gas, the Gas Company has resisted, and a scientific commission has been appointed to decide whether the gas industry has so advanced as to justify a diminution in the price of the commodity. This commission has begun its work, which is to be terminated in a specified time, and it is surmised that the decision will be in favour of the claims of the City of Paris. The report, which will bear upon the whole of the gas industry, history, and actual state, will be at all events exceedingly interesting.

SEVERAL shocks of earthquake were felt in Agram on Tuesday night and early on Wednesday morning last week. Fortunately, the phenomenon was unattended by consequences more serious than the usual earth tremors and subterranean rumblings.

THE discussion of 1600 cases of aurora borealis observed during fifteen years at Godhaab has led M. Tromholt (NATURE, vol. xxvi. p. 130) to the conclusion that, however subject to the law of periodicity of 11·11 years, the periods of frequency at Godhaab are precisely the inverse of what has been observed under lower latitudes. The same holds good with regard to the annual and diurnal periods of frequency. Prof. Lenz, in the *Izvestia* of the Russian Geographical Society, makes an attempt to explain this circumstance by assuming that the zone of auroras (the "Nordlichtgurtel" of Weyprecht) is subject to a system of oscillations. In consequence of these it is slowly displaced towards the north, and when it has reached its most northern position a maximum of auroras is observed at Godhaab and in North Greenland, and a minimum in lower latitudes. The duration of this oscillation is the same as that of the frequency of spots on the sun, the minimum of these last corresponding to a maximum of auroras at Godhaab. The zone of auroras has also an annual period of oscillations; it seems to advance towards the north during the winter, and returns south during the summer (seeming thus to depend on temperature), as also a diurnal period of still smaller oscillations, in consequence of which it seems to be displaced towards the north during the early hours of the day. As to the cause of the connection between the auroras and sun-spots, it still remains unknown. Prof. Lenz points out, however, that it results from an analysis of the magnetic storm of January 31, 1881, that the cause of this storm was not a change in the intensity of the earth's magnetism, but merely a displacement of the region where the origin of magnetic storms must be sought for, and which probably is the zone of auroras. This zone would be submitted thus, on Prof. Lenz's hypothesis, to perturbations which appear either under the shape of auroras or as electrical currents. But might not all the phenomena mentioned be explained as well by the oscillations of Nordenskjöld's corona of auroras, and by variations in its luminous intensity produced by cosmical and telluric causes?

M. POTYLITZIN, who has submitted the waters that accompany naphtha, or are ejected by the mud volcanoes of the Caucasus, to a thorough chemical investigation, has found that they belong to two different groups. Those of the Caspian region are acid and contain almost exclusively chlorides of metals, whilst those of the north-western and southern naphtha regions of the Caucasus contain, besides a large amount of chloride of natrium, also carbonate of natrium, as well as iodine and salts of fatty acids. The presence of bromides and of iodine in these last must be probably explained by their washing out marine deposits of the Eocene, or, may be, of the Cretaceous period, which contain masses of marine organisms. Accepting Prof. Mendeleéff's theory as to the origin of naphtha, the author points out that, its primary seat being probably at a great depth, it impregnates, in consequence of its capillarity, the upper schists; but the water that continually descends from the surface down to the lower schists opposes this ascending motion of the naphtha, and a continuous struggle of both is the consequence of the two opposed movements, resulting in oscillations of the level of naphtha and of its discharge. Thus, at the Groznaya wells the amount of extracted naphtha diminishes from 54,000 gallons in the summer to 32,000 in the winter and spring, whilst at the much deeper (343 feet) well of Paolovsk the reverse is observed, the amount of extracted naptha being from 40,000 to 48,000 gallons in the winter, and only 32,000 gallons in the summer. This circumstance could be easily explained by the retardation which the water experiences in its descent to a greater depth.

THE Statistical Society announces as the subject for the Howard Medal for 1884—"The Preservation of Health, as it is affected by Personal Habits, such as Cleanliness, Temperance, &c."

RAPIDLY as new periodicals and societies with their journals and transactions are started in these days, they do not appear by a column of titles a week as books do. The Mason Science College of Birmingham, accordingly, thinks it not premature to print a first catalogue of about 6000 volumes of these most important publications—British and foreign—which in little more than two years have come into its possession. Such papers form the most fundamental literature of all science, and the wide range of subjects upon which they treat and the completeness of the series of many of those now belonging to the Mason College, will be appreciated by the student for whose service they have been brought together by this noble institution, or by any one who compares this catalogue of them with those of many other collections.

WE are informed that the ships *Dacia* and *International*, used in the expedition which is accompanied by Mr. J. Y. Buchanan, do not belong to the Telegraph Construction Company, but are the property of the Indiarubber, Guttapercha, and Telegraph Works Company, which is engaged in the work of laying the cables from Cadiz to the Canaries, and thence to Senegal, for the Spanish and French Governments.

MR. SCOTT SNELL has made some very interesting experiments on the use of asbestos paint for coating Jablochkoff candles; he finds that with pure asbestos paint the arc is much steadier and the carbons last much longer.

THE additions to the Zoological Society's Gardens during the past week include a Bonnet Monkey (*Macacus radiatus* ♀) from India, presented by the Rev. G. R. Roberts; a White-fronted Capuchin (*Cebus albifrons* ♂) from South America, presented by Capt. King; a Blotched Genet (*Genetta tigrina*), a Long-nosed Crocodile (*Crocodilus cataphractus*) from West Africa, presented by Surgeon Mosse, A.M.D.; an Egyptian Cat (*Felis chaus*) from North Africa, presented by Lieut.-Col. Mitchell Taylor; two Kittiwake Gulls (*Rissa tridactyla*), a Common Guillemot (*Uria troile*), British, presented by Mr. Cuninghame; a Herring Gull (*Larus argentatus*), a Shag (*Phalacrocorax graculus*), a Common Curlew (*Numenius arquata*), British, presented by Dr. A. Günther, F.R.S.; seven European Phyllodactyles (*Phyllodactylus europæus*) from the Island of Elba, presented by Prof. Giglioli, C.M.Z.S.; a Robben Island Snake (*Coronella phocarum*) from South Africa, presented by the Rev. G. H. R. Fisk, C.M.Z.S.; a Pig-tailed Monkey (*Macacus nemestrinus* ♂) from Java, a Common Curlew (*Numenius arquata*), an Oyster-catcher (*Hæmatopus ostralagus*), British, deposited; a River Jack Viper (*Vipera rhinoceros*) from West Africa, seven Short-nosed Sea Horses (*Hippocampus antiquorum*) from the European Coast, purchased.

OUR ASTRONOMICAL COLUMN

The Reappearance of Pons' Comet of 1812.—Probably the comet discovered by Mr. Brooks at Phelps, N.Y., on September 2, would have been earlier identified with the expected comet of 1812, had not the sweeping ephemerides issued by MM. Schulhof and Bossert been confined to an arc of ± 90° of true anomaly, whereas the comet has been detected at an orbital angle of 113° from the perihelion—a greater distance than might have been anticipated. In the *Comptes Rendus* of September 17 they have made a provisional c rrection of the orbit, fixing the perihelion passage to 1884, January 25·82434 M.T. at Berlin. The following positions are extracted from an ephemeris accompanying the communication:—

At Berlin Midnight

	R.A. h. m. s.	Decl. ° ′	Log. distance from earth.	I.
Oct. 5	16 29 48	+57 55·7	0·2984	2·04
7	16 31 14	57 30·2	0·2928	
9	16 32 50	57 4·9	0·2871	2·26
11	16 34 38	56 39·9	0·2812	
13	16 36 36	+56 15·1	0·2751	2·52

Here the intensity of light at discovery on September 2 has been taken as unity.

The comet will be observable in this hemisphere up to perihelion passage, and in the other hemisphere may probably be followed until midsummer or later. The following approximate positions are derived from MM. Schulhof and Bossert's corrected elements:—

At Greenwich Midnight

1884.	R.A. h. m.	Decl. ° ′	Log. distance from Earth.	Sun.
Jan. 28	0 19·7	−23 25	9·8966	9·8901
Feb. 25	1 31·1	45 37	0·0637	9·9841
March 26	2 37·4	57 29	0·1413	0·1252
April 25	4 43·8	67 2	0·1760	0·2387
May 25	8 27·5	68 36	0·2270	0·3268
June 24	11 9·0	−59 56	0·3171	0·3972

The comet will arrive at its least distance from the earth on January 9, and as the moon draws off in December may be expected to be a naked-eye object.

A New Comet.—The *Dun Echt Circular*, No. 81, and the *Astronomische Nachrichten* notify the discovery of a comet by Mr. Lewis Swift at the Warner Observatory, U.S., on September 11. The following approximate positions are given:—

	G.M.T.	R.A. ° ′	Decl. ° ′
September	11·000	280 29	+73 9
	13·501	276 30	73 8

M. Trouvelot's Red Star.—It has been mentioned that during the totality of the solar eclipse of May 6, at Caroline Island, M. Trouvelot saw a decidedly red star "a little to the north and a little to the west of the sun." He now states that on September 5 and 7 he examined the part of the sky where the sun was then situate with a telescope of the same aperture that he used in observing the eclipse, and with the eyepiece then employed he recognised the two white stars which he had noted as 41 and ε Arietis, but the red star was not found, even though he swept to a much greater distance than any probable error of his observation would allow. On this circumstance he remarks: "Bien que l'absence d'une étoile rouge aussi brillante que celle que j'ai observée durant l'éclipse semble tout naturellement conduire à supposer que l'astre en question n'était autre qu'une planète intra-Mercurielle, cependant, comme les éléments les plus nécessaires, tels que la position et un disque ou une phase sensible, manquent à mon observation, je crois qu'il est de mon devoir de me tenir sur la réserve et de suspendre quant à présent mes conclusions sur la nature probable de cet astre."

The place of the sun at the middle of totality at Caroline Island was in R.A. 2h. 52m. 28s., Decl. + 16° 31′·0 for the epoch of the *Durchmusterung* (1855 0).

GEOGRAPHICAL NOTES

A most improbable report appears in a Danish paper as to the violent death of Lieut. Greely, of the U.S. Arctic Expedition. It is stated that the information was obtained from some Eskimo by Hans Hendrik, who accompanied Dr. Nathorst to Cape York; but Baron Nordenskjöld's telegram and the letters from Dr. Nathorst which we print to-day evidently prove that the report is quite untrustworthy.

The last number of the *Izvestia* of the Russian Geographical Society brings us further information about the proposed expedition of Col. Prejevalsky to Thibet. The indefatigable Central-Asian traveller has been taught by experience that one of the greatest difficulties during long journeys is the transport of the scientific collections which steadily grow as the traveller advances on his journey. He intends, therefore, to leave them at several stations, where a few of his men will remain with the collections, continuing at the same time the exploration of the surrounding country. He will start from Kiakhta for Urga, leaving that town this month for the Tsaidam, *viâ* the Alashan and Kookoo-nor. At Dzun-zasak he will establish his first station. Leaving it in February next, he proposes to go towards the Yellow River and the towns Chamdo and Batang. If circumstances be favourable, he will spend the summer in the land of the Si fans, situated between Lake Kookoo-nor and Batang, which land promises a rich crop of scientific information. If it be impossible, he will explore the eastern part of the plateau of Northern Thibet, and return to his station, take there his luggage, and transport it to Hast in the Western Tsaidam, where a second station will be established. Therefrom he will try to penetrate into Northern Thibet, towards Lhassa and Tengri-nor. If he succeeds in that, M. Prejevalsky will go either to the Dzang Province, and thence follow up the Brahmaputra, or north-west towards Ladak and Hast, which may be reached about the spring of 1885. The expedition would be divided there into two parties going by two different routes to Lob-nor, thence to Karakorum and, *viâ* Ak-sou, to Issyk-koul in Turkestan. Such is the scheme of this great expedition, which is intended to bring within the domain of science such parts of Thibet as we know only from the descriptions of the pundits and of a few missionaries. The more than 50,000 roubles which are necessary for covering the expenses have already been granted by the Russian Government.

We learn from the same periodical that two other expeditions of great interest have been organised by the Geographical Society for this summer. M. Adrianoff, who already has made explorations in the Altay and Sayan Mountains, will explore the highlands west of Minusinsk; and Dr. Regel, who has spent the winter at Barpanj, at the foot of the Pamir, has received the means for pursuing his explorations of the Pamir in the direction he will find most convenient. M. Potanin, who is about to start for a new expedition to Southern Mongolia and Hansoo, intends also to establish one or two stations, where part of his luggage will be left, with some of his men, who will there make meteorological observations. Thanks to a gift of 15,000 roubles, which was made for this purpose by M. Soukacheff, the expedition of M. Potanin will be accompanied by a topographer, and by M. Berezovsky, who will make scientific collections. They have started on board the frigate *Minin*, which will land them at Pe-che-li, whence they will proceed, *viâ* Peking, to Min-jou, and establish there their first station.

The chief of the Dutch expedition, Dr. Snellen, has made the following report to the Meteorological Institute of Utrecht:—On October 9, 1882, the scientific observations were commenced. On November 3 the vessel began to suffer from the ice, for which reason we deemed it advisable to camp in tents on the ice. On November 8 we again went on board, the vessel having been made habitable. On December 7 a hut for observations was erected on the ice, but on the 8th the ice again began to drift, separating the same from the ship. It was afterwards recovered. On December 24 the *Varna* was so damaged by the ice that it was dangerous to remain on board, and we consequently went on board the *Dijmphna*, where our observations were for a time continued. On January 15 the observations in the hut on the ice were resumed. On January 25 the greatest cold—47°·2 C.—occurred. On April 6 the first water was seen in the ice, and in the beginning of June the road between the ship and the vessel became impassable through the thawing of the ice. On June 11 the ice began to move, and became loose around the *Dijmphna*. On June 22 new ice one centimetre in thickness was formed. On July 24 the *Varna* foundered. On August 1 the expedition and the crew of the *Varna* left the *Dijmphna* with boats and sleighs. On August 16 land was seen. On the 19th an island in the Kara Strait was passed, and on the 20th we landed on Waigatz Island. On the 25th we

met the steamers *Nordenskjöld*, *Obe*, and *Louise*, the last named of which the expedition went on board. Outside Yugor Schar the *Louise* lost her propeller, and had to be taken in tow to Hammerfest by the *Nordenskjöld*. On August 30 we landed at Vardö.

THE leading paper in Heft ix. of Petermann's *Geographische Mittheilungen* accompanies a map of the two principal, almost exclusive, nationalities of Bohemia, the Germans and the Czechs. At the last census of Austria-Hungary, on December 31, 1880, the Czechs in Bohemia amounted to 3,470,252, the Germans to 2,054,174; in all, 5,524,426. Unfortunately the column of the census paper designed for the specifications of the nationality of each inhabitant was headed "Umgangssprache" (literally, the language of ordinary intercourse), a word by no means best calculated to educe in any case the national leaning of the person filling it up. On comparing this last census with former ones, it appears that the German and Czech elements maintain about the same numerical relation to each other in Bohemia as they had continued to do throughout the three previous decades. Their present proportions are thirty-seven Germans to sixty-three Czechs. The Czechs are in strongest force in the centre, while the surrounding provinces, especially those of the north-west, are chiefly occupied by the Germans. Local fluctuations in the relative proportions of the two nationalities occur principally in the districts where the two are most mixed or where they border on each other in industrial, manufacturing, and mining, especially extensive coal-mining districts, and in places in which there has been a rapid increase of population. In almost none of these cases, however, has the former character of any quarter been changed. It is observable that the Germans in predominatingly Czech districts generally cohere in isolated communities, whereas the Czechs in corresponding cases are disposed to assimilate to the preponderating foreign element. In proportions ranging as low as from one per cent. to one per thousand, Germans are to be found everywhere throughout Bohemia, except in the district of Blatna, where they muster only 43 against 52,522 Czechs. Czechs, again, are totally wanting in the Asch and Plau districts, and number less than one-thousandth of the population respectively of Gabel, Graslitz, Schluckenau, and Tepl. Elsewhere in proportions ranging from one-thousandth to one per cent. and upwards, they are diffused all over the kingdom. Other nationalities than those of Germans and Czechs are found in Bohemia in diminishingly small numbers: Poles reaching 1303, Ruthenes 1285, Italians 141, other nationalities falling short of the number of 100. It is further found that while Bohemians in foreign countries, chiefly in Western Austria and Germany, amount to 490,565, the number of foreigners in Bohemia is only 80,236, drawn, too, chiefly from Western Austria and Germany.

DR. EMIN BEY, continuing his tour through the Mudiriё Rohl, gives a description of the country he traversed between Biti and Bufi, more particularly the river Lau or Doghúrguru, as, along with other names, it is variously called by the natives in various parts of its course. Rumbehk, the principal place of the Mudiriё Rohl, and the Agahr, and other Dinka tribes are next described; then the country passed in traversing the province of Gohk as far as the Roah River, and back to Jalo; the Lori land and the Upper Jalo to Sajadihn, with the march back to Ladò.

IN another article an interesting sketch is given regarding the progress of the cartography of the peninsula of Corea, accompanying which is a map of the country based on the one published in 1875 by the Ministry of War at Tokio, and embracing all the latest tracings of the coast.

THE last volume (thirty-eighth) of the *Memoirs of the Topographical Department of the Russian General Staff* contains, besides the usual reports on the geodetical and topographical operations in Russia, the following memoirs:—On the measurement of the base on a string during the trigonometrical survey of Bulgaria, by Col. Lebedeff; on the measurements of the pendulum made in India, by General Stebnitsky; results of levellings made during the years 1871 to 1877 along Russian railways, by Col. Tillo. It results from these levellings, which were made with a very great degree of accuracy, that the level of the Baltic Sea at Dunamunde is 2·10 feet lower than at Cronstadt. The possible error is ±0·91 feet.

M. LESSAR has written to the Russian Geographical Society from Askabad, on June 16, that he has explored the Ongouz River, which was known only in its upper parts. Even the Tekkes did not know the route to the east of Mirza-chile. The journey was very difficult. The bed of the Ongouz being very undefined, the expedition often lost its way. The *kaks* or cisterns were empty, as there was not a single strong rain in April. Still M. Lessar reached Kavakhly, and thence proceeded to Khiva, whence he returned to Askabad *viâ* Mirza-chile. When writing his letter he was ill, and unable to continue his journey.

THE BRITISH ASSOCIATION

REPORTS

Report of the Committee, consisting of Lieut.-Col. Godwin-Austen, Dr. G. Hartlaub, Sir J. Hooker, Dr. Günther, Mr. Seebohm, and Mr. Sclater (Secretary), appointed for the purpose of investigating the Natural History of Socotra and the adjacent Highlands of Arabia and Somali Land.—Prof. Bayley Balfour's labours on the botanical collection made in Socotra are nearly brought to a close, and the results will shortly be published in a volume of the *Transactions of the Royal Society of Edinburgh*. The value and completeness of this memoir will be much increased by the additional specimens subsequently obtained in Socotra by Dr. Schweinfurth, which have been lent to Prof. Balfour by the collector. The fresh-water shells collected by Prof. Balfour have been described by Lieut.-Col. Godwin-Austen in a paper read before the Zoological Society of London in January last, and published in the first part of their *Proceedings* for the present year. The Diatomaceæ have been examined by Mr. Kilton of Norwich, and described in a paper which will be read before the Zoological Society of London during their next session.

Report of the Committee, consisting of Sir Joseph Hooker, Dr. Günther, Mr. Howard Saunders, and Mr. P. L. Sclater (Secretary), appointed for the purpose of Exploring Kilimanjaro and the adjoining Mountains of Eastern Equatorial Africa.—The Committee having been unsuccessful in obtaining the services of a conductor for this expedition, nothing has been done.

Report of the Committee, consisting of Mr. John Cordeaux (Secretary), Mr. J. A. Harvie-Brown, Mr. P. M. C. Kermode, Prof. Newton, Mr. R. M. Barrington, and Mr. A. G. More, reappointed at Southampton for the purpose of obtaining (with the consent of the Master and Brethren of the Trinity House, and the Commissioners of Northern and Irish Lights) Observations on the Migration of Birds at Lighthouses and Lightships, and of reporting on the same.—The General Report of the Committee, of which this is in fact an abstract, comprises the observations taken at lighthouses and light-vessels, and a few special land stations, on the east and west coasts of England and Scotland, the coasts of Ireland, Isle of Man, Channel Islands, Orkney and Shetland Isles, the Hebrides, Faroes, Iceland, and Heligoland, and one Baltic station—Stevns Fyr on Stevns Klint, Zealand, for which the Committee is indebted to Prof. Lütken of Copenhagen. Altogether 196 stations have been supplied with schedules and printed instructions for registering observations, and returns have been received from about 123—a result which is very satisfactory, showing as it does the general interest taken in the work, and the ready cooperation given by the lightkeepers in assisting the Committee.

As in preceding years, the line of autumn migration has been a broad stream from east to west, or from points south of east to north of west, and covering the whole of the east coast. In 1880, to judge from the returned schedules, a large proportion of the immigrants came in at the more southern stations; in 1881 they covered the whole of the east coast in tolerably equal proportions; but in 1882 the stations north of the Humber show a marked preponderance of arrivals. Altogether a vast migration took place this year upon our east coast, the heaviest waves breaking upon the mouth of the Humber, Flamborough Head, the Farne Islands, Isle of May at the entrance to the Firth of Forth, and again, after missing a long extent of the Scotch coast, at the Pentland Skerries. The Bell Rock also came in for a share, although apparently a much smaller one than the Isle of May. The easterly winds prevailed all along our east coasts, generally strong to gales, and the succession of south-easterly and easterly gales in October, between the 8th and 23rd, occurring as they did at the usual time of the principal migration, brought vast numbers of land birds to our shores. From the Faroes in the north to the extreme south of England this is found to have been the case.

Although migration—that is, direct migration—on our east coast, is shown to have extended over a long period, commencing in July and continuing, with but slight intermissions, throughout the autumn and into the next year to the end of January, yet the main body of migrants appear to have reached the east coast in October, and of these a large proportion during the first fortnight in the month. From the 6th to the 8th inclusive, and again from the 12th to the 15th, there was, night and day, an enormous rush, under circumstances of wind and weather which, observations have shown, are most unfavourable to a good passage. During these periods birds arrived in an exhausted condition, and we have reasons for concluding, from the many reported as alighting on fishing smacks and vessels in the North Sea, that the loss of life must have been very considerable. Large flights also are recorded as having appeared round the lanterns of lighthouses and light-vessels during the night migration. From the 6th to the 9th inclusive strong east winds blew over the North Sea, with fog and drizzling rain, and from the night of the 12th to 17th very similar weather prevailed. Mr. W. Littlewood, of the *Galloper* lightship, forty miles south east of Orfordness, reports that, on the night of October 6, larks, starlings, tree-sparrows, titmice, common wrens, redbreasts, chaffinches, and plovers were picked up on the deck, and that it is calculated that from 500 to 600 struck the rigging and fell overboard: a large proportion of these were larks. Thousands of birds were flying round the lantern from 11.30 p.m. to 4.45 a.m., their white breasts, as they dashed to and fro in the circle of light having the appearance of a heavy snowstorm. This was repeated on the 8th and 12th, and on the night of the 13th 160 were picked up on deck, including larks, starlings, thrushes, and two redbreasts. It was thought that 1000 struck and went overboard into the sea. It is only on dark, rainy nights, with snow or fog, that such casualties occur; when the nights are light, or any stars visible, the birds give the lanterns a wide berth.

Undoubtedly the principal feature of the autumn migration has been the extraordinary abundance of the gold-crested wren. The flights appear to have covered not only the east coast of England, but to have extended southward to the Channel Islands and northward to the Faroes (see Report, East Coast of Scotland). On the east coast of England they are recorded at no less than twenty-one stations from the Farne Islands to the Hanois Lighthouse, Guernsey, and on the east coast of Scotland at the chief stations from the Isle of May to Sunburgh Head (at which latter station they have rarely been seen in previous years). Mr. Garrioch, writing from Lerwick, says: "In the evening of October 9 my attention was called to a large flock of birds crossing the harbour from the Island of Bressay, and on coming to a spot on the shore where a number had taken refuge from the storm, I found the flock to consist of gold-crests and a few fire-crests amongst them; the gold-crests spread over the entire Island, and were observed in considerable numbers till the middle of November." The earliest notice on the east coast is August 6, the latest November 5, or ninety-two days; they arrived somewhat sparingly in August and September, and in enormous numbers in October, more especially on the nights of October 7 and 12, at the latter date with the woodcock. This flight appears to have extended across England to the Irish coast, for on the night of the 12th a dozen struck the lantern of the Tuscar Rock Lighthouse, and on the night of the 13th they were continually striking all night. During the autumn enormous numbers crossed Heligoland, more especially in October. On the night from the 28th to the 29th Mr. Gätke remarks: "We have had a perfect storm of gold-crests, perching on the ledges of the window-panes of the lighthouse, preening their feathers in the glare of the lamps. On the 29th all the island swarmed with them, filling the gardens and over all the cliff—hundreds of thousands. By 9 a.m. most of them had passed on again." Not less remarkable was the great three days' flight of the common jay, past and across Heligoland, on October 6, 7, and 8. Thousands on thousands, without interruption, passed on overhead, north and south of the island too, multitudes like a continual stream, all going east to west in a strong south-easterly gale. It would have been interesting if we had been able to correlate this migration of jays with any visible arrival on our English coast, but in none of the returns is any mention made of jays. Subsequently we have received numerous notices of extraordinary numbers seen during the winter in our English woodlands. This seems especially to have been the case south of a line drawn from Flamborough Head to Portland Bill in Dorset. Additions and unusual numbers were also observed at Arden on Loch Lomond side.

The returns show very clearly that the spring lines of migration followed by birds are the same as those in the autumn, but of course in the reverse direction—from west and north-west to east and south-east. Another point worth noting is the occurrence of many species in spring at the same stations frequented by the species in autumn. Thus double records occur at the Mull of Galloway, Bell Rock, Isle of May, as well as at some English stations.

As this is the fourth report issued by the Committee, we may perhaps, with the mass of facts at our disposal, be expected to draw deductions which, if they do not explain, may serve at least to throw some light on the causes influencing the migration of birds. We might reasonably reply that the work undertaken by us was not to theorise, or attempt explanations, but simply to collect facts and tabulate them; this we have endeavoured to do, in the shortest and simplest manner consistent with accuracy of detail. There is, however, one circumstance which can scarcely fail to present itself to those who have gone carefully into the reports issued by the Committee, namely, the marvellous persistency with which, year by year, birds follow the same lines, or great highways of migration, when approaching or leaving our shores. The constancy of these periodical phenomena is suggestive of some settled law or principle governing the movement. It is clearly evident, from the facts already at our disposal, that there are two distinct migrations going forward at the same time, one the ordinary flow in the spring and ebb in the autumn across the whole of Europe. A great migratory wave moves to and from the nesting-quarters of the birds, in the coldest part of their range, north-east in the spring and south-west in the autumn. Quite independent of this there is a continual stream of immigrants, week by week and month by month, to the eastern shores of these islands, coming directly across Europe from east to west, or more commonly four points south of east to north of west, and the reverse in the spring. These immigrants are mainly composed of those common and well-known species which annually make these islands their winter quarters, and, as a rule, take the place of our summer birds. They come in one broad stream, but denser on some special lines or highways than others. Cutting the line of ordinary migration at nearly right angles, one flank brushes the Orkney and Shetland Isles, pouring through the Pentland Firth, even touching the distant Faroes; the southern wing crosses the Channel Islands, shaping its course in a north-westerly direction to the English coast.

Ninth Report of the Committee for investigating the Circulation of Underground Waters in the Permeable Formations of England, and the Quantity and Character of the Water supplied to various Towns and Districts from these Formations. Report drawn up by C. E. De Rance.—Ten years having elapsed since the Committee was appointed at Belfast, they think this a fitting opportunity to review the results so far obtained, and to point out where they consider additional information is still required, in the hope that they may receive assistance in their investigations from the various local societies or from individuals who may be disposed to aid in the work. The work intrusted to the Committee was twofold—first, to inquire into the circulation of underground waters in permeable formations; secondly, to ascertain the quantity and quality of the water supplied to towns and districts from these formations. The information obtained occupies nine reports; the eight already published fill up no less than 163 pages of the annual volumes of the Association, and contain a record of upwards of 500 wells and borings. The Committee believe that the publication of these results, by directing public opinion to the value of such supplies, and by the preservation of the records of those carried out, has given an impetus to water of this class being generally adopted for domestic consumption in districts where gravitation supplies are unsuitable or unattainable. As regards the first head of inquiry—the circulation of underground water—much remains to be learnt, especially as to the influence of variation of barometrical pressure on the volume of springs. Independent investigation is now being carried on by Mr. Baldwin Latham, but it is exceedingly desirable that numerous observations should be taken in different classes of rocks, the quantity of water a rock is capable of holding being no measure of the quantity of water it is capable of yielding. The difference of the period of time in which two rocks will absorb, and give off by gravity, the same quantity of water is governed by the difference of their chemical composition. The chemical composition of two rocks being identical, their facility of discharge of water is in direct relation

to the amount by which they are traversed by planes of joints and fissures, and the extent these may run parallel or at right angles to the valleys which cut into and expose the water-bearing beds. The proportion of the annual rainfall that is absorbed by different classes of rocks is a subject that requires further examination. The quantity is largely regulated by the quantity stored from previous years. After a succession of dry years the permanent water-level is reduced to minimum figures, and the water gradient becomes nearly flat and springs cease to flow. The first heavy rains will be nearly wholly absorbed, until the maximum water-gradient is reached and the rocks are stored with the largest amount of water they can hold. After they are once charged, all excess of rainfall runs off in floods, and the amount absorbed is practically *nil*. Spread over the twelve months, the annual amount absorbed is probably never more than fifteen inches, and the average ranges from five inches in chalk countries to ten inches in new red sandstone areas. In millstone grit districts about eight inches are absorbed, but the permeable beds are thin, and the water is thrown off again in numerous springs, as a rule in the same drainage basin, giving permanence to the dry-weather flow of the streams traversing them. Except in *waterworks drainage areas* but few observations exist as to the actual volumes run off daily by the rivers of this country, and data on this subject are much required, as well as a permanent record of the height to which floods rise in the various river basins. Further observations are required as to the action of faults in acting as ducts, along the face of which water is constantly passing, and barriers separating districts into distinct drainage areas. The facts so far obtained point to faults traversing thick permeable sandstone and limestone, having their formations on both sides of the dislocation, as offering no obstacle to the free passage of waters, which, even if locally obstructed by the hardened face or slickenside jointing of the fault, invariably finds its way through cracks extending across the width of the fault to faults traversing thick shales and clays of any age. The fissure, be it wide or narrow, always appears to have been filled with the impermeable material forming the sides, and in some cases, when porous rocks have been immediately overlaid by impermeable material since denuded, the fissure of the fault has been filled from above at a time when the fault had an upward prolongation, destroyed with the denuded material referred to. The daily registration of the heights of the streams might easily be made on gauges, painted on the county bridges, but the organisation necessary to carry this out is entirely beyond the scope of the British Association, and should be carried out at the national charge, being of the highest importance to the country. The determination of the number of cubic feet of water carried down at selected points on the English rivers, particularising whether it represents *dry-weather*, *average*, or *flood flow*, would be of very high value, and might well be undertaken by the Association. Such observations, stating the run-off per square mile of drainage area and the geological character of the area drained, would have more than a *local* value. Permeable rocks below the permanent water-level of a district may be regarded as a reservoir of which the cubic content is limited by the size of the spaces between the grains, and the width of the fissures and cracks by which the rock may be traversed. The quantity of water such rocks are capable of storing has had much light thrown upon it by the investigations of Mr. Wethered, published in the fourth appendix to the eighth report.

Third Report of the Committee, consisting of Mr. Sclater, Mr. Howard Saunders, and Mr. Thiselton Dyer (Secretary), appointed for the purpose of investigating the Natural History of Timor Laut.—In the month of January a box containing seventy birds' skins was received from Mr. Forbes, with the note, "This first instalment of birds is a rough selection, which, probably, may contain new species." The collection was examined by Mr. Sclater, who communicated an account of it to the meeting of the Zoological Society on February 20. The species were fifty-five in number, sixteen of which were described in the paper as new to science. "The general facies of the avifauna, as thus indicated, was stated to be decidedly Papuan, with a slight Timorese element, evidenced by the occurrence of certain species of *Geocichla* and *Erythrura*, while the new one (*Strix sororcula*) was apparently a diminutive form of a peculiar Australian species." About the same time the Committee received from Mr. Forbes a detailed report of his proceedings in Timor Laut. This was an extremely interesting document, but dealt principally with ethnographical details. The Committee, therefore, decided that it should be communicated at once to the Anthropological Institute; and this Mr. John Evans, Treasurer of the Royal Society and Vice-President of the Institute, very kindly undertook to do. The paper was read at the meeting on March 13, and has since been published in the *Journal* of the Institute. In February the bulk of Mr. Forbes's collections reached Kew in four cases. They contained an extremely complete ethnographical collection, a further collection of birds, a collection of twelve crania and specimens of human hair, and a miscellaneous zoological collection. The Committee decided that a selection from the ethnographical collection should be handed to Mr. Franks, keeper of the Department of Ethnography in the British Museum; that the additional birds should be examined by Mr. Sclater, and that the miscellaneous zoological collections should be sent to the zoological department of the British Museum to be selected from. This was accordingly done. A series of the ethnographical specimens was sent to the meeting at the Anthropological Institute to illustrate the reading of Mr. Forbes's report, and a description of these, drawn up by Mr. C. H. Read, is printed as an appendix to the paper in the *Journal* of the Institute. Prof. Flower, who presided on the occasion, also stated that "the results of a cursory examination of the twelve crania which Mr. Forbes had collected were that eight were brachycephalic, and of decidedly Malay type; one was dolichocephalic, prognathous, and with large teeth, indicating Papuan or Melanesian affinities; and the other three were more or less intermediate. This is what might have been expected on the border-land of two distinct races; but the great preponderance of the first-named was very marked. Nearly all showed signs of artificial flattening of the occipital region. At the meeting of the Zoological Society on April 17, Mr. Sclater read a second paper on the additional birds collected by Mr. Forbes in the Tenimber group. "The avifauna of the group, as indicated by Mr. Forbes's collection contained fifty-nine species, of which twenty-two were peculiar to these islands." At the meeting of the same Society on May 1, Mr. W. F. Kirby reported on the small collection of Hymenoptera (five new species were described) and of Diptera sent home by Mr. Forbes. On June 5 a communication was read from Mr. A. G. Butler, containing an account of twenty-three Lepidoptera. These comprised twenty-three species of Lepidoptera; the butterflies were well preserved, the moths in poor condition. Mr. Butler described ten new species. Deducting wide-ranging forms, the following is his analysis of the characteristic species:—"Indo-Malayan, 2; Austro-Malayan, 10; Australian, 3. The only surprising thing in this distribution is the preponderance of Timor over Aru or New Guinea forms; the species characteristic of that island being only equalled by those from Aru, New Guinea, and Amboyna combined." Mr. Boulenger also reported, at the same meeting, upon the reptiles and batrachians. Two new species were described—the one a lizard of the Australian genus *Lophognathus*, and the other a snake of the Indian genus *Simotes*. "The snake was of special interest, as no species of the genus *Simotes* had bitherto been previously known to occur eastward of Java."

Report of the Committee, consisting of General Pitt-Rivers, Dr. Beddoe, Mr. Brabrook, Prof. Flower, Mr. F. Galton, Dr. Garson, Mr. J. Park Harrison (Secretary), Dr. Muirhead, Mr. F. W. Rudler, and Prof. Thane, appointed for the purpose of Defining the Facial Characteristics of the Races and Principal Crosses in the British Isles, and obtaining Illustrative Photographs.—Owing to the comparative scarcity of skulls and other remains of the earlier inhabitants of the British Islands, and the imperfect condition of many of them owing to lapse of time, more difficulty has been experienced in completing the identification of the Long-barrow type than occurred in the case of the Round-barrow and Saxon types (B and C), the features of which were defined in the Report of 1882. There appears, however, to be little doubt that the short dark type, which, as the Committee mentioned last year, certainly exists in the population at the present time, and which offers a marked contrast to the other types, accords in stature, lightness of frame, narrowness of skull, and fine osseous features generally, with the skeleton remains found in the majority of the early barrows. The Committee, therefore, have no difficulty in considering it as the main Type A; and its characteristic features have consequently been inserted in the annexed table, for comparison with Types B and C. The question whether there was a second pre-Celtic race in this country is hardly ripe for discussion; but it is receiving the special attention of several members of the Committee.

Table in which the Typical Features of the Three Principal Races in the British Isles are compared

	Features	A	B	C
a	Forehead	Vertical, square	Receding	Vertical, rounded
b	Supra-orbital ridges	Oblique [1]	Prominent, continuous across brows	Smooth
c	Cheeks	Tapering to chin	Long	Wide, full
d	Nose	Straight, long	High-bridged, projecting	Short: bulbed
e	Mouth	Lips thick, unformed	Lips thin, straight, long	Lips well formed
f	Chin	Small, fine	Pointed, projecting	Heavy, rounded
g	Ears	Rounded	Pear-shaped, channelled lobules	Oval
h	Jaw	Narrow	Large, square	Heavy
i	Eyes	Dark	Blue-grey, sunk	Blue, prominent
j	Hair	Very dark, crisp curling	Light-brown, slightly waved	Light, limp
	Skull	Dolichocephalic	Sub-Brachycephalic	Sub-Dolichocephalic
	Average height	5 feet 3 inches (m. 1·600)	5 feet 9 inches (m. 1·753)	5 feet 7 inches (m. 1·702)
	Habit	Slight	Bony, muscular	Stout, well-covered

In the mass of the population one or other type of features is found to predominate. The prevalent type differs in different localities; and the principal cause of the difference appears to be ancestral. Progress has been made in the identification of several sub-types, especially the Gaels, Picts, Angles, and Jutes. But the definitions are not at present complete. The Committee trust that whenever ancient remains are discovered which there may be reason to believe belong to the above people, or to the Long-barrow race, they may be carefully preserved, and information forwarded to the Secretary. The long bones, which are often put away, are specially required for the purpose of ascertaining stature. They request also to be informed of the existence of any skulls in local museums or private collections, that would assist in the identification of the above types. Negatives have been taken of very pure examples of the Cymric type in North Wales, and several photographs have been purchased.

Report of the Raygill Fissure Exploration Committee, consisting of Prof. A. H. Green, M.A., F.G.S., Prof. L. C. Miall, F.G.S., Jno. Brigg, F.G.S., and James W. Davis, F.S.A., F.G.S. (Reporter).—The fissure occurs in an anticlinal of limestone in Lothersdale, near Skipton. The limestone is extensively quarried, and whilst removing the limestone, the fissure, which descends almost perpendicularly, has repeatedly exhibited new sections during several years past. It was decided by the Yorkshire Geological and Polytechnic Society to investigate its contents in 1879, and a grant was made by the British Association to assist in this object. It was found that the fis ure contained, besides laminated clay and layers of sand and stones, a brown, sandy clay with rounded boulders of sandstone and limestone derived from the immediate locality, and numerous bones of animals. The latter comprise the bones, teeth, and tusks of elephant, teeth of rhinoceros, hippopotamus, hyæna, bear, and others, broken horns of the roebuck, and bones of birds. The bones are, when found, soft and friable; and, being cemented to the matrix, are frequently difficult to extricate and individualise. The Committee express their indebtedness to Mr. Spencer, the proprietor of the quarry, and to Mr. Todd, for the kind manner in which they have assisted in the operations.

Report of Committee on Erratic Blocks, presented by Dr. Crosskey.—Additional facts were reported respecting the distribution of erratic blocks. A remarkable group occurs at Crosspool, near Sheffield, at a height of 730 feet above the sea. It consists of slate rocks and tuff from the Borrowdale Volcanic series of the Lake District, Carboniferous limestone and chert from North Lancashire and North-West Yorkshire, New Red Sandstone from North Lancashire, and specimens also occur which were probably derived from the East Lowlands of Scotland, with magnesian limestone from the north-east of England. Near Clun, Shropshire, boulders from Rhayader and Machynlleth and neighbourhood are recorded. The highest boulder is upon Black Hill. It travelled from Rhayader, twenty-three miles west-south-west, and has an elevation of about 1400 feet. The Report included a description of an enormous number of boulders spread over an area of about two miles long by half a mile wide, the longer direction being south-east of Markfield, Leicestershire, from whence they were derived. It also gives an account of the erratics of the north of Hertfordshire. At Kelsall, on the ridge dividing the district draining into the Thames from that draining north and north east into the Cam, are two boulders lying about 500 feet above sea-level. The boulders noted point generally to a derivation from the Midland oolites and coal-measures, and from crystalline rocks further north. The position of many boulders in the Midland Counties and the Isle of Anglesea was also recorded.

[1] In place of "prominent brows," as in the report for 1882.

Report on the Fossil Plants of Halifax, by Prof W. C. Williamson, LL.D., and W. Cash.—Clear evidence of the existence of at least two new types of *Rachiopteris*, which are most probably stems or petioles of ferns. A third is a curious stem in which the vascular bundle approaches that of a Lepidodendron in its defined cylindrical form surrounding a cellular pith, a condition rarely seen among the ferns.

Report of the Committee to Explore Caverns in the Carboniferous Limestone in Ireland, consisting of Prof. Valentine Ball, Prof. Dawkins, and Richard J. Ussher.—The Shandon Cave, near Dungarvan, which yielded remains of extinct Post-Pliocene mammalia in 1859 and in 1875, has been explored during the past year. So far the work has imply been removing the loose material overlying the bone-bearing bed.

Fourth Report of the Committee, consisting of Dr. H. C. Sorby and Mr. G. R. Vine, appointed for the purpose of reporting on Fossil Polyzoa.—Tabulates the Cretaceous Polyzoa of the British area only. Gives the classification of Cyclostomatous Polyzoa, &c., from the Silurian to the Cretaceous epochs. Describes pseudo-polyzoan forms, and gives the bibliography of the subject.

Report of the Committee, consisting of Mr. R. Etheridge, Dr. H. Woodward, and Prof. T. Rupert Jones, on the Fossil Phyllopoda of the Palæozoic Rocks.—Gives a classified synopsis of the genera of this group and detailed descriptions of certain genera.

Report on Seismic Investigations in Japan during the Years 1882–83, by Prof. John Milne.—When in England, arrangements were made with Mr. James White of Glasgow for the construction of a seismometer which will give a complete diagram of all the sensible vibrations of an earthquake in conjunction with the time of occurrence of these vibrations. The results of observations on earth-tremors are given, which show that the pendulum is seldom completely at rest, that a vertical motion is occasionally observed in the pendulum, the style of which oscillates up and down with a rapid, tremulous movement. With sudden changes in the barometer, the motions of the pendulum are relatively very great. A second set of observations has been recorded, which are the motions of the delicate levels placed beneath glass covers.

The Reports prepared by the Chemical Committees appointed at Southampton last year were read at the opening of the Chemical Section. The Committee on Chemical Nomenclature presented an interim Report, and asked to be reappointed to complete their labours. Prof. Hartley read the *Report on the Ultra-Violet Spark Spectra*, which dealt especially with the disappearance of short lines, the lengthening of short lines, and alterations in the spectrum of carbon.

SECTION B—CHEMICAL SCIENCE

Sunspots and the Chemical Elements in the Sun, by Profs. Dewar and Liveing.—The authors, having made an examination of the spectroscopic observations of sunspots made at Greenwich, point out that the dark lines peculiar to spots are not necessarily due to new elements, for cerium and titanium in the arc give a great number of new lines, of which some show coincidences with dark lines seen in sunspots too striking to be merely accidental. Although a spot is less luminous than the photosphere it does not follow that its temperature must be less, inasmuch as the radiation of short wave-length generally increases very rapidly with the temperature, and the spectra of some of the metals most abundant in the sun, such as magnesium and iron, are stronger in the ultra-violet than in the visible part of the spectrum. The unequal widening of the Fraunhofer lines in spots has an analogy in the unequal widening of the lines of some of our elements when the density of their vapour is increased. The disappear-

ance of some Fraunhofer lines from spots has been attributed with much probability to the emission of the upper regions of the sun's atmosphere just balancing the absorption below: the rays for which this happens are those of vapours of low tension (corresponding to Mr. Lockyer's long lines) emitted by the elements in their least complex state of aggregation. The singular ray with wave-length 4923, which is a line of iron of high vapour tension, but behaves in the sun as a line of low vapour tension, being frequently seen high up in solar storms and disappearing from spots, probably belongs to some other metal as well as iron.

Mr. R. Meldola read a paper on *The Colouring Matters of the Diazo-Group*, in which he gave an historical sketch of this important class of bodies discovered by Dr. Griess, and proceeded to describe a number of new compounds in which the diazo-grouping occurred three times. These compounds prepared by the author yielded excellent dyes, specimens of which were exhibited. The great importance of these new products was shown by the fact that since their introduction the cochineal industry had gradually declined.

Mr. H. B. Dixon exhibited tubes in which a dried mixture of carbonic oxide gas and oxygen was submitted to the electric spark. The tubes were shaped like the letter W, the two outer arms being open and sealed with mercury in the two lower bends. In one arm of each tube anhydrous phosphoric acid had been introduced to dry the gaseous mixture. The tubes had been so charged for a period of three days. On passing the spark at the top of the central bend, a very slow and quiet combustion was propagated down the tube in which no phosphoric acid had been placed, but no combustion was propagated down the tube containing the phosphoric acid. In an experiment with another similar tube, only a small fraction of the mixture ignited in one arm. Water was then introduced by a pipette into the mixture, and after the steam had diffused, the spark was passed, producing a loud explosion.

Prof. A. W. Williamson in discussing the *Chemical Constitution of Matter* remarked that when any sufficiently careful attempt has been made to decompose one of our elementary substances this attempt has always failed. Referring to Prout's hypothesis that the atomic weights of the elements were exact multiples of that of hydrogen, Dr. Williamson showed that this idea had been fruitful because it had led chemists to make most accurate and conscientious determinations of atomic weights. The result of the labours of Mendeléeff, Lothar Meyer, and others has shown that the elements belong to a natural family, and has given an authority to the established weights which could not be assigned to them previously on chemical or physical grounds. When chemists speak of matter, they always limit themselves to that which can be weighed: it would be better to throw off that limitation and not hamper our ideas with a condition which may some day have to be removed. What many chemists have regarded as the most fundamental property of matter, its weight, may not be an inherent property in the matter itself, but may depend on forces reacting between the "matter" and the ether surrounding it. All that we know about the atomic weight of atoms is not inconsistent with varieties among individual atoms, but only proves that the average weight of large aggregates of atoms is the same.

Prof. Dewar read papers, by himself and Mr. A. Scott, on *The Atomic Weight of Manganese* and on *The Molecular Weights of Substituted Ammonias*, in the latter of which the authors pointed out the advantage of using the molecular weights of these compounds for accurately determining the relation between the atomic weights of hydrogen and carbon of which elements several atoms are contained in the introduced radicle. The authors conclude from their experiments that if oxygen be taken as 16, the atom of hydrogen must be less than unity, and not larger, as is the generally received opinion.

Prof. W. Ramsay, in a paper on *The Application of Bisulphide of Carbon to the Scouring of Wool*, drew attention to a curious difference in the quality of the bisulphide manufactured in France and England.

The Rev. W. A. Irving exhibited tubes in which trioxide of phosphorus had been sealed up and exposed to sunlight. The tubes contained dark crystals of phosphorus. The author stated that on opening the tubes he found pentoxide of phosphorus present, and argued that the sunlight had decomposed the trioxide into free phosphorus and the pentoxide. In the discussion it was suggested that free phosphorus might have been sealed up together with the oxide, and have changed its condition on exposure to sunlight.

Prof. Dewar pointed out an important relation between the critical temperature and pressure of volatile liquids and their molecular volumes. The ratio of the critical pressure to critical temperature is proportional to the molecular volume, so that the determination of the critical temperature and pressure of a substance gives us a perfectly independent measure of the molecular volume. Prof. Dewar pointed out the great advantage of employing a liquid of low critical temperature and pressure such as liquefied marsh gas for producing exceedingly low temperatures. He hoped to be able to approach the absolute zero by the evaporation of liquefied marsh gas whose critical temperature was less than $-100°$ Centigrade, and whose critical pressure was only 39 atmospheres.—Sir W. Siemens hoped Prof. Dewar would soon succeed in producing a temperature near to the absolute zero, as he had the greatest desire to test at such a low temperature the magnetic and electric behaviour of metals.

Dr. Gladstone, in a paper written in conjunction with Mr. Tribe, on *The Electrolysis of Dilute Sulphuric Acid in Secondary Batteries*, was led to the conclusion that besides the molecular change in the electrolyte, there was also an actual passage of sulphuric acid into the limb containing sulphate of copper. No data exist to decide the question whether it is sulphuric acid or some hydrate of it that is electrolysed, but analogy would lead to the conclusion that it is sulphuric acid.

Mr. H. Brereton Baker, in a paper on *The Alleged Direct Union of Hydrogen and Nitrogen*, described the carefully conducted experiments he had made with nitrogen, derived without heat from the air, and pure hydrogen. These gases led over hot platinum sponge gave no trace of ammonia. He found that, in an apparatus similar to that used by Mr. Stillingfleet Johnson, the oxides of nitrogen produced by the passage of hydrogen through the nitrate of silver solution used to purify it were not completely arrested by the ferrous sulphate absorbers, so that the ammonia produced in Mr. Johnson's experiments was doubtless due to the action of these oxides on hydrogen in presence of hot platinum.

Messrs. Friedel and Crafts communicated a paper on *The Decomposition of Hydrocarbons by Aluminic Chloride*. Chloride of aluminium is not only a synthetical agent but also a reducing agent causing the substitution of hydrogen for methyl, ethyl, &c. For instance naphthalin distilled with 25 per cent of aluminic chloride gave a distillate of benzene and hydrides of naphthalene. Benzene, heated to 235° C. in a sealed tube with the chloride, gave off marsh gas on opening, and the contents of the tube on distillation with water gave hydrocarbons boiling at from 80° to 160°. Diphenylmethane, distilled with chloride, gave a distillate containing benzene and toluene. Triphenylmethane distilled with more than half its weight of chloride gave only benzene. Hexamethylbenzene heated with one-third its weight of chloride gave off plenty of a non-illuminating gas; from the residue crystals of durene were deposited. In the case of the poly-methyl benzenes one or more methyl groups are replaced by hydrogen with the formation of very little hydrochloric acid. The same equation previously adopted to explain the synthesis of hydrocarbons by aluminic chloride, is sufficient to explain the present decomposition:—$C_6H_6 + Al_2Cl_6 = C_6H_5.Al_2Cl_5 + HCl$. The compound $C_6H_5.Al_2Cl_5$, is broken up by heat into diphenyl and aluminous chloride; the latter is decomposed by the free hydrochloric acid into aluminic chloride and hydrogen, and the hydrogen thus set free exerts the reducing action. The Section recommended the paper for publication *in extenso* in the *Transactions*.

Prof. B. Warder of Ohio, U.S.A., communicated a short paper called *Suggestions for Computing the Speed of Chemical Reactions*. He recommended for unit of volume the cubic centimetre, for mass the chemical equivalent expressed in milligrams, and for time the hour. Prof. Warder drew attention to the fact that many determinations of the rate of etherification had been published for twenty years, and yet no mean value of the "rate-constant" had been worked out. Such calculations might fitly be undertaken by students at colleges, and the Chemical Section of the Ohio Institute had begun such work and invited the assistance and cooperation of chemists engaged in teaching.

Mr. P. M. Parsons gave an account of different varieties of manganese-bronze prepared by heating copper with ferro-manganese. The spiegeleisen, as in the Bessemer process, removes the oxygen from the copper, with which part of the manganese forms an alloy of extraordinary tensile strength. One of the varieties, capable of resisting a great transverse strain, is largely employed for making screw-propellers. These are cast in sand.

SECTION E
GEOGRAPHY

OPENING ADDRESS BY LIEUT.-COLONEL H. H. GODWIN-AUSTEN, F.R.S., F.G.S., F.R.G.S., &c., PRESIDENT OF THE SECTION.

MY predecessor, Sir Richard Temple, selected for the subject of his address to this Section last year "The Central Plateau of Asia," and he treated it not only from a broad and general geographical, but also, and to some extent, a political and historical, point of view. Following him, in a measure, over some of the same ground, I have selected the mountain region south of the Central Asian highlands—viz. the Himalayas, and more particularly the western portion of that range, as the subject of this paper. I propose considering this mountain chain with reference to its physical features, past and present; and consequently with reference to its geological history, so far as that relates to later tertiary times—*i.e.* the period immediately preceding the present distribution of seas, land, rivers, and lakes. It is not, however, my intention to enter very deeply into the purely geological branch of the subject.

Comparatively little of the earth's surface now remains unexplored, but much remains to be surveyed and examined in a more scientific manner. Within the last fifty years explorers have made known to us the general features of those dotted or blank spaces which, as boys, we used to look at in our school atlas sheets with so much curiosity, mingled with no little desire to discover the hidden secrets of the unknown lands so shown. The student of the present day enjoys information more or less accurate respecting countries which to us were mere speculative shadows.

But there are other atlas sheets beneath, and only a very few feet beneath, those of this present day, which are closely connected with the latter, and beneath them again others lie still deeper which have modified the geography of this earth over and over again. It is to such a sheet or two relating to the great Himalayan chain that I now invite your attention. If we wish to deal with physical geography (and to my mind it has equal charms with either pure geography or exploration), our inquiry must, if we wish it to be of any really scientific value, be based on geological structure. We must study the ancient atlas sheets, one by one, which nature is, day by day, revealing to us by the denudation of the present surface, taking away and building up the material for atlas sheets of future epochs. Geography and geology are very intimately related; each is truly based upon the other. Local changes of temperature on the surface of this earth, and internally the slow shrinking of its crust, have effected gigantic changes of its surface, and are still altering the topographical features of every country. Directly we look back in time and space and note what changes have taken place, the science of geology steps in, and with it mathematics, chemistry, botany, and zoology. A raised sea-beach with its dead shells, or a submerged forest with the remains of its former fauna and flora, geologically an event of yesterday, sends us back thousands of years into the past, thinking of what were the aspect and dimensions of the former land; therefore, to be a good geographer, something should be known of geology and its kindred sciences. This will be my excuse if in this address I dip somewhat below the surface, and, as some may think, introduce too much geology into this Section. The basis, however, of this branch of knowledge is geography, and this the Royal Geographical Society and the British Association in this particular Section do all they can to foster. There is no gainsaying the fact that very many of our ablest men of science, the ablest naturalists and geologists this country has produced (and it has taken a leading part in geology), have commenced their careers in connection with geographical exploration. Darwin's earlier studies were prosecuted whilst he was attached to marine surveys in other parts of the world; through the same school passed Huxley and Edward Forbes. There was no better example of an able geographer and geologist than Sir Roderick Murchison, who for years took a leading part at these meetings. The list might be largely extended—Sir Joseph Hooker, Wallace, Wyville Thomson, Moseley, &c. That most seductive of all studies, the geographical distribution of species, is intimately connected with geographical exploration. Just as the navy owes much of its efficiency to our coasting and mercantile marine and to our hardy fishermen, so have geography and other sciences been strengthened by the labours of those practical and scientific men who have been engaged in marine or territorial surveys.

The Himalayas, the highest mountains in the world, have excited the interest of many travellers and many geographers; very much has been written about them, some from personal knowledge, and a good deal on second-hand information. Much confusion has resulted from the features of the north-western area being so dissimilar in composition to those of the rest, or eastern part, of the chain, and the limitation placed on the breadth and extent of the whole as a mountain mass. There has been a tendency to apply the term "Himalaya" in too extended a sense: it should, I consider, be restricted to those portions which dominate the plains of India, from the inhabitants of which country we have derived the name. This would, strictly speaking, apply only to the snowy range seen from the plains of India bordering upon the course of the Ganges; but we might, I think, use the term in an extended sense, so as to include that which we may call the north-western Himalaya, north of the Punjab, and also the eastern Himalaya, bordering on Assam.

The orography of this mountain mass has been recently ably handled by Messrs. Medlicott and Blanford,[1] and I follow them in all their main divisions and nomenclature, which are based upon a thorough understanding of the rocks of the country. Some line must be selected where the term Himalaya in its widest sense must cease to be used, and this certainly cannot be better defined than by the valley of the Indus from Attock to Bunji. On this line we find the great bending round or change in the strike of all the ranges. Strictly speaking, the change commences on the south, where the Jhelum River leaves the mountains, but this line, north of Mozufferabad, continues on into the above-mentioned part of the Indus valley. To the mountains north of the Indus on its east and west course the name Himalaya should certainly never be applied. For this north-west, Trans-Indus part of the Asian chain we have the well-known name Mustagh, so far as the head of the Gilgit valley; the Hindu Kush being an excellent term now in common use for its extension to the Afghan country.

The observations made by many of the assistants of the Indian Geological Survey, more especially by Stoliczka, and more recently by Lydekker[2] in the Himalayas, combined with those made by myself in the same region, have, when considered in conjunction with the ascertained strike of the granitoid or gneissic rocks, led me to separate the great Central Asian chain into the following five principal divisions, with some minor subdivisions:—

Central Asian Chain.[3]

1. The main axis or Central Asian, Kuenlun	3. Himalaya
2. Trans-Himalaya	4. Outer or Lower Himalaya
	5. Sub-Himalaya

I use the word "chain" in its widest meaning, so as to comprise the whole length and breadth of a mountain mass, and not, as it has been sometimes used, to describe a "chain" or single line of mountain peaks.

I show these and the equivalent ranges of other geographers and authors in the accompanying synoptical form; and if sections be made, at intervals of about 100 miles apart, through the whole mass of the chain from the plains of India to Thibet, they show where the different ranges are locally represented, and how they separate or are given off from the main axis lines. The same scale for both vertical and horizontal measurements should be used, because there is nothing more misleading than sections in which an exaggerated vertical scale is used. In our present state of ignorance as to the composition of the chain eastward from the source of the Sutlej, we cannot attempt to lay down there any axis lines of original elevation. The separation by Mr. Clements Markham[4] and Mr. Trelawney Saunders[5] of the line of highest peaks into one range, and the water-parting into another, is an acceptable solution of the physical features as at present known of this part of the chain. I am led to think, however, that when this ground is examined it will resolve itself into a series of parallel ridges more or less close, and oblique to the line of greatest altitude as defined by the line of high peaks, crossing diagonally even the main drainage line of the Sanspu, just as we see the Ladak axis crossing the Indus

[1] *A Manual of the Geology of India*, 1879, p. 9.
[2] *Memoirs of the Geology of India.*
[3] Consult Atlas Sheets of the Indian Survey, 1 inch=4 miles, and latest map of Turkestan and the countries between the British and Russian dominions in India—1 inch=32 miles. Compiled under the orders of Lieut.-Gen. J. T. Walker, C.B., R.E., F.R.S.
[4] *Thibet.* Boyle and Manning. Introduction.
[5] *Geographical Magazine*, July, 1877, p. 173.

near Hanlé, or the Pir Panjal that of the Jhelum. Sir Henry Strachey's conception of the general structure was the soundest and most scientific first propounded.[1] He considered it to be

		North-western Himalaya	Western Extension	Eastern Extension	Dr. Thomson, 1847-48	General Cunningham, 1854	Markham, 1876	Trelawney Saunders, 1877	Medlicott and Blanford, 1879
Tibetan Region	A. Main axis or Central Asian	1. Kuenlun	Great Pamir near Siri Kul Lake	Yeshil Kul on Aksaichin	—	—	—	—	Kuenlun
	—	2N. Karakoram	—	Compass La, Lingzi Thang, &c.	—	—	—	—	—
		2. Mustagh	to the Baroghil pass and Hindu Kush	by Rudok to Aling Gangrhi Peak	—	Karakoram Trans-Tibetan or Bolar	Northern main range (western section)	Karakoram—west	Mustagh
	B. Trans-Himalaya	2S. Shayok	Raki Pushi Peak	to Kailas Peak	—	—	Northern main range (western section)	Karakoram in part, Kailas or Gangrhi	—
	—	3. Ladak	North Deosai and Gilgit	to Gurla Peak	—	Kailas or Gangrhi in part	Northern main range (western section)	North Himalaya in part, at Gurla Peak	Ladak
		4N''. Stock	Kargil	Hanlé	—		—	—	—
		4N'. Rukshu	Dras and Gures?	Leo Purgial Peaks	—	Trans-Himalaya	—	North Himalaya in part, north of Spiti	—
	C. Himalaya	4N. Baralasa	Suru	Parang-la, north of Chini Nilang to Niti passes	—		Central main range	North Himalaya in part	—
		4. Zaskar	to South Deosai and Nanga Parbet	Rotang pass to Chini, to Gangotri, &c.	Trans-Sutlej	Western or Great Himalaya	—	South Himalaya in part, at Chini	Zaskar in part, Himalaya
		4S. Chenab	Hoksar pass, cross Sind valley to Haramuk and Khagan to Palas	Rotang Pass	—	—	Southern main range	South Himalaya in part, south of Chenab	—
						Mid-Himalaya in part	—	South Himalaya in part	
Himalayan Region	D. Outer or Lower Himalaya	5. Pir Panjal	Kajnag, Manserah, to Sufeid Koh?	Chatadhar, Dhaoladhar, Chor, Nag Tiba to Almora	Cis-Sutlej in part	The outer Himalaya or Dhaoladhar	—	—	Zaskar in part, Pir Panjal & Dhaoladhar
	E. Suh-Himalaya	6. Sivalik ridges	Attock and Kalahagh	Miri Hills, Assam	—	—	—	South Himalaya in part, near Mozufferabad	Sub-Himalaya
	Terai	—	—	—	—	—	—	—	—
	Plains of India	—	—	Assam Valley	—	—	—	—	—

made up of a series of parallel ranges running in an oblique line to the general direction of the whole mass, the great peaks being on terminal butt-ends of the successive parallel ranges, the watershed following the lowest parts of the ridges, and the drainage crossing the highest, in deep gorges directly transverse to the main lines of elevation.

It will be seen from sections, drawn as above, that the mountain mass of the Himalayas increases gradually in height from the south to about its central portion and then as gradually falls towards the north side. There is no abrupt and conspicuous slope from the higher line of peaks to the plains; a succession of spurs from the main water-parting intervenes, and these spurs retain often a very considerable altitude far to the south. The spurs terminate, usually, abruptly towards the plains of India, at an altitude of 5,000 to 8,000 feet, just within a more or less broad belt of fringing low hills, the well-known Sivaliks.

It has been laid down that the Himalayan chain culminates in two parallel ranges running through its entire length from the Indus to the Brahmaputra, and these have been called the north and south Himalaya, or central and southern; the two combined (they are very close in parts) really constitute the above chain. We can apply this system to certain portions of the range, but it breaks down when we reach the Sutlej on one side and the Monass on the other. The more we increase the scale of our maps, the greater the number of axial lines we can establish, all intimately connected with, and subsidiary to, the run or strike of the greater series of axial elevations.

Explanation of the Different Ranges

1. *Kuenlun Range.*—The most westerly extension of this granitoid axis is found W.N.W. of the Zangi-diwan pass at Oikul and the Victoria Lake. Here Stoliczka records it[1] with slates and schists resting on it to the southward. Now the next great granitoid axis south of the above, with palæozoic rocks on its northern face, is at the Mustagh pass, fifty miles to the south of Kuenlun at Zangi-diwan, and it coincides in position with the gneiss of Kila Panza,[2] the granitic axis of the Mustagh being continued W.N.W. in the high peaks of Hunza-Nagar. The Kuenlun axis passes by Shahdula eastward by peaks E. 61, 23,890, E. 64, 21,500, up to Yeshil-Kul on the Keria route, for a distance of about 450 miles; beyond this is unexplored country.

I have adopted the term Mustagh as one well known to the people on both sides of the range, and better known than Karakoram, applied by them to the pass of that name. The Karakoram pass also lies on an axis of elevation further to the north and intermediate between the Mustagh and Kuenlun.

2. *Mustagh.*—This axis, as I have shown above, commences near Kila Panza in Wakhan, thence by the Baroghil and Kerambar passes to the great peaks dominating the Hunza valley to the Mustagh pass, eastward by K_2, 28,250, to the great peaks north of the Shayok, K_9, K_{10}, K_{11}, K_{12}[3], the Sassar pass, and thence S.E. on to the Marse Mik La and the high mass north of the Pangkong Lake, crossing at Nyak Tso on to the high range south of the Rudok plain, where we again enter unsurveyed ground. It is probably continuous to the Aling Gangri, the old original drainage of the Shayok passing through it at the Pangkong Lake, thus repeating in a similar way that of the Indus through the Ladak range near Hanlé. This most remarkable depression of the whole area, the Rudok plain, lies S.E. of the Pangkong Lake, where, on the same meridian as the sources of the Indus and Sanspu, we have a plain only a little above 14,000 feet, which once drained in glacial and pre-glacial times into the Shayok, rendering that branch as long as, probably longer than, the present Indus. From a high point above the Pangkong I have looked over this plain; for a distance of some sixty miles it was seen bounded to the south by mountains of over 21,000 feet, and no mountain ranges broke the horizon. The depression is a broad and continuous one here, lower and more extensive than that at the head of the Indus. It is not improbable that it indicates the head waters of the next great drainage area north of the Indus, viz. of the rivers that find an exit to the sea through Burmah. The Gang-rhi and Karakoram, or Mustagh, cannot be therefore considered as one range separating the Indus basin from that of the northern or central plateau of Thibet. This must lie across the broad elevated plateau that extends from the Karakoram pass, having a general parallelism to the Kuenlun certainly so far as 34° N. and long. 82° E.

The crystalline limestone near the west end of the Pangkong Lake would appear to be the same as the similar limestone

[1] "Physical Geography of Western Thibet." *Royal Geographical Society's Journal*, vol. xxiii. p. 2.

[1] *Scientific Results of the Yarkand Mission*, p. 38.
[2] Stoliczka, *loc. cit.* p. 38.
[3] Unknown and unnamed peaks were thus designated during the progress of the triangulation.

at Shigar near Scardo. It comes in, too, on the north side of the great gneissic axis, the northern boundary of which follows the Shayok River pretty closely from Tanksé and Shayok to Khapalu. The foldings in the gneiss which have caught up the palæozoic slates near the Tanksé are again on the west indicated by the metamorphic schists on the Indus south of Kartaksho, and by those in the section S.W. of Scardo.

2N. *Karakoram-Lingzi Thang Range.*—West of the pass the country is not known. Eastward the line of elevation passes north of the Dipsang plain to the Compass La, and south of the Lingzi Thang plain, by the Changlung Burma La to the neighbourhood of the Kiang La, and thence still further east it may pass north of Sarthol into Garchethol.

3. *The Ladak-Gurla Range.*—This is the best defined, as a continuous granitoid axis, on the east and west of Leh; the Indus flows at the base of its escarpment for 190 miles, and this line also was not far from the limit of the ancient nummulitic sea. On the west it unites with the great plateau of Deosai and extends to Gilgit. The Indus drainage has cut through it from south to north into the Scardo basin, and back again to south at the sharp bend at Bunji, while on the east at Hanlé the same river passes to the north again, and the range is continued following the left or south bank up to the Gurla peak, south of the Mansarowar Lake. Thence it is probably continuous up to the Fotu La.

2S. *The Shayok-Kailas.*—This subsidiary axis is well marked on the south of the Pangkong Lake N.W. and S.E. of Tanksé, running parallel to the Ladak range. It is then to be followed westward, north of the Shayok River to the junction of the Basha Braldoh Rivers, and thence to Haramosh and Raki Pushi peaks, and perhaps through Yasin to Tirich Mir on the Hindu Kush. To the eastward from Sajam peak, the north side of the Indus and Gartangchu to the Kailas peak, thence very probably north of the head waters of the Brahmaputra.

4. *The Zaskar Range,* where best displayed, is that portion which lies south of the districts of that name in Ladak, and running parallel for 100 miles with the upper sources of that large tributary of the Indus, the river of the same name. In the size of the present glaciers that fill the upper valleys, this portion more closely resembles the Alps of Europe than any other part of the Himalayan chain. It is continued to the N.W., past Dras, to the southern side of the Deosai plains, thus coalescing with that great elevated mass of the primitive rocks. It is continued to the Nanga Purbet, 26,620 feet, and it probably continues still further, west of the Indus, the curve of the range bounding Swat and Bajaur on the north towards Kunar, and which, after the central portion, we may term, at present, the Bajaur range. Taking it up in a S.E. direction, it bends slightly south, crossing the head of the Bagha River by the Rotang pass to that line of lofty snowy peaks seen from Simla and other hill stations leading past Chini to the east of the Sutlej, to the famous peaks of Gangotri, Nandadevi, and Nampa. To the majority of Europeans who have visited India this is perhaps the best known portion of the Himalayas.

4N'. *The Rukshu Ridge.*—Two secondary ranges, more or less connected with the last, one intimately so with an axis of trappæan intrusion of early tertiary age, which from Dras to the Mansarawa is over 400 miles in extent, can be followed. The first is conspicuous at the Tsomorirhi Lake, Mata Peak, 20,600 feet, being of granitic rock; it is seen on the west covered by the earlier sedimentary formations, but it can be traced towards Dras, and on the S.E. to the Imis La, curving thence towards the Leo Purgial mass, the elevated tertiary formations of Hundes coming in on the east.

4N''. *The Stok.*—Another subsidiary and later line of elevation, one I had at first been inclined to disregard in this address, being a minor feature in comparison with the whole chain, flanks conspicuously (attaining the very considerable elevation of over 20,000 feet) the left bank of the Indus for 200 miles, and is still more intimately related to the above trappæan intrusion. It forms a connecting link with the tertiary rocks of the same age on the southern base of the Himalayas (the elevation of which led on successively to the formation of the outermost range of hills, the Sivaliks), and shows the relatively recent date of the elevation of the whole chain, and the obliteration of the topographical details of a previous mountain mass.

4N. *The Baralacha Ridge.*—This line of elevation corresponds with the run of the highly tilted slates, carboniferous and succeeding formations resting against the Zaskar axis, which it follows from near Suru to south of Padam by the Baralacha and Parang passes; here, for a short distance constituting the water-parting between the Indus and Chandrabagha, it can be traced towards the Sutlej, Chini, crossing on to the Keobrang, and in turn the Nilang, Niti, Lakhur, and Tinkar passes, displaying all along this line its characteristic feature, first seen at the Baralacha pass, of being the main water-parting between the Ganges and Kali basins on the south, and the Indus on the north, and constituting from here to the eastward, with the peaks on the granitic or gneissose axis, the main Himalayan range. In the Nipal area to the eastward, we notice the great similarity with which one river basin follows the other, the only difference being that the watersheds of some lie further to the north than others. We may thus, I think, infer that the above character of the Baralacha axis is the type of the physical features along this unsurveyed, little-known territory, until we reach the longitude of Darjiling.

4S. *The Chenab and North Kashmir.*—South of the Chenab River, running parallel with it for many miles, is another gneissic axis, through which the Chenab passes into a sharp bend to the south near Kishtwar; the peak of Gwalga well marks its position here, and the strike of the same rock is continued towards the northern outer hills of the Kashmir valley by Barrapatta and Dalwas Peak, near the Hoksar pass, and the Maro Wardwan valley below Ainshin. For some distance the stratified rocks only are seen, but on the Boodpathar ridge near Srinagar and in the Sind valley, and again from near Haramook Peak to Tragbul, the gneissic rocks appear. Further still they occur in the hills at the head of the large tributaries of the Kahmil River, and thence I suspect are continued across the Kishengunga to the snowy peaks above Wamba and into Khagan. On the S.E. at the Rotang pass at the head of the Beas valley it unites with the Zaskar axis.

5. *The Pir Panjal-Dhaoladhar Ridge.*—On the outer face of the chain there is a well-marked gneissic or granitoid axis. It is well exemplified on the Dhaoladhar ridge above Dharmsala, directly connected with, and equally well displayed in, the Chatadhar ridge south of Budrawar; thence it can be traced to the Chenab, which breaks through it here, to the south-east side of the Kashmir valley, forming the eastern end of the Pir Panjal range. We find it at intervals amidst the older slates along the ridge westward, and close up to the gorge of the Jhelum River, where it leaves the valley of Kashmir. It reappears on the other side of the Jhelum in the Kajnag ridge towards Mozufferabad. The gorges of the Kishengunga and Khagan Rivers are near this place, and to the westward the granitoid rocks are again met with at Manserah in the Hazara valley. Little is known of the mountains to the north of this, but the axis apparently crosses the Indus near Amb, curving round in the Yusufzai Hills north of the Peshawar valley, the Sufedkoh being an analogous range on the south of the Kabul River. Returning to the Dhaoladhar ridge, the granitoid axis continues to Sultanpur on the Beas across that river, by Tuket to Hatu, across the Sutlej to Kuper and Kanchu Peaks, and the well-known peak of the Chor. Nag Tiba, north of Mussoorie, would mark its eastern extension, beneath the slates of that ridge, and beyond Dudatoli and Binsar Peaks, and Almora to the Kali River,[1] near Meenda Ghur. This axis thus holds the same position with regard to the plains of India and at about the same distance from their base for a very great distance.

6. *The Sub-Himalaya.*—This longitudinal section of the Himalaya is easily defined by the fringing line of hills more or less broad, and in places very distinctly marked off from the main chain by open valleys (dhuns), or narrow valleys parallel with the main axis of the chain.

The Eastern Himalaya.—In Western Bhutan, beyond Darjiling, between the Juldoka and the Am Mochu, the gneissic rocks have a N.W. strike by the Pango La, apparently towards Kanchinjunga; to the S.E. by Betso Peak to the Singchula above Buxa. Hooker records Kinchinjhow as of granite, with stratified rocks to the north. This axis may possibly be continued E.S.E. to Chumularhi and the gneiss of the mountains north of Paro.

In the far east, in the Dafla Hills, a more general parallelism of the ranges from W. to E. is found, assimilating to the N.W. area. A well-marked granitoid axis is to be traced from S.W. to N.E. (the outer Himalaya here), convex to the S.E., the tertiaries or the Sub-Himalaya being of considerable breadth and elevation, and following the same curve. Considerable valleys or dhuns are also again a feature on this side.

Lastly, there is the Assam range, which, although not forming a part of the Himalayan mountain system, I must allude to, as

[1] Captain R. Strachey, R.E., P.G.S., 1851.

I shall have to refer to it further on. This is very clearly defined by a gneissic axis on its southern margin, against which the secondary rocks rest, and by a more northern line of the same primitive rock, succeeded by another of isolated low hills following the northern base and the course of the Brahmaputra, and generally lying to the north of it. The last outcrop is seen at Dhoobri, and thence it is no doubt continuous across the delta to similar outcrops of Bengal gneiss on the Ganges, thus connecting this axis of elevation with that of peninsular India. The above range is convex to the south, curving up to the N.E. in the Lhota Naga and Nowgong Hills, and to the W.N.W. in the Garo Hills.

The Burrail range forms another subsidiary line of elevation to the above from the Naga Hills to Jaintiapur, and falls away dipping under the Sylhet *bhils*,[1] to reappear at the most S.W. point of the Garo Hills. From its highest point in the Naga Hills (Japvo), where the strata become nearly horizontal, it merges into and throws off the high N. and S. ridges that bound the Munipur valley on the west, to join the Lushai Hills on the south. This I would call the Western Munipur and Arakan range. It has no granitoid axis: but to the N.E. of Munipur a great mass of intrusive rock occurs at the high peak of Shuruifurar, and thence a high line of elevation runs N.N.E. to Saramethi Peak, and to the south forms the eastern boundary of the Munipur valley, and might be called the eastern Munipur range—it is the water-parting between the above valley and that of the Kyangdweng.

We can, in a measure, exemplify the structure of the Himalaya by that of the bones of the right hand, with fingers much elongated and stretched wide apart, of which the wrist and back may represent the broader belt of granitic rocks of the eastern area, the thumb and fingers the more or less continuous ridges of the N.W., some less prolonged than others to the north-west, such as the Chor axis, which may be represented by the thumb, terminating on the southern margin near the Sutlej. The left hand placed opposite will represent the same features to the west of the Indus. We will even carry this simile further, and as a rough illustration suppose the intervals or long basins between the fingers to be filled with sedimentary deposits, and the fingers then to be brought closer together, producing a crushing and crumpling of the strata. At the same time an elevation or depression, first of one or more of the fingers, then of another or of the whole hand has taken place, and you are presented with very much what has gone on upon a grand scale over this vast area. As these changes of level have not taken place along the whole range from E. to W. in an equal extent, but upon certain transverse or diagonal lines, undulations more or less great have been the result, and some formations have attained a higher position in some places than in others, producing, very early in the history of these mountains, a transverse system of drainage lines, leading through the long axial ridges.

The last efforts of these rising, sinking, and lateral crushing, and, as I believe, very slowly acting forces, are to be seen at the southern face of these mountains in the tertiary strata that make up the Sub-Himalayan axis (Sivalik) a topographical feature which is most striking by reason of its persistence and uniformity for some 1600 miles; for, although a similar and synchronal elevation of the Alps has taken place, the same regularity of orographical features has not been the result, most probably from the difference in the original outline of deposition in the latter area. One object in this address will be to endeavour to point out and compare some of the physical features of the two great European and Asiatic chains.

From Assam on the east to the Punjab on the west, bending round and extending to Scinde, this fringing line of parallel ridges is found at the base of the Himalayas, sometimes higher, sometimes wider, often forming elliptical valleys. Only in one part of the belt east of the Teesta are they absent altogether, and for a distance of fifty miles the metamorphic rocks ri e directly from the plains of India,[2] a feature representing a great break—the correct interpretation of which will tell us very much of the past history of these mountains. These formations are of vast thickness, and in the Punjab, where they attain their greatest width and elevation between the Chenab and the Indus, cover an area of 13,000 square miles.

The whole of this material has been derived from the adjacent Himalayas, representing many feet of the older and higher mountain ranges, and has travelled down valleys that have been excavated in pre-tertiary times. This points to a slow subsidence of the whole southern side of the mountain mass, deposition generally keeping pace with it, broken off by recurring long intervals of re-elevation. Such important, well-marked features as these cannot be omitted when treating of a mountain system. Many long and instructive pages of its history are written on these rocks, with the help of which we may reconstruct some of the outlines of its more ancient geography.

The next most interesting feature connected with the former distribution of land and sea is that these Sub-Himalayan formations are fresh-water, or torrential, showing that since nummulitic or eocene times the sea has never washed the base of the Himalayas.[1] In fact, there is no evidence of this from the gorge where the Ganges leaves the mountains up to the base of the Garo Hills; pointing to an extension northward at that early age of the Arabian Sea, separated from the Bay of Bengal by peninsular India. I am led also to believe that from Assam to Scinde there once existed one continuous drainage line, a great river receiving its tributaries from the Himalayas, partly a land of lakes and marshes, the home of that wonderful mammalian and reptilian fauna which Cautley and Falconer were the first to bring to light. In pliocene times, before the greater displacements commenced, it is not unlikely that the Kashmir basin drained at the north-west end into the Kishingunga Valley to Mozufferabad, and that of Hundes and Ladak trended towards the same direction *viâ* Dras.

The southern boundary of this long alluvial plain was formed by the present peninsula of India, and probably of the extension of the Garo and Khasi Hills westward to the Rajmahal hills.[2] Depression has been considerable in the neighbourhood of Calcutta,[3] nearly 500 feet. We know probably only a portion of the alluvial deposits. At 380 feet beds of peat were passed through in boring, and the lowest beds contained fresh-water shells; the beds also were of such a gravelly nature as to indicate the neighbourhood of hills, now buried beneath the Ganges alluvium. This is precisely the appearance of the country above Calcutta on approaching the present valley of the Brahmaputra. The western termination of the Garo Hills sinks into these later alluvial deposits, and along the southern face of the range up to Sylhet, the waters of the marshes,[4] during the rainy season wash the nummulitic rocks like an inland sea, and point to the very recent depression of all this area. The isolated granite hill-tops jutting up out of the marshy country from Dhoobri to Gwalpara and on to Tezpur all testify to the same continuous depression here. It is exactly north of this that we find the Sivalik formations absent at the base of the Himalayas, and we have the evidence of exclusively marine conditions in pliocene times at the base of the Garo Hills.[5] We find also a large development of marine beds above the nummulitic limestone in the Jaintia country,[6] passing up conformably into a great thickness of upper miocene sandstones of the Burrail range. In such sandstone north of the Munipur valley the only fossils I found were marine forms.

This gradual depression of the delta of the Ganges, the relative higher level of the water-parting and shifting of the Punjab rivers westward, appear to be only the last phase of that post-pliocene disturbance which broke up the Assam Sub-Himalayan lacustrine system draining into the Arabian Sea. Zoological evidence which I cannot here find space to quote is also in favour of this former connection of the now separated waters of the Ganges and Indus basins, and the hill tracts of the Garo and Khasi Hills with peninsular India.

The ground where the miocene rocks are absent is not where any denuding force from the north could have acted with any abnormal intensity. It lies under the hills where no great tributary enters the plain, and might have removed the above formation. All the evidence is in favour of the axis line of depression in the Ganges delta between Rajmahal and the Garo Hills extending thus far, and that the miocene beds, once continuous, are here thus lost to sight beneath the more recent yet extensive gravels and conglomerates that here occur, and have partaken also of a last slight elevation of the mountain chain.

Even if we were to raise the rocks below the delta up to the

[1] "*Bhil*" or "*jhil*"—Hind., a marsh.
[2] Godwin-Austen, *J. A. S. B.* 1867, p. 117. *Memoirs of the Geological Society of India*, Medlicott, vol. iv. pp. 392 and 435.

[1] Blanford and Medlicott, *loc. cit.* p. 393.
[2] Blanford and Medlicott, *Memoirs of the Geological Society of India*, p. 31.
[3] *Loc. cit.* p. 397.
[4] For a very excellent account see Hooker's *Himalayan Journals*, pp. 263-265.
[5] Colebrooke, *Geological Transactions*, vol. i. p. 135.
[6] H. H. Godwin-Austen, *J. A. S. B.* 1869, pp 12 and 152.

maximum level of the Garo Hills, about 4000 feet, it would not be a greater alteration of level than we can see now a very few miles distant to the east. The base of the cretaceous formation rests on granite at the peak of Kailas, about 3000 feet above the sea; at thirty miles eastward it is at the level of the plains of Sylhet, scarcely removed above that level; it is here we find a remarkable depression right across the Assam range from north to south, which it is curious to note faces immediately the Monass valley of the Bhutan Himalaya.

Great lateral rolls or waves of the stratified rocks occur at intervals all along the southern line of the chain, and apparently have a connection with the transverse drainage line. This feature is best seen if we follow the old miocene along its junction with the older rocks. The miocene attains its greatest elevation at Bisari and Keeran Peaks—11,200 feet—close to the end of the Pir Panjal axis; it falls thence towards Mari to 7000 feet, and much lower towards the Potwar. Eastwards it is reduced, above Poonch, to 9900 feet; near Rajaurie to 7000 feet, and Kamrot 6700 feet—or a fall of 4500 feet in fifty miles. The elevation increases again, upon the Chenab, to 8000 and 9500 feet; and, facing the Chatadhar ridge, it is again of great elevation—9096 feet at Hato Peak, and Mandhar 8932 feet. At the Ravi, by Basaoli, there is a depression, east of that river to 4600 feet, but it gradually rises again to 6100 feet at Dhurumsala, under the Dhaoladhar ridge, and retains that altitude to the Beas and Sutlej, where it falls again to 4000 feet, which is its altitude about Nahun and the Jumna. In the Deyra Dhun it is only 3000 feet, but east of the Ganges, where there is a local bend in the strike, it rises again considerably. Beyond this the country has not been visited by me. In the eastern area, under Darjiling, it is of little elevation, but rises to about 4000 feet, disappearing altogether near Dalingkote, but near Buxa the formation reappears, and is only some 2000 feet. Nothing is known of the older tertiary rocks up to the Aka and Dapha Hills, but here they attain large proportion —4700 feet west of the Ranga to 6000 feet beyond that river. South of the Assam range, miocene strata, a distinct group, attain 1500 feet, but are poorly represented in places. At other points, as near the Sylhet *bhils*, they are absent. Near Jaintiapur they expand and reach an altitude of 3000 feet. South of the Lukah River the whole mass gradually rises to 5000 feet near Asalu, and to 9890 at Japvo Peak, its culminating point in the Naga Hills; but these formations are, I believe, marine and estuarine. The great elevation of tertiary rocks here is the exact counterpart of what has taken place on the west, and both are on the great changes of strike in all the formations.

Within the mountains in the old rock basins—and these are analogous to the valleys of the Alps—are pliocene and post-pliocene beds of great thickness, but of fresh-water origin; the remnants of which are to be seen in Kashmir and Scardo at intervals, along the valley of the Indus, and that large—now elevated—accumulation at the head of the Sutlej River in Hundes, first brought to notice by the labours of Captain (now General) R. Strachey. The remnants of these deposits in Kashmir and Scardo are found preserved in the more sheltered portions of the valley basins, untouched by the denuding action during the glacial period—the exponents presented to us of the enormous denudation that went on during the post-pliocene times, of which the glacial period formed a part. The extent and displacement of the upper pliocene beds is in North Italy and here very similar. Often abutting horizontally against the mountains, they are in other places found tilted at considerable angles on the margin of their original extension. When we examine their contents, we find that the fauna of that time in Asia, as well as Europe, was more African in character, and genera now confined to that continent were abundant far to the north. The sluggish rivers and lakes of Sivalik times in Asia and of the corresponding period in Europe were the home of the hippopotamus, crocodiles, and tortoises, of which the common crocodile, the gavial or long-snouted species, and an emys have survived the many geological changes, and still inhabit the rivers and low grounds of India to-day. The fresh-water shells are still the same now as then. Many species of antelope lived in the neighbouring plains and uplands; the elephant was there in the zenith of its existence, for no less than thirteen species have been found fossil in Northern India; but it is impossible, in a short address, to enumerate the richness of this fauna, and the extreme interest that surrounds it.

Miocene of European Area.—If we now turn to Europe to compare formations of similar age, Lombardy and the valley of the Po, with the southern side of the Alps, present to us somewhat similar physical features. A large area of about the size of the north-west Punjab, once a part of the miocene sea, is occupied by a remnant of rocks of that age, considerably elevated and tilted, but not to such an extent as those of the Himalayas. Near Turin these dip towards the mountains, and a very short examination shows the undoubted glacial character of some of the beds;[1] and, as the whole formation is marine, their large sharply angular material, much of which is jurassic limestone, was probably transported from the adjacent mountains by the agency of ice in a narrow sea.[2] After the great crushing and alteration of the previous outlines of the whole country another sea filled the basin of the Po, and pliocene deposits were laid down in a sinking area extending to the base of the mountains all round the new bay or gulf. Re elevation again set in, and with it, or soon after it, the advent of another, and the last, glacial period. But the bounds of the pliocene sea extended even farther than the base of the mountains. At the south end of the Lago d'Orta, well within the hills, sheltering under the isolated porphyry hill of Buccione, and 280 feet above the present lake (or 1500 feet above the sea), I had the good fortune to discover this summer a patch of pliocene sands and clays, with marine shells in excellent preservation, which I am not aware has been noticed before. Sixty-four feet of the section is exposed, capped by moraine matter; its base was not seen, and the beds dip north This remnant tells us a good deal. From where it rests there is a clear horizon to the north down the lake to the junction of its river with the Toce—unmistakable evidence that these beds must have extended far in this northern direction, and that long fjord-like arms of the sea stretched up as far as Domo d'Ossola on one side, and Bellinzona on the other. This marine bed is far above the level of the Lago Maggiore, but I may mention that I also found marine shells of pleistocene age 112 feet above that lake near Arona, of which details cannot here be given.

Before the last great elevation of the Alpine chain the whole line of sea-coast, therefore, ran even high up the long deep valleys of Maggiore, Como, Garda, &c., during the early pliocene period; the mountains, then quite as high as now, enjoying a warm, moist climate, not a glacial one. Then came the gradual but uneven elevation of the whole area, including the miocene hills south of the Po, and lacustrine and estuary conditions prevailed over much of the plain country. The lapse of time was probably enormous, and as the land rose and the sea retired the climate gradually became cooler, and ushered in the glacial period. I do not think it would be an exaggeration to add another 5,000 feet to the Alpine peaks of that time, which would give them an altitude equal to the Zaskar range of the N.W. Himalaya of the present day. With the change and the increased volume of the mountain torrents, the destruction of the upraised marine pliocene beds commenced, and finally culminated in the extreme extension of the glaciers even into the plains; they scoured out almost completely the whole of these deposits, which then filled the great valleys and the country at the base of the mountains, to redistribute them again over the plain of the Po, and silt up what remained there of the old estuary or gulf towards the east. The denudation of this formation has been enormous along the base of the Alps, and only mere remnants are to be found. It is easily seen that their preservation is purely due to the accidental position in places where the great denuding force—viz. the advance of ice from the mountains—has been unable to touch them; in other instances the early deposition of moraine matter upon them has acted like a shield, and prevented their entire destruction. Such examples are well seen near Ivrea, in the well-known section in the gorge of the Chiensella near Stombinella, and in the moraine near San Giovanni.

The scattered remnants of the pliocene formation south of the Alps, which took perhaps thousands of years to lay down, show well how soon a great formation, together with the preserved remains of the fauna living at the time, may be completely destroyed by subsequent denuding forces. Similar destruction must have occurred over and over again in past geological ages, and shows clearly how the scanty, broken record can be accounted for.

It is an established fact that the great valleys of the Alps and

[1] Refer to Gastaldi.

[2] No trace has been observed of this glacial period in the miocene of India; the most lofty portions of the chain had not then attained a greater elevation probably than 14,000 to 18,000 feet, and the outer axis lines far less However, in the tertiary beds (middle eocene?) of the Indus Valley below Leh such conditions are indicated by Lydekker. *Memoirs of Geological Survey of India*, vol. xxii. p. 104, which I have received since this address was sent to press.

Himalaya existed much in their present form during miocene times, and they may owe their excavation partly to the glacial action of that period, when these mountain slopes rose from the plain or margin of the ancient sea, far in front of the present line of slope, and were far higher than now. This idea particularly strikes one when looking at the ice-ground spurs that run out into the plain south of the Lago d'Orta. The general and local elevation and depression that took place in post-miocene times seem quite sufficient to account for the difference in the comparative levels of adjacent transverse valleys, or an elevation along the base of the chain, clearly indicated at Orta by the northerly dip of the marine beds. It is reasonable to suppose that these movements were exerted in different degrees, at points all along this face of the Alps and within the same, and that the depression on the west has been less than on the east, so that the sea never extended far up the valley of Susa, and to a comparatively short distance up that of the Dora Baltea as compared with Maggiore, and the formation and excessive depth of this and similar lakes on the east is mainly due to this local depression and elevation. Depression has steadily continued in the delta of the Po as in the Ganges at Calcutta, for, at Venice, borings showed depression of land surface to an extent of 400 feet, and they did not reach the base of the formation.[1]

It is not improbable that during the earlier extension of the glaciers into the Maggiore basin,[2] the sea still had access to it;[3] this would have greatly aided in the removal of the marine deposits, and then the deeper erosion of its bed near the Borromean Islands, so well put forward by Sir Andrew Ramsay. When we see the gigantic scouring which glaciers have effected in the hardest rocks on the sides and bottoms of valleys, when we know for certain the enormous thickness they reached in the Alps, I do not doubt for a moment their capability of deepening a rock basin very considerably, or their power to move forward over and against slopes so low as 2° to 3°.[4]

The earliest extreme extension of the glaciers was very great; we have evidence of it on the miocene hills near Turin, their surface being scattered over with transported material of great size, quite unconnected with that other ancient period of glacial conditions during the miocene times mentioned above at a period too remote to further dwell upon here. Even now I feel that in dealing with this subject of the glaciation of the Alps, many of you may say that I am departing too much from geography. To this I would answer, glacial periods have been so intimately connected with the interchange of sea and land conditions, that where can the line be drawn in physical geography between the past and the present? It is as undefined as the line which separates species from genera.

An enormous interval of time must have elapsed, during which the cold was increasing and the glaciers advancing, and during which the rivers were distributing the consequent waste over the lower country, spreading out the more or less coarse material, sands, and clays, in broad fans in front of all the great gorges. Then came the first period of contraction of the glaciers, with many oscillations. Of this we have the evidence in the moraines of Ivrea, Maggiore, &c. Sections of these moraines show how they were piled the one upon the other; how the building up of one line of lateral moraine was followed by its partial destruction on another forward movement of the ice, and the throwing down of another moraine upon it. Then were formed many of the smaller lakes, remains of which lie amid the debris thrown out into the plain. The glaciers retained this size for a very considerable time, and then apparently very rapidly retreated to far within the mountains, but still for another considerable period their dimensions were much larger than those of the present time, into which they seem to have again rather rapidly shrunk.

Passing from the glacial action displayed in the outer Alps to that in the Himalaya, we find ample evidence of a period of great extension of such conditions, first in the erratics of the Attock plain and the Potwar,[5] lying fifty to sixty miles from the gorge of the Indus at Torbela. We have again the fact that in Baltistan, in the Indus valley, glaciers have twice descended far beyond their present limits, first down to Scardo itself, and then to some thirty miles below their present limits; while the glaciers of Nanga Purbet, towering above the Indus some 22,000 feet, must have descended into the bed of that river. Even allowing that the Potwar was not formerly a lacustrine basin, the great *débacles* from the mountains would have been sufficient to convey erratics fixed in ice to where they now lie. Cataclysms of the present time, caused by glacial obstructions, have raised the level of the Indus on the plain above Attock so much as eighty feet. When these glaciers were more than double their present size, gigantic floods must have often taken place, and formed boulder deposits high above present levels: such high-level gravels are to be seen not only in the Potwar, but also in the Naoshera Dhun on the Rajaurie Tawi River, containing boulders of nummulitic limestone and other rocks of the Pir Panjal on the north.

Again, north of the Chatadhar ridge, small glaciers, five to six miles in length, at one time filled the lateral valleys, descending towards the Chenab River to about 5,000 feet; and a very perfect moraine occurs in one valley. This ground must be very similar to that which has been described by Theobald as occurring in the adjacent Kangra district[1] on the flanks of the Dhaoladhar ridge. Similar small glaciers existed, I believe, in the valleys of the Kajnag range, but I think that neither in this range nor in Budrawa did they ever descend into the main valleys; but the existence of these glaciers, together with the large snow-beds, had much to do with the formation of the high-level gravel-beds and fans through which the Jhelum and Chenab have since cut their way.

In fact, examples of the former extension of glaciers are widespread along the chain of the Himalayas from west to east. True moraines, and moraine-mounds, at 16,000 feet on the north side of the Baralasa Pass, attest the presence of glaciers on the elevated plain of Rukshu, where now the snow-line is over 20,000 feet.[2] Drew gives much valuable information regarding their former size.[3] On the east, in Sikkim, Sir Joseph Hooker[4] has described moraines of great height (700 feet) and extent.[5] Still further south and east, in the Naga Hills, a short period of greater cold is indicated by the moraine detritus under the loftiest portion of the Burrail range in latitude 25° 30′.[6]

Whatever may have been the length of the glacial period in the Alps—and it was very considerable—in the Himalayas it cannot have been so long and so general, although to a certain extent, contemporaneous.

In the Alps glaciation meets the eye on every side, and the mountains, up to a distinct level, owe their form and outline to its great and universal extension.

In the Himalayas it is difficult to trace polished surfaces or striæ markings, even in the neighbourhood of the largest glaciers that are now advancing in full activity. It has been suggested that obliteration is the result of more powerful denudating forces, but the conditions are not so very dissimilar in the high Alps and high Himalaya as to warrant this; and wherever the oldest striæ marks occur in the Himalaya, they are situated near the bed of the valley. It may interest you if I give an illustration or two of the size of these present glaciers as compared with those of the Alps. The Baltoro glacier would extend, if placed in the Toce valley, from the Simplon to the margin of the Lago Maggiore; or take another illustration of its length, from Mont Blanc to Châtillon in the Valle d'Aosta.

Although of such great length, these Himalayan glaciers could never have reached the enormous thickness which the earlier Alpine glaciers attained. This may thus be accounted for: in the European area a generally low temperature prevailed down to the sea level, while in the Himalayan it was local, and confined to a higher level. It is evident that the snow-line has altered—higher at one period, lower at another—down to recent times, denoting changes of the mean annual temperature, which are not yet fully understood, but have been attributed to very far distant distribution or alterations of land, sea, and the ocean currents.

Two periods of glacial extension are clearly defined, separated by a milder interval of climate: during the earlier glacial period

[1] Lyell, *Prin.* vol. i. p. 426.

[2] With reference to the moraines of Ivrea, see pamphlet by Luigi Bruno, *I terreni costituenti l'anfiteatro allo sbocco della Dora Baltea.*

[3] The evidence is stronger as regards the Lago Garda.

[4] There appears to be too great an advocacy, on the one hand, of ice action having done all the work of denudation; while, on the other, some writers consider this to have been extremely limited; it is the combination of the two forces, I think, that effects so much and in so different a manner and degree.

[5] A. Verchère, *J. Asiat. S. Bengal*, 1867, pp. 113 114; Theobald, *Records of the Geological Society of India*, 1877, p. 140.

[1] *Ibid.* 1874, p. 86.

[2] North of the Karakoram, in that now arid country, great moraines are found in the valleys that descend into the Karakash, in the neighbourhood of the Sujet pass, 17,600 feet. (Harold, Godwin-Austen in Epit.)

[3] *The Jummoo and Kashmir Territories.*

[4] *Himalayan Journals*, vol. i. p. 221.

[5] The equivalents, although very small, of such moraines are to be seen in the Alps on the Simplon jutting out into the valley.

[6] Godwin-Austen, *J. A. S. B.* 1875, p. 209.

the Indus valley was filled with those extensive lacustrine and fluviatile deposits, mixed with large angular debris, such as we see at Scardo, which may be coeval with the extreme extension of the Alpine erratics so far as the miocene hills south of Turin.

The second period followed after a long interval of denudation of the same beds, and would correspond with the last extension of the great moraines of Ivrea, Maggiore, Como, &c., followed by a final retreat to nearly present smaller dimensions. Nowhere on the south of the Himalaya do we find valleys presenting any features similar to those of the Southern Alps, particularly on the Italian lakes, which are, I believe, the result in the first place of marine denudation, succeeded by that of depression, and finally powerful ice-action. On the south face of the Khasi and Jaintia Hills, however, which are orographically connected with the peninsula of India—the conditions altogether different—we find long stretches of water of considerable breadth and depth extending within the hills, and not unlike in miniature the Italian lakes. These valleys, worn out of the sandstone and limestone rock, have been formed here, I think, to some extent by the aid of marine action, and the subsequent depression along this line of hills, also marked here, as in the Western Bhutan Doars, by the absence of beds newer than the nummulitic.

This attempt to bring before you some of the great changes in the geography of Europe and Asia must now be brought to an end. It is a subject of vast time, of absorbing interest. I am only sorry it is not in more able hands than mine to treat it in the manner it deserves, and in better and more eloquent language; but it is a talent given to but few men (sometimes to a Lyell or a Darwin) to explain clearly and in an interesting form the great and gradual changes the surface of the earth has passed through. The study of those changes must create in our minds humble admiration of the great Creator's sublime work, and it is in such a spirit that I now submit for your consideration the subject of this address.

SECTION G

MECHANICAL SCIENCE

Opening Address by James Brunlees, F.R.S.E., F.G.S., Pres. Inst.C.E., President of the Section.

The British Association for the Advancement of Science admits to its annual gathering women as well as men; and I venture to think it does so wisely. Women now take their place regularly in the ranks of several scientific professions; and though they have not shown any desire to enter that to which I belong, there has recently been an example of their capability in that direction which is noteworthy. It has been publicly stated that Col. Roebling, the distinguished engineer of the Brooklyn suspension bridge, which is one of the most remarkable works of the age, was assisted during a long illness in carrying out his work by the talent, industry, and energy of his wife, who acquired theoretical and practical knowledge enough to aid in seeing that her husband's design was properly carried out. I think this example is not unworthy of mention here, as honourable to the individual woman, to the energetic nation to which she belongs, and to the better half of the human race.

The previous meetings of the British Association have been held in places possessing very varied characteristics; but in none in which the pursuits of science could be undertaken under more pleasing circumstances than in Southport, with which I have been acquainted for a good many years.

It is customary for the President of each Section to begin the Session by giving an introductory address. I propose, with your kind indulgence, to offer some brief remarks, as far as possible free from technical language, on a subject which is familiar to my own mind, and within my own experience, during a period now approaching half a century, that is: The growth of mechanical appliances for the construction and working of railways and docks.

The railway of the present day is in principle what it was at the outset; but it differs in detail from the original railway as much as, or more than, the skewer which fastened the dresses of the ladies of Elizabeth's time from the pin of the present day, or the carpets of this era from the rush-strewn floors of that. The progress has been gradual, but not slow. From the opening of the first railway to the present date is only a period of about sixty years, and in that short time Great Britain and Ireland, the continent of Europe, America, North and South, India, Australia, and Africa, have been pretty well supplied with railway lines, more and more perfect in construction, and in a degree more or less suitable to the needs of their populations.

About thirty years ago, when the traffic on railways had been very largely developed, the parts of the permanent way which had at first been thought likely to be the most enduring, the rails themselves, were found to be more rapidly worn away than was expected. Efforts were made to harden the surface of the rails, and a plan was introduced by Mr. Dodds for this purpose. It was extensively used where rails were subject to special wear and tear, at points and crossings. The conversion was easily effected: it cost only about fourteen shillings to a pound a ton, and it was estimated that it doubled the durability of the rails. If they were turned, of course it increased their durability three times.

The plating of rails with a steel surface was probably begun about 1854. It was not till about eight or ten years later that rails were made entirely of steel.

In May, 1862, steel rails were laid down experimentally at Chalk Farm Bridge "side by side with two ordinary iron rails, and after outlasting sixteen faces of the iron rails they were taken out in August 1865, and the one face only which had been exposed during a period of more than three years to the enormous traffic, amounting to something like 9,550,000 engines, trucks, &c., and 95,577,240 tons, although worn to the extent of a little more than a quarter of an inch," even then appeared capable of enduring a good deal more work. Steel rails, however, were dear at that period, costing about double (12*l*. 10*s*. per ton) as much as iron rails; therefore, although their advantages were manifest, they could not all at once replace iron. In 1866, Mr. Webb, the locomotive engineer of the London and North-Western Railway, said they had in use 3000 tons of steel-headed rails and about fifty miles of steel rails; and Mr. Harrison, of the North-Eastern, said he had just contracted for 500 tons. Now, owing to improvements in the manufacture of steel rails, they can be produced as easily and as cheaply as iron rails. It was observed in 1876 that if, in order fully to realise the effect of the enduring quality of steel rails, you take a given section of the busiest portion of one of our leading railways, over which upwards of 7,000,000 tons of live and dead weight pass annually, you would find that the life of a steel rail on that portion of the line would be forty-two years if the traffic remained the same. This would reduce the cost of maintaining the permanent way of railways from 210*l*. to 106*l*. per mile. When you consider that such a saving on a system of 500 miles, which at 25,000*l*. a mile costs twelve and a half millions, is 52,000*l*. a year, or about a half per cent. of the cost of the railway, you will see that, besides some increase of dividend to shareholders, no inconsiderable sum may be, and has been, devoted by the railway systems of Great Britain to the comfort of travellers out of the saving effected by the introduction of steel rails.

You are aware that railways are worked by the aid of an elaborate system of signals, by which those in charge of a train are required to be guided in regard to its movements. The author then gave a history of signals, bringing his account down to the present day.

The subject of brake power is one to which very great attention has been given both in this country and abroad; and certainly, next to the condition of the permanent way and the efficiency of the signalling apparatus, perhaps nothing in connection with railways is of greater importance. Many lives and much property are hourly dependent in a greater or less degree on the power and efficient state and immediate action of brakes. It has been found that most of the collisions which have occurred might have been prevented had those in charge of trains possessed the power of stopping them within a few hundred yards. The higher the speed and the heavier the train, the greater the necessity for a powerful and simple brake, capable of being applied throughout the train in the shortest possible time.

All recent efforts for the improvement of brakes appear to have been devoted to making the action of the brakes automatic, and to increasing the rapidity with which they can be applied.

I do not intend to enter into the controversy respecting the best system in use for obtaining these results. There are several systems by which they are attained more or less effectively; and whereas trains which thirty years ago weighed on the average thirty tons, with engines of the same weight, running at thirty-five miles an hour, could scarcely be brought to a stand in a distance of about 800 or 1000 yards, now trains of twice or three times that weight, and running at a much higher speed, can be brought to absolute rest in twenty or thirty seconds, and within a distance of from 300 to 400 yards.

When railways were first made, the locomotive was a very imperfect machine, which could only travel economically on roads almost level and straight. As there are no level plains of great length in this country, and as reducing the natural surface of the country to a fair level is both tedious and costly, considerable detours were made to avoid steep gradients or their alternative, long tunnels, deep cuttings, and high embankments. In some cases where a very steep gradient could not be avoided, a stationary engine and rope traction were adopted. The great improvements in the locomotive gradually led to the almost entire abandonment of rope traction in this country; and gradients which it would have been impossible for the earlier engines to surmount with a load equal to their own weight are now ascended with ease with heavy trains at moderate speeds. Abroad, however, great natural difficulties and a limited capital were not infrequently concurrent conditions which offered to the engineer troublesome problems for solution. In some districts the locomotive could not do the required work, and other means have had to be resorted to. The plans adopted for overcoming the difficulty presented by the sudden elevation of the surface over which a railway must pass may be typified by the wire-rope system, as employed by myself on the St. Paulo Railway of Brazil, and by the central rail system of Mr. Fell, first employed on the Mont Cenis Railway, and since on steep inclines in New Zealand.

The central rail system was designed by Mr. Fell, and first carried out practically in the railway made over Mont Cenis, under my direction, before the opening of the great tunnel. The peculiarity of the system lies in the use of a deep rail laid on its side between the two ordinary rails; the centre rail is gripped by horizontal wheels, put in motion by the locomotive, the adhesion of which to the centre rail gives the locomotive the force necessary to draw up steep inclines, not only its own weight, but a considerable supplementary load. This is probably the most economical mode of working very steep gradients under ordinary circumstances, and it has been found to answer very well wherever it has been efficiently carried out.

In the construction of railways and docks one of the most expensive and tedious operations is the excavation of the soil. In England the cutting of numerous canals had trained a large body of men to special fitness for the execution of such work, which they performed with a manual dexterity and amount of muscular power which have made the British navvy a special force in the execution of great public works. Where labour was comparatively scarce and inefficient, as, for instance, in America, efforts were made at an early period to supplement, and, if possible, supersede, such manual labour by mechanical contrivances. In 1845 a mechanical excavator, after an American model, was used on the Eastern Counties Railway with a certain amount of success. This machine delivered as much as 100 cubic yards an hour at a cost which did not exceed fifty shillings a day. In principle, and generally in detail, it is very much the same as the excavator which is commonly known as the "steam-navvy" at the present day. The machine was locomotive, and had three other kinds of motion—first, thrusting the scoop or shovel into the earth; second, lifting the scoop when filled; and third, turning round on its centre to deposit the earth in the waggons. The use of small locomotives for tipping the soil for embankments has relieved the workmen of one very laborious and sometimes dangerous occupation, and in a corresponding degree has diminished the cost of construction.

One of the most important operations in connection with shipping is the repairing, cleaning, and painting of ships. For this purpose graving docks, from which the water was removed after the vessel had entered, were and continue to be mostly employed. But during the lifting of the tubes of the Britannia Bridge into place with what were then called hydraulic presses, it occurred to Mr. Edwin Clark that similar means might be used to lift a vessel out of the water and place it in a position to be dealt with similarly to a construction on dry land. Floating docks consisting of pontoons which lifted the vessel out of the water have been used in this country, and more extensively in America, for this purpose: and at San Francisco and Philadelphia a dock was constructed of pontoons in sections called "camels," any number of which might be used according to the size of the vessel to be docked. Mr. Clark's plan is quite different from these. His hydraulic dock consists of a number of columns arranged in two parallel rows, in which columns are placed the hydraulic lifting power. Between these two rows of columns extends a frame or cradle, over which the ship is drawn in the water. When the ship is in position the hydraulic lifts are set to work, and they raise the cradle first to the bottom of the ship, which, being properly secured, is then lifted with the cradle clear of the water. There is no difficulty whatever in the management of this form of dock, and it has been perfectly successful: its chief recommendation being that any area of shallow water can be made available for docking large vessels, and that it is especially valuable in tideless seas.

Among the many mechanical appliances for saving labour on railways and docks, the machinery for shipping coal is remarkable: the bulk, weight, and low price of coal render every item of saving in transport relatively important. It is commercially important also that the coal in the different stages of transport from the pit to the distant consumer should be broken as little as possible, and a good deal of attention has been given to contrivances to secure these ends.

A great variety of hydraulic machinery has been designed by Sir William Armstrong for coal loading, and it is largely employed at Newport Docks and elsewhere.

Many different kinds of labour-saving machinery for dock and railway work in loading and unloading have been invented during the last fifty years, and have had a most important influence on the development of railway and steamship transport.

Hydraulic machinery has also been largely employed for opening and closing dock gates and sluices, and for warping ships through the locks.

A large dock is in course of construction at Hull, by Mr. Abernethy, called the Alexandra Dock, where almost every kind of machinery which can be used in work of that nature is being used by the contractors, Messrs. Lucas and Aird, to expedite the work. Two of Priestman's steam grabs are employed, each capable of filling about 390 cubic yards a day, and are found very useful in opening out work for the steam navvies, six of which are employed, each being capable of filling 600 cubic yards a day. There are a number of steam cranes, steam pile-driving machines, and steam jiggers at work. But, besides those moved by steam power, hydraulic power has here for the first time been applied to machinery for the construction of works. An hydraulic crane puts the stonework of the dock walls in place; an hydraulic jigger raises the barrow-loads of soil from the bottom of the dock to the wall where it is shot to the back for filling. One of the six navvies is moved by hydraulic power; and there is an hydraulic pile-driving machine. The hydraulic machinery is found to work at least as quickly, as easily, and as economically as steam machinery, and it works almost without noise, and quite without smoke. The trial of hydraulic machinery for these purposes has been quite successful, and where circumstances permit it will no doubt be used extensively in works of construction in future. For dock work much of the hydraulic machinery can be used permanently in the ordinary operations of loading and unloading, so that the loss by sale of such expensive plant, which a contractor has to take into account when making his tender, will be avoided, as it can be turned over to the dock company, with a reasonable deduction for wear and tear, at the end of the work. There are 2800 men employed at this dock; and the work is carried on at night by the aid of the electric light. The mechanical navvies and grabs do the work of about 400 additional men.

The working of railways by electricity has not advanced further than to justify merely a brief reference to it in this paper as among the possibilities, perhaps the probabilities, of the not distant future. A line of a mile and a half of tramway has been working successfully at Berlin for over two years without hitch or accident of any kind. A line of narrow gauge railway is constructed from Portrush, the terminus of the Belfast and Northern Counties Railway, to Bush Mills, in the Bush Valley, a distance of six miles, which is now partially worked by electricity, and is to be wholly so worked as soon as the necessary plant is completed. As the generating power is that of the abundant streams of the neighbourhood, it will be economical; and if success should crown this practical experiment, it may lead to important results in regard to the employment of electricity under similar circumstances as a locomotive power.

I have now passed rapidly in review some of the more striking mechanical improvements in the construction and working of railways and docks which have taken place chiefly within my own experience. Each of them has had an influence, important if unnoticed, in promoting the growth of our railway and dock systems. Precisely how far any single appliance has contributed to create these magnificent systems, of which this country may with just reason be proud, it would be difficult to say; and it would be as difficult to say which of them

could be dispensed with without injury to the rest. They may be laid aside in course of time, one by one, as mechanical ingenuity devises new and better plans to take their place, and to meet the new and larger wants of other generations. But as the present age looks back with respect and veneration to the creation of those monuments of engineering science of which little more than ruins or even historic records remain, so will the generations which succeed us look on these, our works, as worthy, and as having contributed in no small degree to the greater and more general civilisation to which we hope those who follow us may attain.

UNIVERSITY AND EDUCATIONAL INTELLIGENCE

CAMBRIDGE.—The following courses of lectures and practical work have been announced for the present term by the Special Board for Physics and Chemistry :—

Chemistry.—Prof. Liveing, General Principles; Prof. Dewar, Physical Chemistry, advanced; Mr. Main (St. John's), Organic, elementary; Mr. Pattison Muir (Caius), Organic, advanced; Metals, elementary; Mr. Scott (Univ. Lab.), Physical, elementary; Mr. Lewis (Downing), catechetical.

Practical Chemistry.—Prof. Liveing, Spectroscopic Analysis; Mr. Sell and Mr. Fenton, Quantitative Analysis; Mr. Robinson, Analysis of Water and Food. The University, St. John's, Caius, and Sidney College Laboratories will be open for practical work.

Physics.—Lord Rayleigh, Current Electricity and its Practical Applications; Mr. Trotter (Trinity), Electricity and Magnetism, elementary; Mr. Atkinson (Trinity Hall), Heat and Hydrostatics, elementary; Mr. Shaw (Emmanuel), Physics, elementary and advanced. Practical work at the Cavendish Laboratory, with advanced demonstrations.

Mineralogy and Crystallography.—Prof. Lewis, with practical demonstrations.

Mechanism.—Prof. Stuart, with practical work at the mechanical workshop.

The Special Board for Biology and Geology have published the following list of lectures for this term :—

Physiology.—Prof. Foster, elementary; Mr. Lea (Caius), Chemical Physiology, advanced; Mr. Langley, Physiology, advanced; Mr. Hill (Downing), second M.B. class.

Zoology and Comparative Anatomy, and Animal Morphology.—Prof. Newton will lecture on Evolution in the Animal Kingdom; Mr. Sedgwick, Practical Morphology, elementary and advanced; Dr. Hans Gadow, Morphology of Ichthyopsida, advanced.

Botany.—Dr. Vines (Christ's College), General Elementary Course, and Advanced Physiology.

Geology.—Prof. Hughes, Geology of France, Switzerland, and Italy; and Pleistocene Geology, with special reference to Prehistoric Archæology; Dr. R. D. Roberts (Clare College), Physiography and Class Work; Palæontology and Petrology by a Demonstrator: and Field Lectures, by special notice.

SOCIETIES AND ACADEMIES

SYDNEY

Royal Society of New South Wales, June 6.—Charles Moore, F.L.S., vice-president, in the chair.—Two new members were elected, and 156 donations received. The following paper by Mr. Peter Beveridge was read:—On the aborigines inhabiting the great lacustrine and riverine depression of the Lower Murray, Lower Murrumbidgee, Lower Lachlan, and Lower Darling.

PARIS

Academy of Sciences, September 24.—M. Blanchard, president, in the chair.—The death was announced of M. Joseph A. F. Plateau, Correspondent of the Section of Physics, who died at Ghent on September 15. A summary of the scientific work of the illustrious *savant* was given by M. Faye.—The death was also reported of M. Thuillier, a member of the Egyptian Cholera Commission, who fell a victim to the disease at Alexandria on September 19.—Note on solar spectra, with the results obtained with the mineral salt refringent apparatus described in the *Comptes Rendus* of September 4, 1882.—Remarks by M. Gaudry on some specimens of extinct Siberian mammoths obtained by him during a recent visit to Russia, and now submitted to the Academy. The specimens consisted of some hair mixed with wool and a piece of skin taken from the mammoth brought to St. Petersburg by M. de Maydell in 1871.—On a new and more general case of the problem of the resistance of an elastic rod, and one of its applications, by M. Maurice Lévy.—On the action of the turbine used to set in motion the electric generator at Vozille-Gare, by M. Marcel Deprez.—Additional note on the probable epochs of earthquakes, by M. J. Delauney. The author replies to the objections recently urged against his theory by M. Faye, and formulates the following law:—Most cosmic and terrestrial meteorological phenomena, and especially the great seismic disturbances, seem to occur when the great planets pass through certain longitudes, notably those of 135° and 265°, or thereabouts.—Observations on the small planets 159, 199, 218, and on the Pons-Brooks Comet, made at the Paris Observatory (equatorial of the West Tower), by M. G. Bigourdan.—Observations on the planet 113 Amalthæa, by M. Perigaud.—On the induction due to the variation in intensity of the electric current in a plane circuit and in a cylindrical solenoid. Two laws analogous to those of Biot and Savart, by M. Quet.—Researches on the dispersion of light, by M. C. E. de Klercker.—On the distribution of the potential in liquid masses of determined form, by M. A. Chervet. Two cases are dealt with: (*a*) that of a rectangular plate of indefinite length; (*b*) that of a liquid mass limited by two vertical parallel planes.—Terrestrial magnetism; solution of the problem of the determination of the magnetic meridian by the compass itself on board iron ships, by M. E. Bisson.—On the composition of the substance known as gelatino-peptone, which is obtained by the action of the gastric juice on gelatine, by M. P. Tatarinoff.—Fresh observations on the tubercules and roots of *Phylloglossum Drummondii* (Kunze), by M. C. Eg. Bertrand.—On the influence of external pressure on the absorption of water by roots, memoir by M. J. Vesque.

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THURSDAY, OCTOBER 11, 1883

THE METAPHYSICAL FOUNDATIONS OF NATURAL SCIENCE

Kant's Prolegomena and Metaphysical Foundations of Natural Science. Translated, with a Biography and Introduction, by Ernest Belfort Bax. (London: George Bell and Sons, 1883.)

THE pages of NATURE are not the appropriate place for the review of works on general metaphysics. The genius and methods of science are so different from those of philosophy that, as their respective histories have amply shown, these branches of intellectual activity are as a rule best kept asunder. But there is at least one important point of contact which cannot be overlooked. And it is just because in the writings of Kant, and particularly in the second of the two treatises which are translated in this volume, that alleged point of contact was formulated for the first time that his work rightly or wrongly demands notice in these pages. As regards the translation, Mr. Bax has done his work with care. He has undertaken simply to furnish a literal and accurate translation of the "Prolegomena" and "Metaphysische Aufangsgründe," and he has fulfilled his undertaking. We should have been glad could he have seen his way to banish such inelegant and inaccurate renderings of "Vorstellung" and "Anschauung" as "representation" and "intuition," and to substitute for them "idea" and "perception," which, despite their vagueness, are English words of intelligible significance. But no one can fail to find in the translation, as it stands, a faithful and consistent rendering of the original.

In an essay on the relation of philosophy to science contributed to a volume entitled "Essays in Philosophical Criticism," I had recently occasion, in conjunction with my brother, to formulate in some detail what are conceived to be not merely Kant's own criticisms of the subject, but certain definite results obtained by the application of the Kantian analysis of the nature of knowledge to some of the methods of science. I mention this circumstance because that essay has undergone vigorous criticism at the hands of Mr. Romanes in a review which appeared in NATURE of August 23 (p. 386), and because the concise and definite objections taken by him *in limine* to the title of the theory of knowledge to criticise certain of the leading scientific conceptions, form a point of application for a review of Kant's teaching.

In the first place it is necessary to state at the outset what the somewhat increasing number of people who read Kant intelligently think about science. That science has justly dominated the region of knowledge generally is for them a truism, and they repudiate in emphatic language any attempt to speculate by *a priori* reasoning upon matters which fall within the province of observation and experiment. Whenever there occurs a question which is really one of fact in nature, that question they recognise as for science alone. But then they say that it is not the faith but the scepticism of men of science which is too small. They ask men of science to consider their general conceptions—to criticise their categories—a little more than they have been in the habit of doing. They doubt whether such questions as the common one whether the phenomena denoted by the word "life" came into existence at a particular time as the effect or creation of some cause or conditions (whether inorganic or supernatural is irrelevant) are really questions of fact. They profess to be able to show that the dilemmas raised in such cases are the result of the application of conceptions which have really no application, and that such dilemmas have as little foundation as that which arises when, to refer to Mr. Romanes' illustration, we ask whether a piece of mechanism is comical or not comical. It is alleged to be the achievement of Kant to have shown that such questions as we have indicated are irrational and absurd, and that their existence necessitates on the part of men of science the possession of philosophical knowledge. Of this class of questions there may be mentioned by way of illustration the general problems of the commencement or non-commencement of the universe or of life in time, and of the existence of an absolutely First Cause, and the validity of a multitude of assumptions in our inquiries into the facts of nature which pass more or less unquestioned.

In the article already referred to, Mr. Romanes peremptorily refuses to accept the result that it is impossible to regard biological phenomena as the effect of mechanical causes, or, more accurately, to find in experience a case of *abiogenesis*. "It is," he says, "the worst form of dogmatism thus to affirm on grounds of metaphysical speculation alone the antecedent impossibility of any discovery in science, most of all with reference to a matter touching which we are so much in the dark." Now this "demurrer to the relevancy" is an *ignoratio elenchi*. Such a question is for Kant not one of discovery in science at all, but a false issue, which discloses its unintelligible and absurd nature whenever we ask ourselves the preliminary question, what is meant by organisation and mechanism. Let us examine more closely the point made by Mr. Romanes. The living organism is derived from one more simple, and the latter from one yet more simple, the process extending back without an apparent limit. Therefore, says Mr. Romanes, it is unscientific to deny the possibility that there may be a case of organisation so simple that it will be seen to be a mere mechanical arrangement. But the series in like manner tends to reach its limit and the curve to touch the asymptotic line, and yet it is neither unscientific nor unwarrantable *a priori* reasoning to show that a coincidence will never be found in experience. We learn what we have here by defining what is involved in the nature of the series and the limit of the curve. Nobody wishes to deny that organisation and the present state of the world generally may have been attained by a process of evolution from a mass of gaseous vapour. What is denied is that it is the same thing or other than an unintelligible statement, to say that organisation is or may have been evolved out of a mere mechanical arrangement. There is a great distinction between these propositions. Science is a process of abstraction in which attention is concentrated on a certain kind or category of relation to the exclusion of other kinds. For instance, in physical science we look only at those dynamical and statical relations which are expressed in time and space, such as causation and reciprocity. Again, in biology

we have before us the facts of organisation and development. But it is one thing to consider a single set of relations, such as those of causation, to the exclusion of the rest, for the sake of clearness of knowledge, and quite another to say that this particular aspect of the object exclusively constitutes it. Mr. Romanes thinks that biologists do not require any transcendental analytic to inform them that an organism is something more than a mechanism. But he finds it startling to be told that in the investigation of an organism we are to rise above the category of causality, and carry into our inquiry the conception of teleology. Surely the latter proposition is the logical consequence of the former. No one says that the category of causation is not to be used in the investigation of the phenomena of organisation. In anatomy, and in its dynamical correlative physiology, the parts of the organism are constantly treated as independent of each other, and related as cause and effect. But this is an abstract point of view employed for a special purpose—the obtaining of measurements—and is qualified by the recognition of the complete conception of the organs as part of a self-conserving whole or system. This is all that is implied by the unfortunate term "teleology" when used in the theory of knowledge. What Kant professes to show is that this fact of nature cannot be reduced to or expressed in terms of the dynamical and statical relations of time and space. No doubt the laws of matter and energy apply in biology as strictly as elsewhere, but they do not express, much less exhaust, biological phenomena. And therefore we must be careful in biology not to distort those conceptions or hypotheses which are, despite assertions to the contrary, the necessary guides and interpreters of observation and experiments by the exclusive employment of categories which, like causation, neither are drawn from, nor are adequate to, the facts. The subject of the detailed effect of the neglect in this reference of Kant's warning I will not pursue here, as my brother has treated it at some length, with special reference to the objections made by Mr. Romanes, in a paper which will appear elsewhere. It ought to be borne in mind, as illustrating the point of view here emphasised, that Kant himself was one of the first to advance the nebular hypothesis. The truth is that, in speaking of the universe as having presumably originated from a mass of incandescent vapour, Kant, and everybody else, so far from reducing life to mechanism, is really raising mechanism to life. Kant would have told us that in the phenomena of such a developing mass there were potentially present all the relations of the universe as we know it. No doubt the approximate conceptions for the advance of knowledge are at this point the laws of matter and energy. But these do not exhaust the object, and if we have abstracted from the others we have done so in just the same way as we have abstracted from fact that the phenomena are there only for a percipient subject.

Such considerations and the doubts they raise may seem remote. But the number of those is increasing who think that they should be better known to and understood by men of science. It will not do to say that such criticism has no bearing on scientific inquiry until it has been ascertained whether its neglect has not already—even in matters of minute detail—misled and stultified certain phases of such inquiry. Fact and theory are not so very easy to distinguish. With scientific method no one wishes to interfere. But we would subject to closer investigation the question whether what are commonly taken to be the legitimate problems of science are really what they profess to be. It is not to the "Hannibals" of science, but to her Don Quixotes that Kant addresses himself.

R. B. HALDANE

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications.*

[*The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to insure the appearance even of communications containing interesting and novel facts.*]

The "Transmission Eastwards Round the Globe of Barometric Abnormal Movements"

THE following questions suggest themselves in connection with the above matter:—(1) Is it a fact that atmospheric movements of such small amplitude take the form of waves, and, if so, that the waves have so slow a rate of travel? (2) How is it to be accounted for that the waves travel eastward more slowly than westward? And (3) How can it be explained that they appear at an eastward station with a greater amplitude than at a westward?

With regard to the first question, it may be said that almost absolute proof of the existence of such waves can be brought forward. A recent investigation, the results of which are not yet published, has shown that a barometric wave measuring from maximum to minimum only ·108 inch, which occurred in Western India in 1877–78, was accurately reproduced over the same region three years later, namely, in 1880–81. The wave at the time of its reappearance had all the larger details which it possessed during its original appearance, these details agreeing in many cases to within less than ·001 inch. But whereas the amplitude in 1877–78 was ·108 inch, in 1880–81 it was only ·048 inch. Waves have also been recognised which appear in the summer half of the year at the northern part of Western India and travel southwards, arriving at the southern parts between two and three months after their appearance in the north; and also waves which appear in the winter half of the year at the south and travel northwards. This movement from north to south and from south to north during alternate halves of the year has been traced regularly since 1869; and indeed is so constant that in many cases it has been possible by means of it to calculate quantitatively the average position of the barometer during the next three months.

The second question was answered in my paper which you were good enough to publish in your issues of the 9th and 16th ult. Owing to the upper air currents travelling from equatorial to higher latitudes, and the lower air currents travelling equatorwards, there must be in high latitudes a general movement of the atmosphere eastwards, whereas in tropical and subtropical regions there must be a general movement westwards. This at once explains why in tropical and subtropical regions the atmospheric waves should travel more rapidly westwards than eastwards.

The third question is a difficult one, and the answer not perhaps quite satisfactory. If it were the case that undulations in fluid bodies become heaped up and increased in amplitude when travelling in a direction opposite to that of a current, and are affected in a contrary way when travelling with the current, a satisfactory answer might be furnished.

But an explanation may be sought in a different direction. If the circumstances of any latitude situated a little way from the equator be considered, it will be noticed that two principal air currents are flowing there—a lower one with a westward component of a certain velocity, and an upper one with an eastward component of a less velocity. Now it may be supposed that these two currents are affected by waves of two kinds, the first being waves common to the two currents, the second being waves which have been generated in the two currents in regions from which they are proceeding. And there are two reasons

why this latter class of waves should affect the barometer more strongly in the case of the current with the eastward component. In the first place that current is proceeding from the tropics, the region in which all great atmospheric movements originate; and in the second place there is a greater mass of matter moving with it than with the other current, there being not only an equal quantity of dry air returning northwards to compensate for that which is flowing southwards, but there being also a considerable quantity of water vapour, which does not return southwards in the form of vapour, but, having been precipitated as rain, returns with the ocean currents. It may not be easy, but still it is perhaps possible, to demonstrate how this fact should explain the greater amplitude of the eastward than of the westward transmitted waves.

A. N. PEARSON

Meteorological Office, Bombay, September 4

Apparent Disappearance of Jupiter's Satellites

ON the morning of October 15 next Jupiter will appear to be deprived of the satellites usually attending him. This s mewhat rare phenomenon has only been observed on four occasions during the present century, as follows:—

May 23, 1802	September 27, 1843
April 15, 1826	August 21, 1867

In 1826 the disappearance of the Jovian moons extended over an interval of 2 hours; in 1843 the interval was thirty-five minutes; in 1867 1 hour 45 minutes; but on October 15 next the phenomenon only endures 19 minutes (*i.e.* from 3h. 56m. to 4h. 15m. a.m.). The second, third, and fourth satellites will be in transit across the disk, while the first will be occulted by the planet. On August 21, 1867, the first, third, and fourth were in transit, while the second was eclipsed, and afterwards occulted. These occasions offer excellent opportunities for comparing the appearance of the satellites while in transit, and for re-detecting the dusky spots which were formerly distinguished upon them by Dawes, Secchi, and others. On August 21, 1867, I observed the phenomenon with a $4\frac{1}{4}$ inch refractor, and noticed that the satellites appeared nearly as dark and distinct as their shadows while projected on the disk of their primary.

There is a remarkable agreement in the intervals separating this rare occurrence. Between the disappearances of May 23, 1802, and April 15, 1826, there is a period of 24 years less 38 days (= 8728 days), and between those of September 27, 1843, and August 21, 1867, 24 years less 37 days (= 8729 days). The other intervals are irregular, there being 17 years 165 days between that of 1826-1843, and 16 years 55 days between that of 1867-1883. If, however, there is a regularly-recurring cycle of nearly 24 years, as the above dates apparently indicate, after every alternate disappearance of these satellites, then we may expect a repetition of the phenomenon on about September 7, 1907.

W. F. DENNING

Bristol, October 6, 1883

The English Viper

IN regard to the English viper, I send a small contribution to the information that Mr. R. Langdon seeks in your issue for August 2 (p. 319). During a residence of more than twenty years on the outskirts of the Forest of Dean, the following facts concerning the adder's bite came more or less under my notice:—

1. A girl was bitten on the thumb, she sucked the bite, and her head, throat, and tongue swelled so much that she nearly died of suffocation and starvation. She was laid up more or less for six months, and folks said that she was never herself again, but became "silly-like," but so far as my memory goes she was but weak minded before the bite.

2. A gamekeeper was bitten on the thick part of the hand. He could hardly get home, and did not leave his bed for three months afterwards.

3. A woman in the Forest was bitten on the thumb, her arm swelled, and became black, but on the application of a herb (which I cannot identify, though she called it "adder's tongue"), the swelling went down at once, and in a day or so no trace of the bite remained.

4. Though the following case did not occur in our neighbourhood, yet as the patient was a family connection, and the details were given me by his mother, I bring it forward. The young man was bi ten in the hand, and his arm swelled rapidly to such a size, that the coat sleeve had to be cut open. The youth was ill for many months, and more than a year afterwards had not regained his former mental and physical condition.

5. Cows were often bitten on the legs, but more often on the udder; they never died from the bites.

6. Sheep often died; and lambs, so far as memory serves, did so invariably.

7. A pointer was bitten on the chest. The bite did not bleed, but the dog swelled quickly and could not walk; it was ill for a long time, but did not die.

8. I remember hearing that a little girl had died from the bite of an adder; but I mention the case with little confidence, as it did not come within the limits of my observation.

In 1865 or 1866 adders were more numerous in our neighbourhood than the "oldest inhabitant" had ever known them to be. The farmers were advised to turn their pigs into the fields, and and the result was that wherever the pigs ranged the adders were nearly exterminated. A student of folk-lore would find a wide field in the traditions respecting the adder and its bite. In our neighbourhood the fat of the adder, especially that of the biter, was considered the best antidote for the bite. To roast an adder alive was not only a means of relieving the sufferer, but by making "the varmin squeal" it was said to draw others from their holes, and thus lead to their destruction.

KATHARINE B. CLAYPOLE

THOUGH not precisely in reply to Mr. Langdon's question, yet I add a short postscript to my wife's letter.

In this district we have two venomous snakes, the rattlesnake and the copperhead. The former is now becoming scarce, but the latter is still common. I have never been able to learn that any human being has been killed by the bite of either of these snakes in this neighbourhood. Bites of the rattlesnake are exceedingly rare, but I have known some, and heard of many persons who have been bitten by copperheads.

1. A lady was bitten on the foot at her garden gate; the leg swelled up to the thigh, and was exceedingly painful. She was more or less ill for a week.

2. A boy was bitten on the foot, and the leg swelled and turned black. No remedies were applied for many hours. A poultice of some herb which I have not been able to identify was put on the wound, and in twelve hours more the swelling had gone down, and the boy could walk.

3. In a third case of which I have heard the wound was said to reopen, or at least to become irritable, every year at the date of the bite.

4. A friend of mine had a dog which was bitten by a copperhead. He treated the wound with new milk, but the dog died.

5. In one case of which I have heard a man was bitten by a rattlesnake, but though I do not know the details of the case, the man is still alive.

6. A dog belonging to the friend mentioned above was bitten by a rattlesnake, and treated with new milk. He recovered.

I have heard of and known other cases of snakebite, but similar results followed. The remedies recommended for snakebite are too numerous to mention. Whisky in large doses is the most popular, and it never seems in such cases to produce intoxication. The common remedy—"the fat of the snake that bit you"—is, I suspect, an ingenious device for insuring the destruction of the reptile. It would appear as if the bite of the two snakes which I have mentioned can hardly be as deadly as is commonly supposed. The frequent swelling of the head and tongue appears to me to be caused by sucking the poison from the wound when a sore may have existed in the mouth. Much probably depends on the size and condition of the snake, the time of year, and the place and depth of the bite.

E. W. CLAYPOLE

New Bloomfield, Perry County, Pa., September 3

Solar Halo

I HAD the pleasure of witnessing, this morning, what Mr. Backhouse refers to in the last number of NATURE (p. 515) "as seen on rare occasions—a small portion of an ordinary halo brilliantly coloured."

Looking from a window at 9.40 a.m. towards the south-east, I saw a brilliant patch of light which for a moment I took to be the sun, but which I soon perceived was part of a solar halo, the sun being (roughly speaking) 20° distant in a horizontal line.

The colours were exactly those of the rainbow, especially at

the red end; at the violet the light was so brilliant as to appear almost white. The only clouds at the time were bars of white cirri, and it was across some of these that the halo showed itself. This lasted for eight minutes, and then began to fade as the cirri moved away, but the colours again brightened, and were still visible, even when the sky was apparently clear, although, where the patch of colour remained, very faint cirri could still be perceived behind and through the brightness. At 9.51 the whole had disappeared. The wind at the time was nearly due north. I should like to know whether these solar halos are considered to be produced by ice-crystals in the higher regions. They appear to me quite as prevalent in summer as in winter.

Great Malvern, October 2 E. BROWN

A Remarkable Rainbow

THE phenomenon of supernumerary bows noticed by "L. C." on September 24, has been repeatedly observed and described. Various explanations have been suggested; and "L. C." will probably find what he wants in Archdeacon Pratt's paper in *Phil. Mag.*, 4th series, vol. v. pp. 78–86 (1853).

A. RAMSAY

Meteor

A SPLENDID meteor was seen yesterday (Saturday) evening at about nine o'clock. It passed from the north-east, beneath the Pole star, to the west, where it vanished instantaneously without bursting. The nucleus measured, I should say, at least 5′ of arc in breadth, and was extremely brilliant. A. TAUN

31, Mornington Road, N.W., October 7

A Palæolithic Flake

IT may interest some of your readers to know that I found last week a Palæolithic flake in some gravel at Gray's Inn Lane, where they are now making excavations for sewers. It is a somewhat large, flattish, subtriangular flake of implement-like form, exhibiting a large cone on the plain side towards the butt, and the other side showing several facets; ochreous all over, and somewhat abraded. There is one in the British Museum from this spot, only it is an *implement*, black, lustrous, and spear-shaped, and seems to have come from a higher stratum than the flake before mentioned. Mr. W. G. Smith has an implement from Drury Lane—brought to him by an excavator instructed by him to look for implements at Shacklewell, and while at work at Drury Lane he found one, and, recognising it as an implement, brought it to Mr. Smith. It is subtriangular, worked all over on both sides, blackish indigo, lustrous, and very slightly abraded. These are as yet the only relics of Palæolithic man recorded as found in Central London.

49, Beech Street, E.C. G. F. LAWRENCE

Hop "Condition"

I OBSERVE that it is asserted in a German technical journal that the golden microscopic dust on hops, which English growers call "condition," and in which the finest properties of the hop are supposed to reside, does *not* increase in quantity, as generally it is supposed to do, with the growth of the inflorescence. The quantity on the plants is declared to be as great when the buds are first developed as at maturity. Can any of your readers oblige me with observations or references in point?

H. M. C.

JOACHIM BARRANDE

THE announcement that Barrande has passed away will be received with sincere regret in every quarter of the globe where geology is cultivated. His death severs another of the few remaining links that connect the present generation of workers with the early pioneers of geological science. Born in 1800, he was eventually appointed tutor to the young Duc de Bordeaux. So attached did he become to the royal family of France, that when Charles X. abdicated he voluntarily went into exile, accompanying his young pupil to Prague, which remained his domicile thenceforward to the end of his long life. It was during the early years of his exile that he gave himself to natural history pursuits. In a brief visit to Vienna he came upon a copy of Murchison's "Silurian System," then recently published, and finding some of the fossils therein figured to resemble others which he had himself picked up in Bohemia, he on his return began to look more attentively at the rocks of his neighbourhood. Getting more interested with every fresh excursion, he began to open quarries and employ workmen to search for fossils. In order the more easily to direct their work he laboriously acquired their language. Year after year he continued these researches, devoting to them his time, energy, and fortune. He became the prince of fossil collectors. But at the same time he applied himself with unwearied industry to the scientific study of the fossils and of the rocks containing them. By degrees his labours took shape, and there resulted from them his colossal work, the "Système Silurien de la Bohême," a noble monument of scientific enthusiasm. It was begun as far back as 1852. Since that time no fewer than twenty-two massive quarto volumes of text and plates have been published. Undeterred by the remonstrances of a publisher who would insist on counting the cost and the sale, Barrande was his own publisher, and prosecuted his labour of love down to the end of his life. His numerous separate papers on geological subjects began to appear in 1846, and have been continued to the present time. Living in exile for upwards of half a century, Barrande occasionally visited his native country, and took a keen interest in scientific progress there, but remained an unflinching royalist, refusing to do anything or accept any distinction which might seem to compromise his political principles. He even declined to be nominated a corresponding member of the French Academy. But honours were heaped upon him by the scientific societies of other countries. Due tribute will no doubt be paid to his scientific achievements; for the present we have time only to offer these few lines to the memory of one of the most unwearied and profound students of palæontology, and one of the most upright and honourable of men.

THE SANITARY CONGRESS ON HOUSE SANITATION

A CONSIDERABLE amount of attention was given at the recent Congress of the Sanitary Institute in Glasgow to the question of house construction, and to the evils which are attendant upon the present system under which human habitations are erected both in the metropolis and elsewhere. When it is remembered how large a portion of time the inhabitants of this country are compelled, by reason of climate and otherwise, to spend inside their dwelling houses, it is obvious that the health both of the present and of future generations must be largely dependent on the sanitary condition of those dwellings, and that very earnest consideration should be given, both by experts in matters of building and also by the public themselves, to the sanitary details of house accommodation. And yet it is notorious that houses, which are faulty in almost every particular relating to health, are week by week being run up by hundreds and thousands; that even where money does not enter into consideration the dwelling-rooms of mansions are left without any provision for ventilation whatever; and that both the wealthy and the poor are stricken with disease by reason of the foul air which has been conveyed from the sewers into their homes as the result of arrangements which are, in point of fact, almost always more costly than should have been the more simple appliances which would have prevented the possibility of such an occurrence.

As the law now stands there are certain evils which

cannot be controlled either by any existing statute or under by-laws. Thus, whereas a reasonable width of street may be secured by means of a by-law, there is absolutely no provision to prevent the erection of houses of such a height as effectually to exclude sunlight, and so it comes to pass that windows open, not on to bright, dry, open spaces, but into comparatively narrow thoroughfares which tend to remain damp and imperfectly lighted. Prof. Tyndall's experiments as to the arrestation of infusorial life by solar light should alone suffice to secure for the spaces about our dwellings ample exposure to the rays of the sun, for he has clearly shown that, after infecting certain sterilised infusions and exposing one set where no sun could reach them and another set to the influence of the sun, infusorial life was much more rapidly developed in the former than in the latter; and this notwithstanding the fact that the temperature of the flasks exposed to solar influence was far more favourable to the development of low forms of life than was the case as regards the others. It has also been decided by the law officers of the Crown that the height of rooms cannot be regulated either under any general statute or by means of a by-law. As to this, however, we note that Mr. John Honeyman, a well-known architect, strongly advocated at the Congress the desirability of low ceilings in small houses, alleging that such an arrangement, by inducing economy in construction, facilities for warming, and other incidental advantages, would tend to prevent overcrowding and also add to the comfort of the lower classes. There can be no question that wherever the height of a room is such that the upper portion becomes a mere reservoir for overheated, stagnant, and vitiated air, and whenever adequacy of floor space per head of the occupants is sacrificed on account of an increased cubic space resulting from a high ceiling, then distinct harm results from the loftiness of the apartment; but, due regard being paid to ventilation by means of windows opening nearly up to the ceiling level and other contrivances, a reasonably high apartment has distinct advantages over many of the low ones which are now constructed.

Turning, however, to matters which are well within the control of sanitary authorities, the members of the Congress were unanimous in condemning the present system by which dwelling-houses are now constructed. Thus, instead of covering the ground surface of the sites of new houses with concrete so as to prevent both moisture and effluvia from any neighbouring leaky drain from ascending into the dwelling, the builders round about London and elsewhere either put their brick foundations directly on to the clay or other soil, or else they provide a material which can only be regarded as a make-shift in so far as imperviousness is concerned, and even this is only placed immediately beneath the house walls. Then again, pieces of tarred felt are inserted in the place of adequate damp courses, and so it comes to pass that, within a few months of occupation, the residents are, apart from other evils, exposed to one of the principal predisposing, if not exciting, causes of phthisis. As for drainage, this work can, as soon as completed, be hidden out of sight, and it is notorious how much illness has resulted, and how many lives have been sacrificed, to the want on the part of builders of the most elementary knowledge in connection with the construction and adequate ventilation of house drains.

In dealing with these and other allied subjects, the several speakers paid a tribute of praise to the Model By-laws which have been issued by the Local Government Board, and which in their annotated form fully explain, by means of diagrams and otherwise, how all the various health and other requirements may be most effectually provided. But even where such by-laws have been adopted, we fear they are in many instances not enforced; and evidence was given at the Congress to the effect that the principal offenders are themselves often members of the authorities whose duty it is to see the several provisions carried out. As long as this is the case, subordinate officers can hardly be expected to perform their duties efficiently, and the principal remedies needed are, firstly, by means of congresses, lectures to working men, and such measures, to spread broadcast, and in an easily acquired form, a knowledge as to the elements of house sanitation; and, secondly, a determination on the part of the public to elect as members of local authorities only those who have such knowledge and who will use it for the public benefit.

THE ASTRONOMISCHE GESELLSCHAFT

[From our Vienna Correspondent]

The meeting of the Astronomische Gesellschaft was held this year on September 14, 15, and 16, in the Academy of Sciences in Vienna. There was a good attendance, and among others present we observed the astronomers Auwers of Berlin, Gould of Cordoba, Pickering of Cambridge, U.S., Elkin of the Cape, Lœwy and Janssen of Paris, Foli of Liége, Gylden of Stockholm, Engstroem of Lund, Oudemans of Utrecht, Foerster of Berlin, Vogel of Potsdam, Gautier, sen. and jun., of Geneva, Thiele and Pechule of Copenhagen, Wagner, Hasselberg, and Dubiago of Pulkova, Bruhns of Leipzig, Wolf and Schönfeld of Bonn, Gruey of Besançon. England was represented by Prof. G. Forbes. The head of the Ministry of Public Instruction in Austria, Dr. Siedler, having welcomed the assembly in the name of the Government, the president, Prof. A. Auwers, briefly addressed the Congress. For the second time, he said, the Congress held its sittings in those halls. The first time they met there they found in this building the old observatory. They now beheld an institution which in magnificence was hardly matched by any other institution in the world and surpassed by none. The President then thanked the Government for their friendly welcome, and the assembly for the large attendance present. The subjects which were the order of the day were then taken up. From the report of President Auwers on the great zone undertaking, it appeared that the observations of all the observatories in connection therewith might be deemed completed, so that next year they would be in a position to enter on the printing of the official catalogue. It was further shown that the preparations for extending this enterprise to the south as far as 23° or thereabout, southern declination, an object which for several years had been in contemplation, were so far advanced that the scheme might now be considered as secured. In the course of the three sittings of the Society a large number of interesting addresses were given and demonstrations made, most of them followed by lively discussions.

Prof. Bruhns spoke on astronomical refractions, and on the formulæ according to which from the observed refraction the law of reduction of temperature in the atmosphere might be determined. Gylden referred to investigations he had made on the subject of the perturbation theory of planets, and to the labours of the Stockholm Observatory towards drawing up tables of planets in accordance with his theory. Prof. Weiss (Vienna) produced the two printed volumes of the annals of the Vienna Observatory, as also the first sheets of his new edition of "The Wonders of the Heavens" by Littrow, and showed drawings of Jupiter and Saturn, executed by help of the 27-inch instrument of the Vienna Observatory, together with drawings of lunar maculæ taken by means of the 12-inch instrument of the same observatory. Photographs of the sun's corona taken in full daylight, sent by Dr. Huggins and laid before the meeting by

Prof. Weiss, were received with much approval. Dr. Elkin reported parallax determinations of southern stars executed by him and Gill at the Cape, especially that of α Centauri, which might be fixed at about ·75 sec., and that of Sirius, which increased to ·4 sec.

Pickering drew attention to photometric investigations carried out at the observatory of Harvard College. Janssen (Meudon) spoke on the observations which had been made in connection with the sun's eclipse of May 6 this year, discussed the photographs of the corona they had taken, and referred to the efforts made by Palisa with a view to discovering an intra-Mercurial planet, efforts which, as was well known, had yielded a negative result. Prof. Foerster gave an interesting account of observations made in the Berlin Observatory, by which he endeavoured to prove that the one ground-pillar of the Berlin Observatory on which investigations had hitherto been conducted had for the last twenty-five years been subjected to angular movements which were connected with the eleven years' period of solar spots. The annual averages of inclinations of the pillar from 1856 to 1881 adhered as closely to Wolf's relative numbers as did the annual averages of the magnetic declination. From these facts Prof. Foerster drew the conclusion that very considerable effects could be traced from the radical changes of the eleven years' solar period. In connection with this communication of Foerster's, B. A. Gould reported how he had made quite similar observations on the sea coast. Prof. Oppolzer (Vienna) spoke of investigations he had made in the Vienna Observatory with a view to determining the length of the seconds pendulum and the influence of the simultaneous oscillations of the stand. Foli gave an account of his examinations into the daily nutation and precession of the earth and his new tables of precession, and communicated some information regarding the observatory of Liége. Pechule (Copenhagen) sought to refute Stone's assertion that there was a difference of 1½ sec. between the former and the present Julian year, an assertion which had already been declared by Airy to be incorrect. He pointed out where Stone had committed an error in his calculations, and brought forward proof to show that the difference amounted to but four seconds every thousand years. Oudemans (Utrecht) corrected an assertion of Stone's that there was an error of 28 sec. in the reduction from median to sidereal time. Steinheil (Munich) referred to new constructions of telescopes calculated by him, and to the influenee of the prism in the case of refracting telescopes on the sharpness of the image, in respect of the achromatism of the images, and to the means by which he counteracted the prejudicial effects in such cases.

On Sept. 15 the members of the Congress visited the Observatory. The astronomers were highly satisfied with the arrangement of the observatory and the system of apparatus. The 27-inch instrument, supplied by Grubb of Dublin, and described in NATURE shortly after its completion, was an especial object of interest. Unfortunately the sky was clouded, so that it was impossible for the astronomers to carry out the observations they had intended with this powerful instrument. In reference to administrative matters we have also to mention Foerster's report on the conclusions of the International Commission respecting Kiel as an international centre, particularly in regard to the contributions of the respective astronomical institutes. On September 16 a social excursion was made to the neighbouring Kahlenberg, and this event was also made the occasion of the baptism of several newly discovered planets; the planet 234, discovered by Peters, receiving the name of Barbara; the three discovered by Palisa (Vienna) being called (229) Adelinda, (231) Vindobona, (225) Henrietta. It was resolved that the next meeting of the Society should be held at Geneva in 1885.

THE NORWEGIAN CIRCUMPOLAR STATION

IT is with pleasure that I respond to the invitation of NATURE to give an account of our labours here during the last twelve months, and I may, in doing so, state that I have purposely delayed writing these lines, in order to be able to give the result of our researches during a whole year.

The Norwegian Government have contributed their share to the international research of the physical conditions of the Polar regions by the establishment of the observatory here at Bossekop in Alten (Finmarken). The station, which is situated at the bottom of the Alten Fjord in 69° 58′ lat. and 23° 15′ long., commenced its labours on August 1, 1882, which are to be continued until September 1, 1883, in accordance with the programme of the Polar Congress held in St. Petersburg in 1881. The equipment and instruments of the station, as well as its whole organisation, are also in accordance with the principles formulated by the Polar Congresses held in Hamburg in 1879, in Bern in 1880, and in St. Petersburg in 1881. The *personnel* of the station consists of the writer as chief, Doctors C. Krafft, sub-chief, J. Schroeter and F. Hesselberg, observers, and Herr O. Hagen, instrument maker and caretaker.

The obligatory observations embrace astronomical observations, readings every hour of the meteorological and magnetic variations, absolute measurements of the terrestrial current's three components, and studies of the aurora borealis. For the hourly observations the day and night have been divided into four watches of six hours each, which are taken by each one in turn.

In order to effect the astronomical observations, *i.e.* the determination of time and place, a small observatory 25 m. in length, breadth, and height has been erected of deals, the roof and the southern and northern walls of which are provided with shutters to be opened during meridian observations. On a fixed pillar in the centre is placed a universal instrument by Repsold, besides which we possess three box chronometers by Frodsham, Kessels, and Mewes, the last named being regulated by star time, as well as two pocket chronometers by Kessels and Bröcking.

The weather during the winter and certain other circumstances have to some extent affected the astronomical observations, so that for the determination of time only a few meridian passages have been observed; but this circumstance is of no consequence whatever, as the station is independent of local determination of the time for ascertaining the absolute time, viz. the common mean time of Göttingen, which has been adopted at all the Polar stations as the common time, and which we receive from the observatory at Christiania through the telegraph office, about ten minutes distant, twice a week, at 9 a.m. on Sundays and 8 a.m. on Wednesdays, Greenwich time.

We have, however, a few meridian passages and observations of time with corresponding sun altitudes, which may be used for the verification of the longitude of the place. The Polar altitude has been verified by the measuring of circummeridian altitudes of the sun and Polaris. The universal instrument has also been employed for the determination of azimuth for the observations of the aurora borealis and the magnetic declination.

Of magnetical instruments we have a set for the determination of the elements of the terrestrial current, viz. a unifilar magnetometer by Elliott Brothers of London, and a Dover inclinatorium, both verified at Kew. The variation instruments, which are constructed on Lamont's principle by Prof. Mohn, and executed by Herr C. Olsen, optician, of Christiania, consist of a variation instrument, a unifilar apparatus with two fixed deflectors for the observation of the variations of the horizontal intensity, and a unifilar apparatus with vertical bars of soft iron as deflectors, whose magnetic moment varies accord-

ing to the changes in the vertical intensity. We have two sets of these instruments exactly similar in construction, of which one is kept in reserve. All the magnetical instruments are erected in the magnetic observatory in the manner shown in the diagram (Fig. 1). This observatory is divided into three sections, and arranged as follows:—Furthest to the east is the variation chamber, which has, in order to make room for both sets of instruments, been made rather large, viz. 7·5 m. long, 5 m. wide, and 2·8 m. high. It has been constructed partly under the earth's surface, the ground having been hollowed to a depth of about 1 m., where the floor has been laid, and the mould cast up along the outer sides of the hut, which have first been covered with birch bark, and in turn by turf. The roof has first been covered with fireproof paper, and then with turf and mould, which gives to the observatory the appearance of a subterranean chamber. The object of this is, of course, to keep the temperature in the room as constant as possible, which we have in fact fully succeeded in doing, as the diurnal variations have seldom exceeded 5° to 6° C., while averagely the temperature during twenty-four hours has only varied from 2° to 3°. The lowest temperature registered in the chamber during the winter was − 3° C. during a high wind, while the highest during the summer was 23° C. Of windows there are none; but lighting is effected by means of four petroleum lamps, which are kept

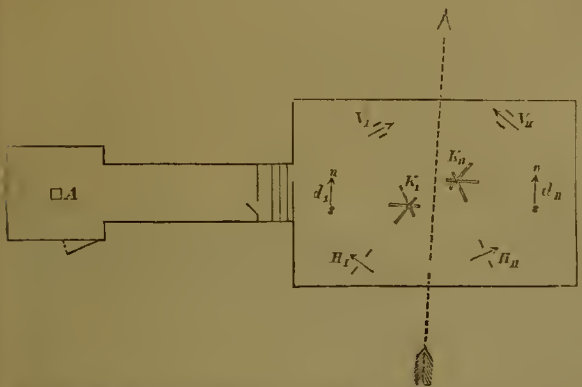


Fig. 1.—Diagram showing the plan of the Magnetic Observatory. d_I d_{II}, variation instruments for the declination; H_I H_{II}, variation instruments for the horizontal intensity; V_I V_{II}, variation instruments for the vertical intensity; K_I K_{II}, the reading telescopes; A, pillar for absolute measurements; ⟶, magnetic meridian.

burning day and night, and to the faint heat emitted by these I ascribe the circumstance that the average temperature during the winter was several degrees above freezing-point, and this under an open-air temperature of − 20° C. From the great variation chamber a small ladder leads through an aperture in the western cross wall up into a narrow corridor 5 m. by 1·5 m., which opens into the chamber for the absolute measurements. This is 2·5 m. in length, width, and height, and from this a door leads out into the open, while it is also provided with a small window in the western wall. In the corridor is a partition with a door, in order to prevent as much as possible any change in the temperature in the variation chamber. The building is constructed of rough deals, while the use of iron has, of course, been carefully avoided. The whole has been joined by means of wooden pegs, and the roof-paper fastened with copper nails. The lamps have been hung on brass wire or placed on wooden tripods; while the hinges of the doors are of brass, and the handles of wood.

In the variation chamber eight pillars have been raised of iron-free bricks and cement. These pillars rest on large slates, which have been laid at a depth of about 7 decimetres below the floor, and run free through openings in the floor. The tops of the pillars have a marble slab attached to them, on which the instruments are placed, viz. one on each of the six pillars, and a reading telescope on each one of the remainder.

The needles in the six instruments are exactly alike in size as well as in magnetic moment. They are made of lamellar watch-spring, separated by three small bits of brass, and are about 9 cm. in length. Above the needle, and parallel with the same, a mirror is affixed, from which the image of the scale (paper on spruce wood), placed perpendicularly on the reading telescope, is reflected in the focus of the telescope. Each scale is one metre in length, but even this comparatively great length has, as regards the intensity instruments, been found insufficient during great perturbation, and we have been compelled to lengthen the scale on the side most exposed by adding an auxiliary scale. The scale is divided into millimetres, the distance between mirror and scale being exactly 1719 mm., making the value of the angle of one part of the scale exactly 1′. The reading telescopes of the two sets are fixed on a common vertical axis, the one for horizontal intensity being highest, the one for declination in the centre, and the one for vertical intensity lowest. This is also the order in which the instruments are read, a reading of all three being easily effected in fifteen to twenty seconds.

The regular magnetic variation observations are, as previously stated, effected every hour, seven readings being taken of each instrument. At fixed terms, viz. on the 1st and the 15th of the month, readings are effected every fifth minute, while, during a certain hour of these two days, the variations of the declination and the horizontal intensity are read every 20th second. Magnetic disturbances, some even of great violence, have during the twelve months been the order of the day here. Thus when perusing our "log," one will hardly find five days in average in each month during which the needle has remained completely at rest throughout twenty-four hours. Little jerks or oscillations have constantly occurred, particularly during the night, when the disturbance has generally been greatest, and it has not been an uncommon occurrence that the readings have fallen out on the auxiliary scale, oftenest, however, as regards the vertical intensity.

The zero of the variation instruments is partly controlled by direct simultaneous observations of both systems, compared once a week, and partly by absolute measurements of the three terrestrial magnetic elements effected several times during the month. The unifilar magnetometer is employed for determining the declination as well as the horizontal intensity, the latter embracing both vibratory motions and deflections with the magnet deflector at two different distances. The effect of the torsion on the position of the declination needle is also determined by deflections in the manner advocated by Lamont in his "Handbuch der Erdmagnetismus," § 91. The inclinometer is provided with four needles, of which three are in use, and during the last half year all three have been used during inclinatory observations, which have given results with a pretty uniform difference. As a matter of course, every reading during absolute measurements has been accompanied by a stringent and simultaneous reading of the corresponding variation instruments.

For the study of the aurora borealis we have employed a theodolite which has been constructed by Prof. Mohn, and finished by Herr C. Olsen. The theodolite has an excentrically attached conic tube, which serves as drop. The small ocular end is closed by a disk in which is a circular hole a little larger than the pupil of the eye. The objective end, which forms the basis of the conus, is completely open, with a cross of thin steel wire suspended therein. The one half of the curved surface of the tube, the ocular part, is a solid brass pipe, while the

other half, the objective part, is constructed in open trellis work, partly in order that the wire cross may easily be seen when a lantern is held at its side, and partly on account of the balance. The horizontal as well as the vertical circle, is provided with noninus, whereby 10′ is read directly and 1′ may easily be ascertained. To the horizontal axis belongs a libella, on which each part represents 1′. A massive base of cast-iron—a vertical column with three projectors at the foot, each one with a foot-screw—gives the instrument an exceedingly solid rest, and keeps the due level for weeks when the instrument is not subjected to gales or other violent exterior influences. Our station possesses two such theodolites, while the Swedes at Spitzbergen have three, the Dutch on board the *Varna* two, and Dr. Sophus Tromholt, at Kautokeino, one.

Auroræ have been seen here during the winter almost every night, and during all weathers, thus, even behind cumulo-stratus clouds, oscillating waves of the aurora borealis have at times been observed. Proper measurements have, however, of course only been effected on clear nights. With Dr. Sophus Tromholt, who has, as the readers of NATURE know, from his communications to this journal, during the winter sojourned at Kautokeino specially for his researches on the aurora borealis, we have arranged to measure at agreed periods the elevation of the aurora in the common plane Kautokeino-Bossekop. We have effected a number of such measurements here, which will, I believe, give important results as regards this phenomenon, when compared with those made by Dr. Tromholt. A closer auxiliary station for parallax measurements was also contemplated here, and to this end I had a pillar raised in an open place for a theodolite, about seven kilometres south of our station, but the want of telephonic connection and assistants, I regret to say, prevented this project from being carried out.

The auroral forms or types which have appeared here have been those generally known, from the grand corona to the modest pulsating little luminous cloud, but as a characteristic feature attending them all I must mention

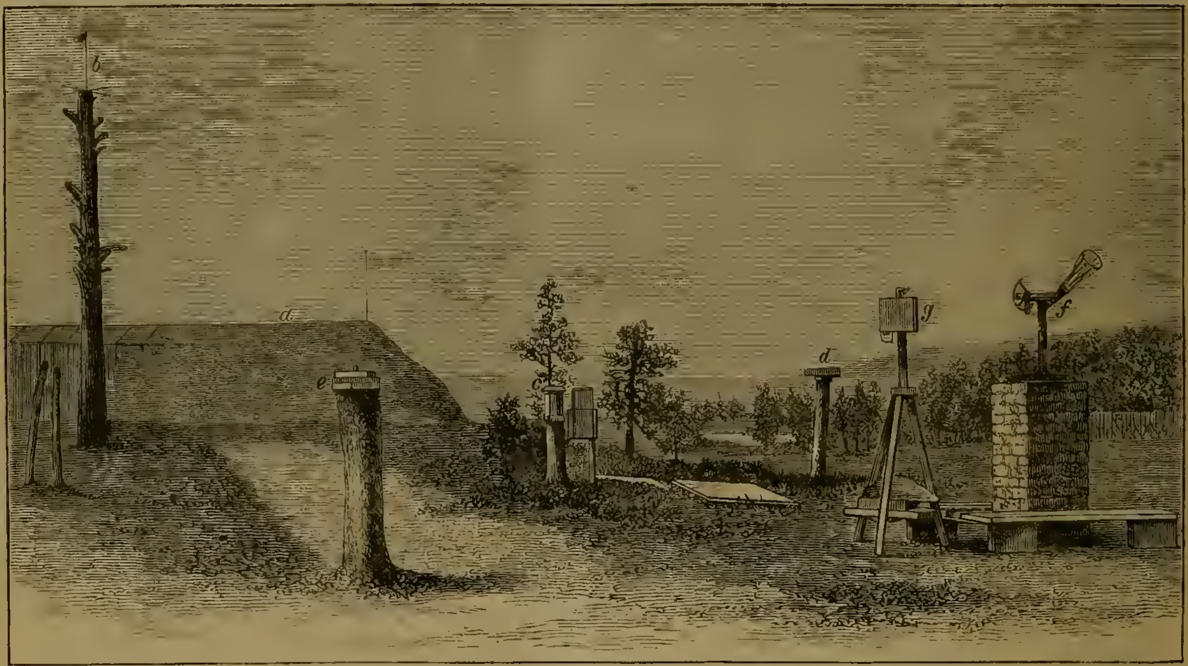


FIG. 2.—The Norwegian Circumpolar Station at Bossekop. *a*, magnetic observatory; *b*, vane; *c*, rain-gauge; *d*, black-ball thermometer; *e*, transit instrument; *f*, auroral theodolite; *g*, instrument for recording the auroral observations.

the absence of stability in the types. Thus only on a few occasions has there been an opportunity of watching the quiet stationary arc, but in general the auroræ have represented wafting draperies and shining streamers with ever-changing position and intensity.

As often as there has been an opportunity, spectroscopic researches have been effected with a Wrede's spectroscope. The well-known yellow-green auroral line has always been observed, and on one occasion also the red, the position of which we succeeded in fixing approximately in the spectroscope.

The meteorological instruments with which we have been furnished are similar to those in use at the meteorological observatory in Christiania. A Kew station-barometer (Adie) is employed at the hourly observations, while a Fortin barometer (Secretan) serves as normal and control barometer. Generally a few times a week both barometers, which hang parallel in the office of the station, are compared. As a reserve instrument we have an aneroid, but this we have fortunately had no need of using.

For the protection of the thermometers we have erected a wooden cage on four poles facing the north, with blinds, a double back wall and a roof, exactly in conformity with Wild's model. In the centre of the cage is the actual thermometer box placed, of sheet iron and with a free circulation for the air, in which is to be found dry and wet thermometers (divided into $0^{\circ}\cdot2$ C.), as well as maximum and minimum ones. All the thermometers, which were manufactured by Aderman of Stockholm, have several times during the winter been examined as to the stability of zero, which has always been found perfectly correct. Besides these a black-ball thermometer has during the summer been erected, and read several times a day. Psychrometer readings have on some occasions been controlled at low temperatures by observations with Allnard's modified form of Regnault's hygrometer (Golaz, Paris), while absolute determinations of moisture

through weighing have been attempted by Dr. Krafft with a chemical weight (Bunge), however without result, caused chiefly by the circumstance that he was unable to give the instrument the proper rest. The observations of the directions of the wind have been made by a weather-cock fixed on the top of a stripped fir tree, the force being registered by Beaufort's scale, and the velocity partly by Mohn's hand thermometer, which are used during the hourly observations, and partly by means of a Robinson anemometer placed on the roof of the dwelling-house, which is read once in twenty-four hours. Further, one of Hageman's anemometers is erected in the office, from which the conductor, made of indiarubber and lead pipes, is carried outside the house and along the flagstaff in such a manner that the absorbing point or tube is situated a couple of centimetres above the knob of the same. The observations with this anemometer have, however, not given so satisfactory results as might have been expected from my experiences in other places. The cause of this is no doubt the circumstance that the instrument used was not a new one, and consequently, perhaps, not very sensitive, while the position was, we found, not the most advantageous. On the bare ground we have placed two rain-gauges—the one square and the other round—with a receiving surface of 225 square cm. each. The rainfall here is very small, averaging only 267 mm. per year, but during the past twelve months it has been rather less.

The measurement of the snowfall we have found almost an impossibility, on account of the frequent gales during the winter, which sweep the snow away as quicksand from one place and deposit large drifts in others.

Every month measurements of the temperature of the sea have been effected, in the Alten Fjord, with one of Negretti and Zambra's deep-sea thermometers. The depth is 100 English fathoms, and the temperature read at every tenth fathom. We have during these researches discovered that from the bottom and 10 to 20 fathoms upwards the temperature keeps constant throughout the year, whereas in the layers above this depth some very interesting variations occur with the seasons.

Last winter here has been milder than we anticipated, the lowest temperature registered being $-21^{\circ}\cdot7$ C., which was read by the minimum thermometer at 8 a.m. on December 31, 1882. Under high wind such a temperature is unpleasant enough, and with gales we have several times been favoured. Thus, on October 5 the velocity of the wind under a storm from the north-west was 26 metres per second, and on later occasions the anemometer has not seldom shown a velocity of from 10 to 20 metres per second.

Our labours at this station are now approaching their completion, and it is satisfactory to me to be able to state that no accident has occurred to our instruments, the accuracy of which has been controlled throughout in various manners, and that the scientific researches have been continued during the entire year without a single interruption.

What the ultimate results of our researches during our sojourn here will be it is of course at the present moment an impossibility for me to state, but I feel confident that, when all the materials of research have been collected from the various circumpolar stations and compared, it will be found that the Norwegian station at Bossekop has formed an important link in the chain of international meteorological research around the Pole. AKSEL S. STEEN

Bossekop, Finmarken, Norway, August

A NATIONAL LABORATORY OF MARINE ZOOLOGY

It is pretty well understood that the Executive Committee of the London International Fisheries Exhibition of 1883 will have a sum of money in hand when all expenses connected with the Exhibition are paid, amounting to some thousands of pounds. The gentlemen who have organised and carried through this very successful enterprise are to be congratulated on the popularity which has attended the Exhibition and on the amount of interest which they have excited in all classes of the community in matters relating to our national fisheries. Not only this, but the Committee deserves hearty thanks for the valuable series of pamphlets on subjects connected with fisheries which it has printed and circulated far and wide. These pamphlets are for the most part reports of lectures delivered by highly competent specialists at the "Conferences" inaugurated by Prof. Huxley under the presidency of the Prince of Wales, and amongst them are such important essays as that of Prof. Hubrecht on oyster culture, of Dr. Day on the food of fishes, of Prof. Brown Goode on the fishery industries of the United States, and of Mr. Duff on the herring fisheries of Scotland.

It is not surprising that at the present moment suggestions should be offered from various sides to the Exhibition Committee with reference to the best use of the surplus funds in its hands. No one will pretend for a moment that the Committee has not the full right to make what use of those funds it may deem most fitting; and the public has every reason to feel confidence that the ultimate decision of the Committee will be made with the intention of doing what is best for the national interests bound up with our fishery industries. At the same time it is a legitimate thing for men of public position and responsibility to place before the Committee suggestions as to useful modes of employing the surplus funds in its hands. Accordingly we note with satisfaction that a number of our leading biologists, whose opinion upon this matter is certainly entitled to very great weight, have placed before the Committee a suggestion for the foundation of a laboratory upon the British coast, which shall be devoted to the study of marine animals and plants in relation to fisheries. A similar proposal has also been independently placed before the Exhibition Committee by the executive of the British Association for the Advancement of Science.

It is a very striking fact that the one point in which all speakers at the Conferences held during the past summer at the Exhibition were agreed was this: that our knowledge of the habits, time, and place of spawning, food, peculiarities of the young, migrations, &c., of the fish which form the basis of British fisheries, is lamentably deficient, and that without further knowledge any legislation or attempts to improve our fisheries by better modes of fishing, or by protection or culture, must be dangerous, and indeed unreasonable. Prof. Brown Goode, the United States Commissioner, declared at the Conference on July 20 that "the spread of actual scientific knowledge concerning fish and fisheries was one of the things which, above all others, would be the most profitable and satisfactory outcome of this Exhibition." At the same Conference Prof. Hubrecht, the Netherlands Commissioner, said that "he endorsed from the bottom of his heart the principle that there must be inquiry, and still further inquiry, before legislation based on scientific and accurate principles could be carried out. They must take as a motto, more knowledge, more science, more zoology." On the same occasion the Duke of Argyll referred to the suggestion which had been submitted to the Conference to the effect that the foundation of a laboratory of marine zoology might well be undertaken by those who had organised and carried out the International Fisheries Exhibition. Speaking with the authority of one well acquainted with the Scotch herring fisheries, as well as with the knowledge of an accomplished naturalist, he stated that in his judgment this suggestion was a most important one, which he hoped would be brought forward in the proper quarter, and that he should give all the help he could in the matter.

The memorandum which we print below briefly sets forth the proposal as now submitted to the Executive Committee of the Fisheries Exhibition. It has been signed by the following naturalists:—Sir John Lubbock, Mr. P. L. Sclater, Prof. Jeffrey Bell, Prof. Michael Foster, Prof. Burdon Sanderson, Prof. Flower, Prof. Allman, Prof. Richard Owen, Dr. G. J. Romanes, Prof. Lankester, Prof. Moseley, Dr. Carpenter, Mr. John Murray, Mr. Thiselton Dyer, Prof. Milnes Marshall, and Mr. Adam Sedgwick. The absence of the names of one or two influential zoologists from this list is explained by their official connection with the Exhibition, which has rendered it undesirable to ask them to commit themselves in reference to a question in the consideration of which they will ultimately have the greatest responsibility and weight.

The memorandum runs as follows:—

Proposal for the Foundation of an Observatory on the British Coast for the Study of Marine Animals and Plants in relation to Fish and Fisheries.

The value to the fish industry of an increased knowledge of the habits and life-history of fishes has been proved by the experience of the American and French Commissions. Without such knowledge we cannot improve our fisheries commercially; with it, there is every probability that a great deal may be done in the way of controlling and extending them. In order to gain accurate knowledge as to the circumstances which affect the life of fishes, and the various mollusks, shell-fishes, corals, and sponges, which are important commercially as well as interesting from the scientific point of view, it is necessary that continuous observations should be made upon their growth from the egg onwards, upon their food and its natural history, as well as upon their enemies and the conditions favouring, or injurious to, their life. Such observations can only be successfully carried out by persons resident on the sea-coast. In order to enable competent observers to spend such time as they can afford for these studies to the greatest advantage, zoological observatories have been established on the sea-coast of foreign countries, but at present there is no such observatory on the British coast. The first observatory of the kind is the "zoological station" established by Dr. Dohrn at Naples, which is frequented by naturalists from all parts of Europe. Its buildings and aquaria represent an expenditure of 20,000*l.*, and its annual expenditure is over 4000*l.* Similar observatories have been established by the Austrian Government at Trieste, and by the French Government at Concarneau, Roscoff, and Villefranche. It has been for some years the desire of English naturalists to establish a zoological observatory on the British coast, which would be in charge of a competent resident superintendent, and fitted with aquaria, laboratories, and apparatus, and possessed of boats and dredging apparatus. Two or three fishermen would be kept in the pay of the observatory. The institution thus organised would be frequented at all times of the year by naturalists desirous of carrying on original investigations relative to the life-history and structure of marine organisms. Accommodation for as many as six such naturalists might be provided. The affairs of the observatory and the granting of permission to make use of its appliances might be intrusted to a small committee, consisting (for example) in the first place of the Warden of the Fishmongers' Company, the professors of zoology, botany, and physiology in the universities of Great Britain, and in the London colleges, and the secretaries of the Linnæan and Zoological Societies of London. Were such an observatory once established, there is every reason to believe that funds could be raised annually for the purpose of extending its operations, and of carrying on special work in it by grants from scientific societies, the universities, and such sources. The obstacle hitherto to the establishment of a British zoological observatory has been the difficulty of obtaining the large sum necessary to launch the institution. It is calculated that 8,000*l.* would be sufficient to secure a site and erect and furnish a suitable building—whilst 500*l.* a year should be secured as a minimum income for the purpose of paying a salary of 250*l.* a year to a resident superintendent, minor salaries to fishermen and attendants, and of meeting the small current expenses. The income of the institution might be materially aided by the payment of a fee (say 5*l.* a month) on the part of those naturalists making use of its resources. The opportunity for securing the 20,000*l.* necessary for the inauguration of such a zoological observatory has presented itself in connection with the International Fisheries Exhibition. Should there be, as there is reason to hope, a large surplus fund in the hands of the Committee of the Exhibition at its close, it is proposed to bring the suggestion of the establishment of a Marine Zoological Observatory before the Committee, and to endeavour to obtain the support of that body for the scheme. It is proposed that a deputation of scientific men should interview the committee of the Fisheries Exhibition, in order to explain the importance of a marine observatory and the close relationship of the work done in such an institution to the interests of our fisheries; and the Committee would then be asked to consider the propriety of handing over the sum of 20,000*l.* (or if possible a larger sum, this being a minimum) to trustees, for the purpose of building and endowing such an observatory, provision being made as to the future government and occupation of the observatory, as above suggested.

NOTES

At the opening of the London Hospital Medical School, Prof. Huxley gave an address on the relations of the State to the medical profession. He considers the present relations on the whole satisfactory, and that it is not desirable that the State should do more than it does to protect the public against incompetent persons and quacks. He thinks that no license should be granted except for the three qualifications, and that the course of study should be extended somewhat backwards, by insisting, instead of the general education test, upon some knowledge of elementary physics, chemistry, and so forth, by the young man desirous of entering upon a course of medical studies. In conclusion, he referred to the want of organisation for the advancement of the science of medicine considered as a pure science.

Prof. Michael Foster gave the introductory address at the School of Pharmacy last week; the subject was "Cramming," and the address will be found reported in full in the *Pharmaceutical Journal* of October 6.

The remains of William Harvey are about to be removed to a new sarcophagus in Hemel Hempstead Church.

An amusing incident is related in our contemporary *L'Électricien*, showing that the knowledge of electrical terminology is yet far from perfect amongst patrons of the latest applications of the science. One of the most eminent and old-established firms who supply incandescent lamps had lately fulfilled an order for a certain number of lamps, specified to be of twenty candle-power at forty-five volts. They received, three days after despatching the goods, the following memorandum:—"We have received your lamps as per invoice, together with the supports, but we were unable to find amongst the goods consigned the forty-five volts invoiced with the lamps . . . "!

As the papers often refer to Chinese telegrams sent to and from Europe in connection with the Franco-Chinese negotiations, it may not be useless to state that a special code of telegraphy has been devised for the use of the Chinese. All the characters of the Chinese language have been numbered, and these numbers are sent by telegraph as secret messages. On arriving in China they are translated into Chinese numbers for the use of Chinese officials.

In carrying out an Act passed by Congress, President Arthur has invited the various countries to send representatives to an International Conference at Washington, the date of which is unfixed, to establish a common prime meridian. The Governments of Austria, Norway, and Sweden have declined, but the two latter approve of the object. Spain is favourable, but has deferred its reply. Belgium is uncertain, but Denmark and Portugal have accepted the invitation conditionally. Switzerland, Venezuela, Mexico, Turkey, Greece, China, Japan, Hawaii, Hayti, Liberia, Holland, Canada, Guatemala, Rou-

mania, Nicaragua, and Honduras have accepted. Replies are expected from Italy, Great Britain, Russia, France, Chili, Brazil, and Germany.

UNIVERSITY COLLEGE, DUNDEE, the munificent gift of Miss Baxter, was duly "inaugurated" on Friday, Prof. Stewart of Cambridge giving an able address on higher education. The college starts with a clear endowment of 100,000*l.*, and a well-selected staff of professors.

THE Photographic Society's Exhibition has been opened at the rooms of the Royal Society of Painters in Water Colours.

WE notice in the *Izvestia* of the Russian Geographical Society an interesting paper by Dr. Woeikof, on the velocity of the wind in Russia. In addition to the important works of MM. Hann and Koeppen, Dr. Woeikof has calculated for fifty Russian and Siberian stations the ratio between the velocity of the wind at 1 p.m. and that in the morning and evening. These calculations have been made in order to show the increase of the force of the wind towards midday and to verify Herr Koeppen's hypothesis as to the dependency of this increase upon the differences of velocities of air in its upper and lower strata, which strata are mixed together by the ascending currents occasioned by the heating of the surface of the soil. The Russian and Siberian stations displaying a great variety of local conditions, M. Woeikof points out the influence of these conditions, but arrives, in their broad features, at the following conclusions:—Throughout Northern and Middle Russia, where the heating of the surface of the soil is very small during the winter, and the ascending current is feeble, the force of the wind increases but slowly as the sun rises above the horizon. The increase is much more during the spring and summer, and at some places the wind at midday blows with a force on an average nearly double what it was in the morning and will be in the evening. In Southern and South-Eastern Russia the increase of the force of the wind during the day is felt even in the winter, owing to the greater heating of the steppes in these lower latitudes. In the Ural region the same increase becomes obvious after February, and the ratio between the forces of the wind at 1 p.m. and at 7 a.m. and 9 p.m. becomes more than 2 to 1 in the summer. In Siberia and Mongolia the relations become more complicated on account of the anticyclones, but the same explanation of the phenomena holds good if the local circumstances be taken into account.

IN his recent work on "Jade and Nephrite Articles in the Dresden Museum," Dr. A. B. Meyer expressed the opinion that there must be other sources of the raw material than those of raw nephrite found in North Germany, Turkestan, New Zealand, and New Caledonia, and of raw jade in Burmah and Montevideo, in order to account for the diffusion of articles wrought from these materials. This view has been so far confirmed that four pieces of raw nephrite of the specific weight of 3·01 have since been found in Suckow, Uckermark, a boulder of the same material in Steiermark, and raw jade in large masses, generally in the form of boulders, in Alaska. He further was of opinion that China could not draw all its nephrite from Turkestan. It had already been shown that the large masses of raw material transported by sea from Burmah to China consisted of jade with the specific weight of nephrite, and Dr. Meyer remarked that by far the largest number of Chinese articles seemed to be of nephrite. Out of the stone hatchets, as they were thought to be, brought by Mr. Anderson from Yunnan, there were but three which had the specific weight of nephrite, and Dr. Meyer conjectured that they were of jade. A piece of the only "indubitable" hatchet out of the three, having been forwarded by Mr. Anderson to Dr. Meyer, was on examination found to be genuine nephrite. The fact is therefore established that genuine nephrite as well as jade exists in the region of Further India, though their exact locality has yet to be discovered.

MESSRS. CROSBY LOCKWOOD AND CO. announce the following new and forthcoming publications:—"British Mining; a Practical Treatise on the Metalliferous Mines and Minerals of the United Kingdom, dealing comprehensively with the theories of Mineral Deposits, the History of Mines, their Practical Working, and the Future Prospects of British Mining Industry," fully illustrated, by Robert Hunt, F.R.S., late Keeper of Mining Records, editor of Ure's "Dictionary of Arts, Manufactures, and Mines, author of "Researches on Light," &c., formerly Professor of Physics, Royal School of Mines; "Earthy and other Minerals and Mining," with numerous illustrations, by D. C. Davies, F.G.S., Mining Engineer, &c., uniform with and forming a companion volume to the same author's "Metalliferous Minerals and Mining"; "Graphic and Analytic Statics in Theory and Comparison, their Practical Application to the Treatment of Stresses in Roofs, Solid Girders, Lattice, Bowstring and Suspension Bridges, Braced Iron Arches and Piers, and other Frameworks, to which is added a chapter on Wind Pressures," by R. Hudson Graham, C.E., containing diagrams and plates to scale, with numerous examples, many taken from existing structures; "A Handbook of the Art of Soap-making, including the Manufacture of Hard and Soft Soaps, Toilet Soaps, Medicated and Special Soaps, Bleaching and Purifying Oils and Fats, Recovery of Glycerine, &c., &c.," with a series of engravings, by Alexander Watt, author of "Electro-Metallurgy Practically Treated," &c.; "The Engineers' and Shipowners' Coal Tables," by Nelson Foley, author of "The Engineer's Office Book of Boiler Construction."

MESSRS. SIEMENS AND HALSKE have brought out an instrument called a torsion galvanometer to be used for large currents. It consists of a magnet suspended between two coils, so as to be affected by both, but to which is attached a torsion spring so arranged that the amount of torsion necessary to bring the needle back to its normal position can easily be determined. These instruments are made in two forms, a vertical and a horizontal form. In the vertical form the needle is suspended by a cocoon silk, and the reading is taken from above; this is the more delicate form. In the horizontal form, which is meant for more practical work, the needle is balanced on knife-edges, and carries at one end a light pointer which passes behind a scale. The amount of torsion required to bring the needle back to zero is indicated by another pointer attached to a handle, and which moves in front of the scale. These instruments can be used either in main circuit or shunt; in the latter case they are often used in conjunction with a resistance box so arranged as to reduce the fall of potential between the terminals of the instrument in a known ratio. It is necessary, however, to use a table of calibrations which are subject to very little change with time.

A COURSE of elementary lectures upon Recent Astronomy and Sidereal Astronomy will be delivered in Gresham College, at six o'clock p.m. on October 16, 17, 18, and 19, by the Rev. Edmund Ledger.

THE President of the Aristotelian Society, Mr. Shadworth H. Hodgson, M.A., LL.D., will open the ensuing session with an address, on Monday evening, October 15, 1883, and the society will then meet fortnightly as usual. The chief work of the session will be a study of Berkeley's "New Theory of Vision" and "Principles of Human Knowledge," and Hume's "Treatise of Human Nature."

THE recent *soirée* of the Chester Society of Natural Science was marked by the publication of a useful programme or descriptive catalogue, which gave to the objects exhibited a teaching value, which may be well imitated, and which forms a permanent reference to those who had the opportunity of being present. The sixty microscopes shown were classified, according to the

subject under the lens, into groups, exemplifying the intimate structure of each of the classes into which animals and plants have been divided, the chief points of structure being briefly described under each head in the "programme," which thus formed a biological text-book of twenty pages with real objects for illustration. This society, founded by Canon Kingsley, is doing exceedingly good work in limiting its operations to the natural history and geology of its own district, scrupulously defined on an ordnance map. The study of local biology is encouraged by the annual grant of 10*l.*, known as the Kingsley Memorial Prize, open to any resident within the Society's district; that of next year is offered for the best collection of "Slides of the Freshwater Algæ of the Society's District, omitting the Diatoms." The Kingsley Memorial Medal this year was awarded to Mr. Shrubsole, F.G.S.

THE report on the progress and condition of the Botanic Garden and Government Plantations in South Australia for 1882, by the Director, Dr. Schomburgk, contains the usual amount of information on the introduction and cultivation of useful and ornamental plants. Dr. Schomburgk draws attention to the small rainfall for the year. He says that during 1881 it amounted to 18 192 inches, but during 1882 it only amounted to 15·742 inches, which was 5·469 inches below the general average (21 inches odd) of the previous forty-three years, the only years during which the rainfall was less than that of last year. During May and June severe frosts prevailed. The temperature was on several nights as low as 29° and 30°. These frosts had, of course, a disastrous effect upon plants in the gardens. "The tropical and subtropical trees and shrubs which had scarcely recovered from the frosts of 1881, especially the tropical *Ficus*, constituted the chief bulk of the sufferers; they have suffered materially, and they have been sadly reduced—from 30 feet and 40 feet in height, to 6 feet and 10 feet." As early as the latter end of September some very hot days were experienced, the thermometer showing 96° in the shade, and 120° in the sun, the highest temperature experienced in any former September. During December and January three slight showers of rain alone fell. Notwithstanding these checks to vegetation a considerable amount of work seems to have been done of a varied character. In the matter of useful plants we quote the following paragraph as an example:—"The demand by invalids for medical herbs becomes more frequent, and it is gratifying to be able to supply them. Inquiries are especially made for the following, viz.: the common English broom (*Cytisus scoparius*), of which a decoction is used in dropsy; the leaves of the mullein or shepherd's club (*Verbascum thapsus*), a decoction of the leaves being recommended by some of the American papers as a remedy against consumption; the globular sponge (*Euphorbia pilulifera*), a native of the tropical regions of the New and Old World. It is found growing in Queensland, and a decoction of the plant is said to be used with the best results in asthmatic complaints."

MR. F. S. MOSELEY, F.Z.S., writes to the *Times* to state that a Marmoset (*Hapale jacchus*) in his possession gave birth to two young ones on the 4th inst.; Mr. Moseley supposes this to be the first case of the kind in Europe.

A TELEGRAM received at Paris on Tuesday night from Algiers states that a strong shock of earthquake was felt at Philippeville at half-past one o'clock that morning. The oscillation was in the direction from north to south. At Jammasses the church and barrack walls were cracked; at Stora a house was also damaged.

A CORRESPONDENT residing at Accra, West Coast of Africa, sends some particulars of the recent earthquake at that place:—"It was at 2.30 a.m. on the morning of Sunday, August 12, that several shocks of earthquake were experienced. The evening previous had been cool, with alternate periods of thick, hot air, which rather presaged a thunderstorm, it being the season of the year when tornadoes pass over the coast. On the night in question it was observed that the surf was particularly violent until half an hour prior to the first shock, when the water seemed to subside and become comparatively calm. The first shock was followed by a second and more violent shock, shaking the foundations. In each case the shock was preceded by an explosion resembling in a great degree the sound usually caused by the discharge of a gun from ships lying in the roadstead. Christiansborg Castle, which in 1863 was wrecked by an earthquake at the same time of year, felt the force of the disturbance severely. Several of the castle walls and those of the neighbouring European houses were found to be cracked the next day. The critical phase lasted, as far as could be calculated, from thirty to forty seconds. During the period—2.30 a.m. to 3.30 a.m.—there was a variation of temperature of 3°—viz. from 71° to 74°, and *vice versâ*. In this interval the wind had completely died away, the atmosphere being hot and almost stifling. It was very difficult to trace the direction of the earthquake, but my own opinion is that it travelled from the south-west, and this is somewhat confirmed by the reports since received from that quarter. Small shocks were repeated at intervals of one hour till seven o'clock in the morning, and on two days since the 12th slight tremors have been felt, but not of sufficient power to do much damage. Since the event the weather has become remarkably cool, considering our proximity to the Equator, the average temperatures being, night and morning, 72°, sun 97°, shade 56°."

THE additions to the Zoological Society's Gardens during the past week include a Bubaline Antelope (*Alcelaphus bubalis* ♀), a Domestic Goat (*Capra hircus*) from Algeria, presented by Mr. Robert Pitcairn; a Black Hornbill (*Buceros atratus*) from West Africa, presented by Mr. J. T. Carrington; two Grey Monitors (*Varanus griseus*) from Arabia, presented by Capt. J. S. Sanderson; four Ural Phrynocephales (*Phrynocephalus helioscopus*) from the east coast of the Caspian, presented by Dr. A. Strauch, F.M.Z.S.; twelve European Tree Frogs (*Hyla arborea*), European, presented by Mr. Carl Schorlemmer; a Cape Hyrax (*Hyrax capensis*) from South Africa, a Great Bustard (*Otis tarda*), European, deposited; an Ocelot (*Felis pardalis*), a King Vulture (*Gypagus papa*), a Brazilian Caracara (*Polyborus brasiliensis*), an Anaconda (*Eunectes murinus*), a Common Boa (*Boa constrictor*) from Brazil, purchased; two Mandarin Ducks (*Æx galericulata*), two Cockateels (*Calopsitta novæ-hollandiæ*), bred in the Gardens.

GEOGRAPHICAL NOTES

A LETTER from Mr. H. M. Stanley, dated July 14 has been published in New York, in which he reports the discovery of a new lake called Mantumba. He has also explored the river marked in the maps as the Ikelembu, but which is really the Malundu, and finds it to be a deep, broad, navigable stream. Stanley expresses his increasing surprise at the density of the population in the equatorial portions of the Congo basin, and says if what he has seen may be taken as representing the state of things generally, there is a population in this river basin of forty-nine millions. Extensive commercial openings are offering themselves.

A TELEGRAM from New York, October 9, states that exploring parties who had just descended the Yukon River, in Alaska, say that they travelled down the stream for two thousand miles. They report the river to be one of the largest in the world, discharging 50 per cent. more water than the Mississippi. Its breadth in some places is seven miles.

THE Austrian African explorer, Dr. Stecker, after five years' absence in the service of the German African Society, has just returned home. For the most part he travelled in company with Herr Gerhard Rohlfs, but Stecker has himself discovered about a dozen countries east and south of Abyssinia, which

before him, no European had ever entered. He was imprisoned as a spy by King Melelek, of Shoa, but was eventually released through the intercession of Marquis Antinori. He has brought back with him numerous valuable maps and a large collection of the fauna, flora, minerals, and other objects connected with the regions he explored.

LIEUT. WISSMANN is preparing to set out on a new expedition to the Upper Congo.

THE United States observing party at Point Barrow have returned to Alaska, *en route* for San Franscisco.

THE French war steamer, which was sent out last year with the French scientific mission to Cape Horn, is daily expected with the party, who have spent their winter in this remote part of the world. These observations have been carried on in connection with the Polar observations as organised by the International Conference, and have been made from August, 1882, to August, 1883.

"THE Yearly Report of the Swiss Alpine Club" for 1882, the eighteenth volume of the series, contains many and various contributions towards a fuller knowledge of the Alps. Besides valuable letterpress we are treated to excellent panoramas after original drawings, coloured views, woodcuts, and cartographical sketches.

IN one of a collection of lectures published at Heidelberg, 1883, by the house of Carl Winter, A. von Lasaulx, the well-known geologist, draws an ingenious parallel between Ireland and Sicily, and attempts to explain the backward state of the inhabitants of these two islands and the disorders of which they have been the theatre by the nature of their geological strata, the formation of their coasts and their positions.

THE last number of the *Izvestia* of the Russian Geographical Society, contains, besides minutes of proceedings, two papers by Dr. Woeikof, on the diurnal period of the velocity of the wind in Russia, and on the distribution of heat in the oceans; a paper by Prof. Lenz, on the periodicity of auroras; the annual reports of the western and eastern Siberian branches of the Society; the end of M. Polyakoff's letters from Sakhalin, wherein he describes his journey on boat down the Tym River and on the eastern coast of Sakhalin; and several notes. We notice among these latter a list of forty-two places in Persia, Attak, and Akhal-Tekke, the positions of which were determined by Capt. Gladysheff.

THE EVOLUTIONARY POSITION[1]

I HAVE been requested by the Subjects Committee of the Congress to place before you a brief statement of some of the advances which have recently been made in natural science, with a view to open a discussion upon their relations, real or supposed, to religious belief. The particular advances which, as I am given to understand, were especially in the minds of the Committee in proposing this question, are those which have resulted in the more or less general adoption by scientific men of the view of the sequence of events which have taken place, and are still taking place, in the universe, to which the term "evolution" is now commonly applied.

All that is embraced by this term, the various realms of nature in which its manifestations are traced, the various shades of meaning attached to it by different persons, would constitute far too large and complex a subject to be treated of in the time to which addresses to this meeting are wisely restricted. I will therefore select for special consideration the only point in the application of the theory upon which I can speak with any practical knowledge; one which is, however, in the eyes of many of very vital interest. It is the one, at all events, which at the present moment attracts most attention; the new ideas upon it being received with enthusiasm by some, and with distrust, if not with abhorrence, by others.

The doctrine of continuity, or of direct relation of an event to some preceding event according to a natural and orderly sequence is now generally recognised in the inorganic world; and although the modern expansion of this doctrine as applied to the living inhabitants of the earth appears to many so startling, and has met with so much opposition, it is, in a more restricted application, a very old and widespread article of scientific as well as of popular faith.

Putting aside, as quite immaterial to the present discussion, the still controverted question of the evidences of the production of the lowest and most rudimentary forms of life from inorganic matter, it may be stated as certain that there is no rational and educated person, whatever his religious beliefs or philosophical views, who is not convinced that every individual animal or plant, sufficiently highly organised to deserve such distinctive appellation, now existing upon the world, has been produced from pre-existing parents by the operation of a series of processes of the order to which the term natural is commonly applied; processes also fundamentally the same throughout the whole range of living beings, however much modified in detail to suit the various manifestations under which those beings are presented to us. We feel absolutely certain, when we see a horse, a bird, a butterfly, or an oak tree, that each was derived from pre-existing parents more or less closely resembling itself. Though we have no direct evidence of the fact in each individual case, the knowledge derived from the combined observations of an overwhelming number of analogous cases is of such a positive character, that we should entirely refuse to credit any one who made the contrary assertion, and should feel satisfied that he had been deluded by some error of observation. We cannot, indeed, conceive of the sudden beginning of any such creatures, either from nothing, from inorganic matter, or even from other animals or plants totally unlike themselves.

To persons whose opportunities of observation of animal and plant life are limited to a comparatively few kinds, existing under comparatively similar circumstances, and which observations moreover only extend over a comparatively limited period of time, it appears that in each kind of animal or plant, such as those just mentioned, individuals of various succeeding generations present a very close resemblance to each other. That they often vary a little cannot escape careful observation, but the deviations from the common characters of the kind to be noticed by persons whose range of vision is thus limited are not striking, and usually appear not to pass beyond certain bounds. Hence arose the common idea, natural enough under such circumstances, but which gradually developed itself, not only into a scientific hypothesis, but even, it would appear, almost into an article of religious belief, that the different kinds or "species," as they are technically called, of animals and plants, had each its separate origin, its fixed limits of variation, and could not under any circumstances become modified or changed into any other form.

This idea became deeply rooted in the human mind, in consequence of the very long period during which it prevailed, the horizon of observation having remained practically stationary from the time man first began to observe and record the phenomena of nature until little more than a century ago, when commenced that sudden expansion of knowledge of the facts of the animal and vegetable world which has been steadily widening ever since. Now it is important to observe that it is strictly *pari passu* with the growth of knowledge of the facts, that the theoretical views of nature have changed, and the older hypothesis of species to which I have referred has gradually given way to a new and different one.

The expansion of the special branches of knowledge affecting our views upon this subject has taken place in many different directions, of which I can here only indicate the most striking.

1. The discovery of enormous numbers of forms of life, the existence of which was entirely unknown a hundred years ago. The increase of knowledge in this respect is something inconceivable to those who have not followed its progress. Not only has the number of well defined species known multiplied prodigiously, but infinite series of gradations between what were formerly supposed to be distinct species are being constantly brought to light. The difficulty of giving any satisfactory definition of what is meant by the term "species" is increasingly felt day by day by practical zoologists, as evidenced by the introduction of such terms as "sub-species," "permanent local variety," &c., into general use, and especially by the wide differences of opinion as to the number or limits of the species included in any given group of animals or plants among naturalists who have made such group their special study.

2. Vast increase in the knowledge of the intimate structure of organic bodies, both as revealed by ordinary dissection and by microscopic examination, a method of investigation only brought to perfection in very recent years. By the knowledge thus acquired has been demonstrated the unity of plan pervading, under diverse modifications, the different members of each

[1] The following address by Prof. Flower, F.R.S., President of the Zoological Society, was given at the recent Church Congress as introductory to a discussion on "Recent Advances in Natural Science in their Relation to the Christian Faith." The address has been revised by the author.

natural group of organisms at one time attributed to "conformity to type," a so-called explanation which explained nothing, but for which a *vera causa* may be found in descent from a common ancestor. Wonderful gradations in the perfection to which different structures have attained in the progress of their adaptation to their respective purposes have also been shown, and of still greater importance and interest, the numerous cases of apparently useless or rudimentary organs in both animals and plants, which were absolutely unaccounted for under the older hypothesis.

3. The comparatively new study of the geographical distribution of living things, which has only become possible since the prosecution of the systematic and scientific explorations of the earth's surface which have distinguished the present century. The results of this branch of inquiry alone have been sufficient to convince many naturalists of the unsoundness of the old view of the distinct origin of species, whether created each in the region of the globe to which it is now confined, or, as many still imagine, all in one spot, from which they have spread themselves unchanged in form, colour, or other essential attributes to their present abodes, however diverse in climate and other environments or conditions of existence.

4. Lastly, though most important of all, must be mentioned the entirely new science of palæontology, opening up worlds of organic life before unknown, also showing infinite gradations of structure, but mainly important as increasing our horizon of observation to an extent not previously dreamt of in the direction of time. Powers of observation formerly limited to the brief space of a few generations are now extended over ages, which the concurrent testimony of various branches of knowledge, of astronomy, cosmogony, and geology, show are immeasurable compared with any periods of which we hitherto had cognisance. We are enabled to trace, and every year, as discovery succeeds discovery, with increasing distinctness, numerous cases of sequences of modification running through groups of animals in successive periods of time, such as the gradual progress in the development and perfection of the antlers of deer, from their entire absence in the earliest known representatives of the type, through the simple conical or bifurcated form, increasing in complexity as time advanced to the magnificent many-branched appendages which adorn the heads of some species of recent stags; such also as the progressive modifications, so often described, beginning in the short-necked, heavy-limbed, many-toed tapir-like animal of the Eocene period, and ending in the graceful, long-necked, light-limbed, single-toed horse of our own age, and numerous others which time will not allow me even to mention.

It would be impossible here to trace the history of the effect of this enormous influx of knowledge upon the doctrine of the separate origin and fixed characters of species; to narrate the scattered efforts of philosophical minds, discontented with the former views, but not yet clearly seeing the light; to describe the slow and struggling growth of the new views, amid difficulties arising from imperfections of knowledge, and the opposition of prejudice, or to apportion to each of those who by their labours have contributed to the final result his exact share in bringing it about. How much, for instance, is due to the work and the writings of our illustrious countryman Darwin? and how much to those who have preceded or followed him? All this forms an episode in the history of the progress of human knowledge which has been abundantly chronicled elsewhere.

The result may, however, be briefly stated to be that the opinion now almost, if not quite, universal among skilled and thoughtful naturalists of all countries, and whatever their beliefs upon other subjects, is that the various forms of life which we see around us, and the existence of which we know from their fossil remains, are the product, not of independent creations, but of descent, with gradual modification from pre-existing forms. In short, the law of the natural descent of individuals, of varieties, races, or breeds (which, being within the limits of the previous powers of observation, was already universally admitted) has been extended to the still greater modifications constituting what we call species, and consequently to the higher groups called genera, families, and orders. The barrier fancied to exist between so-called varieties and so-called species has broken down.

Any one commencing the study of the subject at the present time without prejudice, and carefully investigating the evidence upon which to form his conclusions, bearing in mind that he must look for his proofs, not so much in direct experiments or absolute demonstrations, which from the nature of the case are impossible, but in the convergence of the indications furnished by the interpretations of multitudinous facts of most diverse kinds, must find it extremely difficult to place himself in the position of those who held the older view, so much more reasonable, so much more in accordance with all that we know of the general phenomena of nature, does this new one seem. In fact the *onus probandi* now appears entirely to lie with those who make the assertion that species have been separately created. Where, it may be asked, is the shadow of a scientific proof that the first individual of any species has come into being without pre-existing parents? Has any competent observer at any time witnessed such an occurrence? The apparent advent of a new species in geological history, a common event enough, has certainly been cited as such. As well might the presence of a horse in a field, with no sign of other animals of the same kind near it, be quoted as evidence of the fallacy of the common view of the descent of individuals. Ordinary observation tells us of the numerous causes which may have isolated that horse from its parents and kindred. Geologists know equally well how slight the chances of more than a stray individual or fragment of an individual here and there being first preserved and afterwards discovered to give any indication of the existence of the race. Those who object to the new view complain sometimes of the frequency with which its advocates take refuge, as they call it, in the "imperfection of the geological record." I think, on the contrary, the difficulty is always to allow sufficiently for this imperfection. When we contrast the present knowledge of palæontology with what it was fifty or even ten years ago; when we see by what mere accident, as it were, a railway driven through a new country, a quarry worked for commercial purposes, a city newly fortified, all the most important discoveries of extinct animals have been made, we must be convinced that all arguments drawn from the absence of the required links are utterly valueless. The study of palæontology is as yet in its merest infancy; the wonder is that it has already furnished so much, not so little, corroboration of the doctrine of transmutation of species.

Direct proof is, then, equally absent from both theories. For the old view it may be said that it has been held for a very long time by persons whose knowledge of the facts of nature which bear upon it was extremely limited. On the other hand, the new view is continually receiving more support as that knowledge increases, and furnishes a key to a vast number of otherwise inexplicable facts in every branch of natural history, in geological and geographical distribution, in the habits of animals, in their development and growth, and especially in their structure. Allow me to take one instance from the last named—the anatomy of the whale. How is it possible, upon any other supposition than that it is the descendant of some land animal, with completely developed limbs and teeth, which has become gradually modified to suit an aquatic mode of existence, to explain the presence of the numerous rudimentary, and to their present possessors absolutely useless, structures found in its body. Amongst others, a complete set of teeth, existing only in embryonic life, entirely disappearing even before birth, and rudimentary hind legs, with their various bones, joints, and muscles, of which no trace is seen externally. It may be asserted that the whale was originally created so, as it was asserted, and long maintained, that fossil shells and bones were originally created as such in the rocks in which they are found. It took more than two centuries of continuous and most acrimonious discussion to convince the world, especially the theological world, that these were the actual remains of animals which had once lived in a former period of the earth's history. Their evidence is now, however, universally admitted as supplying knowledge of the changed conditions of the surface of the earth, and with equal clearness do these rudimentary organs, hidden in the secret recesses of the whale's body, furnish, to those who inquire, indications that the animal has passed through phases of existence unlike those in which we now see it.

I do not for a moment assert that the new view explains everything that we students of nature are longing to know, or that we do not everywhere meet with obscure problems and perplexing difficulties, facts that we cannot account for, and breaks in the chain of evidence. As to the details and mode of operation of the secondary laws by which variation and modification have been brought about, we are far from being in accord. Happy for us that it is so, or our work would be at an end. I only maintain that the transmutation view removes more difficulties, requires fewer assumptions, and presents so much more consistency with observed facts than that which it seeks to supersede, and is, therefore, so generally accepted, that there is no

more probability of its being abandoned, and the old doctrine of the fixity of species revived, than that we should revert to the old astronomical theories which placed the earth in the centre of the universe, and limited the date of its creation to six ordinary days.

The question of the fixity or the transmutation of species is a purely scientific one, only to be discussed and decided on scientific grounds. To the naturalist, it is clearly one of extreme importance, as it gives him for the first time a key to the interpretation of the phenomena with which he has to deal. It may seem to many that a question like this is entirely beside the business of a Church Congress, as it is one with which only those expert in the ways of scientific investigation, and deeply imbued with knowledge of scientific facts, could be called upon to deal. This would certainly have been my view, if it had not been that some who, from their capacities and education, should have been onlookers in such a controversy, awaiting the issues of the conflict while the lists are being fought out by the trained knights, have rushed into the fray, and by their unskilful interposition have only confused the issues, casting about dust instead of light. In the hope of clearing away some of this dust the present discussion has been decided upon.

It is self-evident that a solid advance of any branch of knowledge must, in some way or other, and to a greater or less degree, influence many others, even those not directly connected with it, and therefore the rapid simultaneous strides of so many branches of knowledge as may be embraced under the term of "Recent Advances in Natural Science," will be very likely to have some bearing upon theological beliefs. Whether in the direction of expanding, improving, purifying, elevating, or in the direction of contracting, hardening, or destroying, depends not upon those engaged in contributing to the advance of science, but upon those whose special duty it is to show the bearing of these advances upon hitherto received theological dogmas. The scientific questions themselves may well be left to experts. If the new doctrines are not true, there are plenty of keen critics among men of science ready to sift the sound from the unsound. Error in scientific subjects has its day, but it is certain not long to survive the ordeal, yearly increasing in severity, to which it is subjected by those devoted to its cultivation. On the other hand, the advances of truth, though they may be retarded, will never be stopped by the opposition of those who are incompetent by the nature of their education to deal with the evidence on which it rests. There is no position so fraught with danger to religion as that which binds it up essentially with this or that scientific doctrine, with which it must either stand or fall. The history of the reception of the greatest discoveries in astronomy and geology, the passionate clinging to the exploded pseudo-scientific views on those subjects supposed to be bound up with religious faith, the fierce denunciations of the advocates of the then new, but now universally accepted, ideas, are well-worn subjects, and would not be alluded to but for the repetition, almost literal repetion in some cases, of that reception which has been accorded to the new views of biology.

Ought not the history of those discoveries and the controversies to which they gave rise to be both a warning and an encouragement? Those who hoped and those who feared that faith would be destroyed by them have been equally mistaken; and is not probable that the same result will follow the great biological discoveries and controversies of the present day?

In stating thus briefly what is the issue of these discoveries, as generally understood and accepted by men of science, I have done all that I promised, and must leave in far more competent hands the part of the subject especially appropriate for discussion at this meeting. I may, however, perhaps be allowed to put a few plain and simple considerations before you, which may have some bearing upon the subject, and which have no pretensions to novelty, though, being often lost sight of, their repetition may do no harm.

I said at the commencement of this paper that it has long been admitted by all educated persons, whatever their religious faith may be, that that very universal but still most wonderful process, the commencement and gradual development of a new individual of whatever living form, whether plant, animal, or man, takes place according to definite and regularly acting laws, without miraculous interposition. Further than this, I believe that every one will admit that the production of the various races or breeds of domestic animals is brought about by similar means. We do not think it necessary to call in any special intervention of creative power to produce a short-horned race of cattle, or to account for the difference between a bulldog and a greyhound, a Dorking and a Cochin China fowl. The gradual modifications by which these races were produced, having taken place under our own eyes as it were, we are satisfied that they are the consequence of what we call natural laws, modified and directed in these particular cases by man's agency. We have even gone further, having long admitted, without the slightest fear of producing a collision with religious faith, that variation has taken place among animals in a wild state, producing local races of more or less stable and permanent character, and brought about by the influence of food, climate, and other surrounding circumstances.

The evidences of the Divine government of the world, and of the Christian faith, have been sufficient for us, notwithstanding our knowledge that the individual was created according to law, and that the race or variety was also created according to law. In what way then can they be affected by the knowledge that the somewhat greater modifications, which we call species, were also created according to law? The difficulties, which to some minds seem insuperable, remain exactly as they were; the proofs, which to others are so convincing, are entirely unaffected by this widening of scientific knowledge.

Even to what is to many the supreme difficulty of all, the origin of man, the same considerations are applicable. Believe everything you will about man in his highest intellectual and moral development, about the nature, origin, existence, and destiny of the human soul—you have long been able to reconcile all this with the knowledge of his individual material origin according to law, in no whit different in principle from that of the beasts of the field, passing through all the phases they go through, and existing long before possessing, except potentially, any of the special attributes of humanity. At what exact period and by what means the great transformation takes place no one can tell. If the most Godlike of men have passed through the stages which physiologists recognise in human development without prejudice to the noblest, highest, most divine part of their nature, why should not the race of mankind, as a whole, have had a similar origin, followed by similar progress and development, equally without prejudice to its present condition and future destiny? Can it be of real consequence at the present time, either to our faith or our practice, whether the first man had such an extremely lowly beginning as the dust of the earth, in the literal sense of the words, or whether he was formed through the intervention of various progressive stages of animal life?

The reign of order and law in the government of the world has been so far admitted that all these questions have really become questions of a little more or a little less order and law. Science may well be left to work out the details as it may. It has thrown some light, little enough at present, but ever increasing, and for which we should all be thankful, upon the processes or methods by which the world in which we dwell has been brought into its present condition. The wonder and mystery of creation remains as wonderful and mysterious as before. Of the origin of the whole, science tells us nothing. It is still as impossible as ever to conceive that such a world, governed by laws, the operations of which have led to such mighty results, and are attended by such future promise, could have originated without the intervention of some power external to itself. If the succession of small miracles, formerly supposed to regulate the operations of nature, no longer satisfies us, have we not substituted for them one of immeasurable greatness and grandeur?

A GREEN SUN IN INDIA

WE have received the following communications on this phenomenon. At the same time we may refer to a passage in one of Mr. Norman Lockyer's papers on "Physical Science for Artists," in which he speaks of the marked effects of aqueous vapour in the atmosphere on the character of the sun's light. He states that he asked Dr. Schuster to test his theory while in India. "Theory," he states, "had led me to expect that with the enormous thickness of air available there, absorption at the red end of the spectrum by aqueous vapour would be seen as well as the absorption at the blue, which is so common with us. Seeing the sun a vivid green through the steam of the little paddle-boat on Windermere first led me to inquire into the possibility of aqueous vapour following the same law as that which I think we may now accept in the cases of the vapours of metals. As in these experiments with vapours absorption of the red end alone was seen, as well as absorption at the blue end alone, the

assumption that these two absorptions existed in aqueous vapour at once accounted for the green sun." In the sequel it will be found that Dr. Schuster's observations quite confirmed Mr. Lockyer's theory.

By my friend Major A. T. Fraser, R.E., I have just been favoured with a copy, which I now inclose to you, of a Madras paper, dated September 12, giving not only the frightened comments of a dozen different, unpractised observers on the *green* sun, seen morning and evening over the south-east of India during the two or three previous days, but also the spectroscopic explanation thereof by Rev. Prof. Michie Smith, which is so good that you may perhaps think well to introduce it into your columns.

The case too is further worth notice here, as an example of the occasional powers of the rain-band spectroscope over and above the wet and dry bulb thermometers, to tell us what is in the upper air now and will visit us soon below. For on those days when the greenness of the sun was undoubtedly due to being seen through strata of atmosphere inordinately charged with watery vapour (much as I set forth in vol. xiv. of "Edinburgh Astronomical Observations," was the case so eminently at Palermo in 1872), the air in contact with the dwellings of man was dry.

It has been so too for a considerable time, as testified by Mr. Pogson at the Madras Observatory, both by his daily hygrometric observations, and by his record of rainfall for the year being behind the usual quantity by about a third of the whole. From this circumstance apparently, "some of the learned old men," but knowing nothing of spectroscopy, in that locality, needlessly afflicted themselves and their neighbours also, by proclaiming that the *green* sun, in place of being a sign of good times coming, "was a bad omen for the country, and would bring a famine this year."

But though there was still a drought at Madras, the same paper involuntarily reports that abundant rain had begun to fall further away to the south and west, or to the extent, at Travancore, of 5·91 inches, and at Malabar of 10·14 inches, in one week.

C. Piazzi Smyth

15, Royal Terrace, Edinburgh, October 9

We have just been having the curious phenomenon of a greenish colour in the light of the sun. Letters to the Madras papers show that the same thing has been noticed in many other parts of Southern India. It is new to me, and to every one else who has seen it, so far as I have heard. The native astronomers say that there is a planet wholly absorbed in the sun, and that if it leaves the sun a green light will appear. Or, according to others, if Venus comes in contact with the sun, which, according to their calendars, is the case now, a green light will be seen. Both agree, however, in portending evil to the inhabitants of this planet; consequently there has been no little curiosity and speculation awakened by this singular appearance, and more or less of uneasiness in the minds of the ignorant and superstitious natives. As I am principal of the High School in this place, and teacher in a small way of science, they apply to me for the scientific solution of the mystery; but, farther than a mere conjecture, I have to confess myself quite as much in the dark as the rest.

Doubtless others of your correspondents have noticed the same thing, and my description may be superfluous; yet I venture to give it, as it may at least corroborate the statements of other observers.

My attention was first called to the matter by one of my teachers about four o'clock on the afternoon of September 10, but I learn that the same thing was noticed elsewhere the day before. At the time of which I speak I noticed that the light from the sun shining into the room through an open western door threw a curious pale blue colour on the floor. I also noticed that it had something of the effect upon colours that is commonly seen in coloured lights. On looking out I saw that the sun, which was somewhat dimmed by a haze, had a decidedly greenish-blue tinge. The same thing was observed on the 11th and 12th, both morning and evening; but my observations were confined to the evenings. About four o'clock (at least I did not notice it earlier) an indistinct bluish tinge appeared in the light. This gradually passed into a greenish colour, and this in turn became tinged with yellow as the sun approached the horizon. As the sun sank, bands of smoky haze drifted across its disk. After the sun was down, bright yellow, orange, and red appeared in the west, a very deep red remaining for more than an hour after sunset; whereas under ordinary conditions all traces of colour leave the sky in this latitude within half an hour after the sun disappears. At night the moon, just past the first quarter was surrounded by a pale greenish halo some thirty degrees in breadth.

After sunset I observed a peculiar appearance in the haze which covered the sky. It was not of sufficient density to be at all visible, except where it reflected the direct rays of the sun. There it had a singular mottled appearance, with a smoky look along the borders of the denser portions, suggesting clouds of smoke or dust in the upper regions of the atmosphere.

Of course the question which every one is asking now is, What caused the green light? very few, so far as I have learned, having noticed anything else. The succession of colours which I have mentioned, occurring exactly in the order of the solar spectrum, would seem to indicate beyond a doubt the presence of some highly refracting substance in the atmosphere which resolved the sun's rays into primary colours and gave us in succession, according to the angle of the sun to the horizon, blue, green, yellow, orange, and red, the two latter only appearing as reflected from the under surface of the haze. On the evening of the 13th the sun appeared to be perfectly clear, but after it was below the horizon the western sky was seen to be covered with a smoky haze of a singular appearance, which became brilliantly illuminated with yellow, orange, and red in the order I have mentioned, counting upward from the horizon. These sank one after the other, leaving at last an arc of brilliant red along the west, the inner portion of the segment contained by the arc being composed of orange. This disappeared in turn, and the whole western sky became yellow again without any distinct outlines, and this gradually deepened into red, which remained for an hour or more after sunset. The latter phenomenon was not unlike an ordinary sunset, except in brightness and duration.

But what could the refracting medium be? The air itself has refracting power, and so have the minute particles of moisture or ice which constitute the ordinary hazy clouds in the upper regions of the atmosphere; but it would be difficult to say why the effects which I have described should not be of more frequent occurrence if produced by either or both of these causes, since they are always present in a greater or less degree, and especially as there was apparently nothing unusual in the state of the weather at the time. Neither would clouds of smoke alone produce such effects as I have mentioned. In my native place in the United States, where vast tracts of prairie, and often of woodland, are burned over every autumn, a smoky haze is a common thing at that season of the year. Sometimes for days together the haze is so dense as almost to hide the sun, yet I never heard of any other effect upon the light than this obscuring of it, while the sunsets were especially dull and colourless. Nor do I see how these phenomena could be referred to unusual electrical conditions, for in that case they should be more common, as electrical storms are by no means unusual. I have been led, therefore, to wonder if the phenomena may not be the result of some fine transparent or semi-transparent dust particles in the upper regions of the atmosphere, which form the singular looking haze, and refract the light in the manner I have described, and whether the whole may not be traceable to the recent volcanic eruptions in Java.

According to the telegrams, the city of Batavia was darkened for thirty-six hours by clouds of dust from the volcano. True, we are a long distance from Java here, Ongole being in latitude 15° 30′ N., and longitude 80° 6′ E. But it is well known that the smoke and ashes from volcanoes are often carried to immense distances, and in this case, if acted upon by the trade winds, they would be carried away to the westward, and rising with the upward currents might enter the return trades, and so be swept over Southern India in a north easterly direction—the direction of our prevailing winds at this season of the year. However, I only venture this solution of the problem as a timid conjecture, the truth or falsity of which I have no means of demonstrating.

Will not some of the readers of Nature who are better informed in regard to such matters kindly give me their opinions of the phenomena? I should be glad also to know if such effects upon the light as I have described have ever been noticed as the result of the smoke or dust or gases emitted by volcanoes.

Another thing which awakened about as much curiosity and speculation in the minds of the natives as the coloured light was the large spot now crossing the sun's disk. Ordinarily the atmosphere is so clear that even at sunset the sun cannot be

viewed with the naked eye; but, being obscured as it was by the haze, the spot was distinctly visible to all.

Ongole, India, September 14 W. R. MANLEY

[Mr. Manley sends us several letters from the *Madras Mail* on the phenomenon; from these we give the following extract, dated September 10:—]

A. T. M. writes:—"Unlike his usual custom the sun rose this morning clothed as it were in bright blue colour, rendering the whole horizon and all beneath it of the same hue. The pure, colourless river water looked as if it was just let out of an indigo vat. Even the green fields with grass and trees about looked blue. Our whitewashed house also had a temporary change of colour. This phenomenon lasted from 6 to 10 a.m."

The same phenomenon seems to have been observed in Trinidad on the afternoon of Sunday, September 2. Mr. J. Arnold, writing to the *Times*, gives the following extract from a correspondent's letter from Port of Spain:—

"We have been having very curious weather; last Sunday, about five o'clock, the sun looked like a blue globe, and with the aid of a small telescope I saw plainly three dark spots on it. After dark we thought there was a fire in the town from the bright redness of the heavens." Mr. Arnold adds: "All my correspondents agree as to the blue colour, and several seem to have noticed the spots. This occurrence, which was heid to foretell bad weather, took place three days before the cyclone that swept Martinique."

THE JAVA ERUPTION

THE following details concerning this catastrophe have been sent by Lloyd's agents at Batavia, under date of Sept. 1:—

"The past week is memorable as having witnessed one of the most disastrous and severe volcanic eruptions ever known in the Malay Archipelago. Krakatoa has again been the origin of the disturbance. On Sunday last, about 4 p.m., a series of detonations were heard proceeding apparently from the south-west. Towards night these grew louder, till in the early morning the reports and concussions were simply deafening, not to say alarming. When day broke the atmosphere to the west had a sulphurous and lurid appearance, and a thin layer of fine white ash covered the ground. Towards 9 a.m. the reports died away, but about an hour later dark clouds quite obscured the sky and the sun. A heavy rain of ashes, sulphur, and dust commenced to fall, and at 11 a.m. this town was in pitch darkness and business totally suspended. About twelve o'clock (midday) a large wave about seventeen feet in height swept in from the sea, causing many prows and small craft to be driven ashore, but doing but little damage to the shipping in harbour. This being the dry monsoon, the rivers are low at present. The wave, however, drove an immense volume of water up our rivers, which suddenly rose so high that the banks at the river mouth were flooded and many small crafts stranded. Happily the wave subsided again suddenly, leaving the rivers almost dry, and about one o'clock the rain of ashes subsided and the atmosphere grew lighter. Shortly after 2 p.m., however, another wave, larger than the first, came rolling in from the sea. A few native fishermen were drowned by this wave, and two Europeans at Onrust also lost their lives. At Tandjong Priok the *Princess Wilhelmina* was within an ace of stranding, while some small crafts and prows were cast high up on land. No further damage, however, occurred in Batavia. The eruption, however, so far as we can learn, has had most fatal and disastrous effects all along the south-west coast of Java, and also on the south coast of Sumatra. We shall not probably be in possession of full particulars for some days yet, as telegraph lines are damaged and roads destroyed, but so far we can give the following particulars. The Island of Krakatoa, the summit of which peak was 2600 feet above water level, has totally disappeared below the sea, and the neighbouring Island of Dwaisindeweg is split in five parts. Sixteen new volcanic islands have been formed between Krakatoa and Sibesie, and the sea bottom in the Straits of Sunda has completely changed. In fact the Admiral Commanding-in-Chief has issued a circular stating that till new soundings have been taken the navigation of the Straits of Sunda is likely to be extremely dangerous. Anjer and lighthouse and the other lights of south-west Java have all been destroyed. The subsidences and upheavals we have alluded to caused a large wave about 100 feet in height to sweep down on the south-west coast of Java and south of Sumatra. This wave swept inland for a great distance, thereby doing great injury both to life and property. We are here only twelve miles away from one of the points on which the wave spent its fury. The whole coastline to the south-west has changed its configuration. The inhabitants of the Island of Onrust were only saved from the flood which swept over the island by taking refuge on board two steamers. At Merak Government establishment the inhabitants took refuge on the knoll, 50 feet high, but were all swept off and drowned, with the exception of one European and two Malays, who were saved. Mauk and Kramat, west side of Batavia Roads, have been laid waste, and about 300 lives lost. In Tjeringin only one house has been left standing. Both the native and European officials have perished. A rain of mud also fell at the above place, which is situated opposite to where Krakatoa Island once lay. Anjer seems to have been completely destroyed. Lloyd's subagent there wires from Serang: 'All gone. Plenty lives lost.' The dry dock at Amsterdam Island was carried away by the waves, but has since been found stranded in Middleberg Island. The Padang steamer, which left here on Sunday, returned next day to Anjer, only to find the place in ruins. The captain reports that his vessel was in great danger, owing to the eruption from Krakatoa. On his arrival at Telok Betong, his first port of call, the place was found completely destroyed. We understand that it has been submerged, but are not yet in possession of full particulars. We hear that on Monday the whole of West Java, as far as Banding, was shrouded in darkness and covered by ash rains. A telegram just in informs us that the explosions from Krakatoa were heard at Deli (Sumatra), which place is opposite Penang. The Government here, we understand, in the interest of shipping, are sending out steamers to cruise at either end of the Straits of Sunda, to warn vessels to observe caution while passing the Straits, as charts are no longer reliable. According to latest telegram from Serang we learn that in the residence of Tjeringin alone 10,000 lives were calculated to be lost. The Padang steamer just in reports that it is impossible to approach to the place where Telok Betong once was situated, owing to the sea being filled with pumice stone and mud. In some parts of Sumatra Straits the pumice stone is seven to eight feet deep."

THE BRITISH ASSOCIATION

SECTION C—GEOLOGY

On some Fossil Fish Remains found in the Upper Beds of the Yoredale Series at Leyburn, in Yorkshire, by James W. Davis, F.G.S.—The red limestone forms the upper part of the main limestone of Phillips, being separated from it by only one foot of shale or plate. It is about 100 feet below the millstone grits, the intermediate beds being composed of grits and shales with one bed of limestone about 16 or 18 feet thick. A peculiar aggregation of fish remains has been discovered in the red beds by Mr. Wm. Horne of Leyburn. They comprise nearly forty species, the majority of which are peculiar to the beds; others like *Cladodus* and *Petalodus* are common to the Mountain Limestone, and do not appear to differ either in size or otherwise from those of the lower massive limestone. The representatives of the genera *Psamodus*, *Cochliodus*, and *Polyrhizodus*, which are found abundantly in the lower limestone, and are of great size and importance, are in this locality comparatively small and rare, and appear to indicate that the fishes they represent were gradually becoming extinct. Their representatives are not known to occur in the superimposed Millstone Grits either in this locality or any other. There are in addition species of *Megalichthys* and *Pleurodus*, which are characteristic of the coal measures. The presence of so varied a fauna naturally leads to the inference that the circumstances under which they existed were not those usually characteristic of the aggregation of limestones, but rather indicate a shallow or shore deposit with occasional influxes of fresh water. *Megalichthys* and *Pleurodus* are fishes which in the coal measures probably lived in fresh or brackish water; and though they may have been adapted to exist in marine conditions, the occurrence of beds of sand and shale intercalated with the thin limestones of the Yoredales evidently shows the proximity of land, and it is probable that they were carried to their present position by rivers, and there deposited with the marine forms with which they are associated. The supposition that the water was brackish may account for the small size of some of the genera already mentioned and their final extinction in the grits and shales which succeed the limestone The great fishes whose remains are found in the lower lime

stone, represented by *Ctenacanthus*, *Orthacanthus*, and others, are absent, the only species hitherto found being those of the curious *Cladacanthus* and *Physonemus*.

On the Occurrence of the Remains of Labyrinthodonts in the Yoredale Rocks of Wensleydale, by James W. Davis, F.S.A., F.G.S.—Some bones of the leg of a Labyrinthodont were discovered by Mr. Horne and described by Prof. L. C. Miall in the *Quarterly Journal of the Geological Society*, vol. xxx. p. 775. They were found in a dark-coloured flagrock above the Harmby Quarry, which also extends with an easterly dip to the Harmby railway cutting. The same flagrock is also found behind Leyburn and the Shawl, and in that locality it has been extensively quarried. In addition to the leg-bones already mentioned, others have been found in the same flagrock, but separated by considerable distances, so that it is not probable that they belonged to the same Labyrinthodont. In the railway cutting a portion of a cranium was found. It is 1·9 inch in length and 1·4 in breadth. A number of sutures, not very well defined, seem to indicate that the bone constituted the back part of the skull. The third specimen was found in the quarries beyond the Shawl north-west of Leyburn, and exhibits casts of the jaws of another Labyrinthodont. Each ramus is about three inches in length; they have been disturbed and displaced. The external surface of the jaws was ornamented with a reticulated arrangement of tubercles, an impression of which is preserved in the specimen. Along the margin of the impression of the alveolar portion of one of the rami there is a series of impressions which appear to have been caused by small pointed teeth.

Section across the Trias recently exposed by a Railway Excavation in Liverpool, by G. H. Morton, F.G.S.—During the last eight years a very important section of the Triassic strata has been exposed in Liverpool, by excavations for widening the line of the London and North-Western Railway Company. The section presents a solid mass of sandstone on both sides of the new railway cutting from Lime Street Station to Edge Hill Station, a distance of 2300 yards from east to west. The height of the rock on each side varies. The strata exposed belong to the Keuper and Bunter formations. The Pebble Beds of the Bunter crop out for 914 yards along the east of the cutting, but do not contain any marl partings, and not a single pebble of any kind has been noticed. Only two faults occur along the whole length of the Pebble Beds exposed, and they are of very little importance. The subdivision ends at Smithdown Lane, where there is a fault with a downthrow to the west, which brings in the upper mottled sandstone, the highest member of the Bunter formation, where it is not represented on the map of the Geological Survey, or the fault recorded. The Upper Mottled is a fine-grained, soft, bright red sandstone with grey streaks, and as it readily crumbles into sand is never hard enough for building purposes. It crops out to the west from Smithdown Lane to University College, when a fault throws down the strata about 600 feet and brings in the Keuper sandstone, which is 400 feet thick, and interstratified with thin beds of marl. The highest beds of the Keuper are at the College; lower strata containing the beds of marl crop out from beneath, and are thrown down to the west by faults three times in succession, when the basement beds crop up in Lime Street Station. The section shows that all the faults throw down the strata to the west and bring in higher beds in that direction. It also shows the exact position of the fault between the Bunter and Keuper formations, which was not known before. The position of the Keuper, as a wedge-shaped mass of sandstone, with the Bunter formation faulted against it on the east and west, is of great local interest, and it is easy to understand how the succession of the strata has not been satisfactorily explained before in the absence of any such a continuous section as that described. The remarkable absence of faults in the pebble-beds has an important bearing on the construction of the Mersey Tunnel, which will have to be carried through these beds along its entire length. The section shows that while faults are numerous in the Keuper sandstone, which was frequently fractured during subsidence into a depression, the pebble-beds are very little faulted. A few days ago, when under the Mersey, I did not find a single fault either in the tunnel or in the heading beneath.

Recent Opinions on the Loess Deposits of the Valley of the Rhine as Evidence of a "Great Post-Glacial Flood," by Mark Stirrup, F.G.S., adversely criticises recent opinions of Mr. H. H. Howorth, F.S.A., as to the mammoth in several of the superficial deposits proving a "great Post-Glacial flood." The facts connected with the loess deposits of the Rhine Valley are not consistent with the interpretation given to them by Mr. Howorth, nor is the assumption that the materials of the loess were derived from volcanic mud-streams borne out by the evidence. The author considers Mr. Howorth has failed to prove his postulate that not only the extinction of the mammoth but the existence of several superficial or Post-Glacial deposits were due "to a sudden catastrophe involving a great diluvial movement which extended over the larger part of the northern hemisphere, and accompanied by an equally sudden and violent change of climate," and the author considers the whole of the evidence adduced by Mr. Howorth as unsound and inconclusive. He regards Mr. Howorth's attempt to resuscitate some of the obsolete doctrines of Cuvier and Buckland as a retrograde movement in the history of geology.

Master Divisions of the Tertiary Period, by Prof. Boyd Dawkins.—The classification of the Tertiary rocks sketched out some fifty years ago and since then altered in no important degree is out of harmony with our present knowledge, and the definitions of the series of events which took place in it has been greatly modified by the process of discovery in various parts of the world. The terms Eocene, Miocene, and Pliocene no longer express the idea of percentages of living species of fossil mollusca upon which they were founded, and Post-Tertiary, Quaternary, and Recent are founded on the assumed existence of a great break comparable to that separating the Secondary from the Primary or Tertiary periods which is now known not to exist. The author proposed a classification of the Tertiary period in Europe, by an appeal to the land mammalia, and since that time his definition has been found to apply equally well to the Tertiaries of Asia and the Americas and to the late Tertiaries of Australia. He stated that the forms of life in the rocks have changed at a very variable rate, and in direct proportion to their complexity of organisation, the lower and simpler having an enormous range, while the higher and more complex have a much narrower range and are more easily affected by the change in their environment.

On a Boulder from the Chloritic Marl of Ashwell, Herts, by H. G. Fordham.—Boulders found in these marls, in the so-called coprolite workings in Cambridgeshire and the neighbouring counties, are usually little more than pebbles. The boulder now described measures 12 × 9½ × 5½ inches. It is somewhat triangular in general form, and is much rounded and worn. The material, according to Prof. Bonney, is a quartz-felsite. The author attributes its origin to its being brought to its present position by floating ice.

Preliminary Note on the Further Discovery of Vertebrate Footprints in the Penrith Sandstone, by G. V. Smith.—The specimens were obtained from a quarry opened out by the Settle and Carlisle Railway, situated on the slope of the hill, north of the highway from Penrith to Alston, and about three and a half miles from Penrith, the sandstone is strongly current bedded, and is largely used for building purposes; these sandstones are older than the magnesian limestone. The impressions indicate several distinct forms of vertebrates.

On a Supposed Case of Metamorphism in an Alpine Rock of Carboniferous Age, by Prof. T. G. Bonney, M.A., F.R.S.—At the base of the Carboniferous series in some parts of the Western Alps is a conglomerate called the *Poudingue de Val Orsine*, the matrix of which abounds in mica, and is supposed by some geologists to exhibit true foliation. In the Alps there is always an abrupt transition from the comparatively unmetamorphosed rocks of known geological age to the true schists and gneisses of unknown but certainly far greater antiquity, and nothing short of the clearest proof would justify us in considering any of these crystallised foliated rocks as altered Devonian or Silurian, even though the latter term be used in its most extended sense.

On the Geological Age of the North Atlantic Ocean, by Prof. Edward Hull, LL.D., F.R.S., &c., Director of the Geological Survey of Ireland.—In this paper the author made use of three leading formations as factors in his inquiry, viz. the Archæan (or Laurentian), the Silurian (chiefly the Lower Silurian), and the Carboniferous. He considers that throughout the Archæan, or Laurentian, the Lower Silurian, and the Carboniferous epochs, the regions of North America, on the one hand, and of the British Isles and Western Europe were submerged, while a large part of the North Atlantic area existed as dry land, from the waste of which these great formations had been built up; and he urged that if such were the case, the doctrine of the permanency of oceans and continents, as tested by the case of the North Atlantic, falls to the ground.

Dyas versus Permian, by Rev. A. Irving, B.Sc., B.A.,

F.G.S.—This subject is brought forward for discussion both as having a special local interest, and on account of the international importance of the subject in view of the Berlin Congress next year, and the progress of the geological map of Europe. The author, referring to previous papers in the *Geological Magazine* during the year 1882, in which strong reasons were given for abandoning the threefold division of the so-called Permian system, and to the discussions raised in the same periodical, maintains that the "Permian system" of Murchison, which represents the group of strata as marked by three stages, is inapplicable to the English rocks of Post-Carboniferous age. The term "Permian" has only a local and subordinate value, and scarcely applies even to the whole Russian area in which these strata are developed. He considers that the application of the "Permian system," as propounded by Murchison, to the Post-Carboniferous rocks of Central Europe is no longer tenable, any more than is its application to the British series, as the author has shown elsewhere.

On the Coloration of some Sands, and the Cementation of Siliceous Sandstones. By the Rev. A. Irving, B.A.—In the first part of this paper attention is drawn to the occurrence of certain green-coloured sands which are frequently met with below the peaty layers, at the heads of the small valleys, in the Upper Bagshot sands. The local and exceptional nature of these green deposits, and their relation to the decomposing vegetal matter which has overlain them for a long period of time, suggest the connection of the green colour with the decomposition of vegetation. Chemical analysis of these sands shows that the green colour is in no way connected with any of the ordinary green minerals which enter into the formation of rocks, but reveals the *organic* origin of the colouring matter. In the second part of the paper attention is drawn to some recent investigations by the author of the origin of the siliceous cementing material of the sarsen stones.

Note on the Nagel Flue of the Rigi and Rossberg. By Prof. T. G. Bonney, M.A., F.R.S.—The author called attention to the following points in regard to the conglomerate of these mountains:—(1) That the pebbles were not seldom indented by mutual pressure; (2) that the pebbles in this district consisted mainly of grits and limestones from the Secondary and perhaps early Tertiary series of the Alps, with a variable amount of a reddish granite (of whose locality he was ignorant). He considers there was a close analogy between the Bunter conglomerate and the nagel flue, the former also resembling the British Old Red Sandstone, and a part of the Calciferous sandstone series in Scotland. As these three were admittedly freshwater deposits, he argued that the Bunter series (the parts of which had some resemblance to the ordinary molasse) should be reckoned among the true fluviatile or fluvio-lacustrine deposits.

Notes on Geological Sections within Forty Miles' Radius of Southport. By C. E. De Rance, F.G.S.—The sections in Silurian works of the Lake District and North Wales within the radius are described, also those in the Carboniferous limestone, coal measures, Permian, and the Triassic rocks, especially the Keuper sandstones and worls around Southport. The sections in the glacial drift of West Lancashire and Cheshire are mentioned, and the sequence and character of the overlying post-glacial beds. Southport is built upon blown sand resting on peat, which is 79 feet below the surface at the sea-coast, rising inland to the surface; the whole series rests on the Keuper marls, which have been bored into to a depth of 187 yards at the Palace Hotel, Birkdale, without finding the base. Fragments of gypsum and pseudomorphous crystals of salt occurred in the boring. The section in the Mersey tunnel, now in course of erection, was alluded to.

On the Pre-Cambrian Igneous Rocks of St. David's, by Prof. J. F. Blake, M.A., F.G.S.—The rocks below the Cambrian conglomerate have been described by Dr. Hicks as bedded rocks belonging to three distinct periods. The same rocks have been recently asserted by Dr. Geikie to be partly Cambrian and partly intrusive. The author contends that they are Pre-Cambrian in age, but form a very complete volcanic series, which may well be designated the Dimetian. The basis of the series is the Dimetian granite, serving as the core. This is surrounded by the more acid rocks, as the quartz-felsites and the felspar porphyries (the so-called Arvonian), and the more outlying portions consist of very varying materials, chiefly rough ashes or agglomerate breccias—on the east side finely bedded "halleflintas," and on the north side many basic lava flows. These are the so-called "Pebidian." The arrangement of these rocks shows the characteristic irregularity of volcanic rocks, and though many portions are bedded, they have no dominant strike over the whole district. The Cambrian series commencing with the conglomerates is quite independent and hangs together as a whole. In no case can a continuous passage be proved from the one series to the other; the junction is in most cases a faulted one, and at the places where this is not so, the conglomerate lies on different beds of the volcanic series.

On a Coral Atoll on the Shore Line at Arvigland, near Dumfries, Scotland, by James Thomson, describes a band of Carboniferous limestone, with corals of several genera, which form seventeen coral reefs, extending through a depth of 400 feet of strata.

On the Former Physical Condition of Glendale, Northumberland, by G. P. Hughes, describes the River Till, as once filling this valley, and forming a lake, on the site of which occurs peat, forest beds, grey clays, with *Bos urus*, *Cervus megaceros*, and red stag, and gravels, resting on boulder clay.

Additional Notes on Anthracosaurus Edgei, by W. H. Baily, describes a large Sauro-Batrachian from the lower coal measures, Jarrow Colliery, near Castlecomer, co. Kilkenny.

On Basalt apparently overlying Post-Glacial Beds, co. Antrim, by W. T. Knowles, describes a mass of basalt twenty yards in length, lying on sands and gravels; probably is a glacial erratic.

On the Geological Relatives and Mode of Preservation of Eozoon Canadense, by Principal Dawson.—The oldest known formation in Canada is the Ottawa gneiss, or fundamental gneiss, a mass of great but unknown thickness and of vast area, consisting entirely of orthoclase gneiss imperfectly bedded, and destitute of limestones, quartzites, or other rocks, which might be supposed to indicate the presence of land surfaces and ordinary aqueous deposition. It constitutes the Lower Laurentian of Logan, and may be regarded either as a portion of the earth's original crust, or as a deposit thereon by aqueo-igneous agency, and without any evidence of derivative deposits. Succeeding it is a formation of very different character, though still belonging to the Lower Laurentian of Logan. It may be named the Glenville series, and includes beds of limestone, quartzite, tin ore, graphitic and hornblendic schists, with local beds of pebbles; it is in one of these great limestones that Eozoon occurs. The Grenville series give distinct evidence of ordinary atmospheric erosion of the older rocks, and of ordinary aqueous as well as organic deposition. The author hopes to exhibit specimens, now in the McGill University, to the Association.

On the Topography and Geology of the Troad, by T. S. Diller.—The Liparites are older than the Andesites, rocks that are probably pre-Cambrian from the base of the older sedimentary rocks, which are much altered and often highly crystalline. The more sedimentary rocks are also partially crystalline; they are less important in determining the physical geography. Positions of streams have varied much, but the coast-line has probably changed little since the days of Troy. Mount Ida is an anticlinal with a very short axis, and is almost a dome, the summit of which has been denuded.

SECTION D—BIOLOGY

Department of Zoology and Botany

On the Origin and Development of the Rhinoceros Group, by Messrs. Scott and Osborne.—The very extensive series of Tertiary lake deposits in the north-western United States have afforded these gentlemen material for some generalisations on this subject. Their conclusions are as follows:—That from the Rhinoceros group of the Middle Eocene there diverge three distinct lines, one represented by the forms still living in the Old World, the other two exclusively American and extinct. The first of these lines is continued into the Upper Eocene formation by the genus Amynodon. In this form the rhinocerotic features of the skull are slightly more marked; the lower canines are somewhat more procumbent and have caused the atrophy of the lower incisors. The digits are four in the manus and three in the pes. In the Lower Miocene follows the genus Aceratherium, which, retaining the number of digits found in Amynodon and many lophidont skeletal characters, is yet an unmistakable rhinoceros. From Aceratherium, again, we get two diverging lines, one belonging to the Old World, the other to the New. These authors think that very probably Aceratherium originated in America, and migrated to Asia in early Miocene times. In the Old World it gave rise to the horned series of genera, probably beginning, as suggested by Cope, with

Ceratorhinus. In America are found a number of large rhinoceroses in the Loup Fork deposits of the River Platte, which are variously designated as Uppermost Miocene and Oldest Pliocene. These have left no successors unless *R. inennis* of Le Gros should turn out to be an Aphelops. In brief, the rhinoceros line branched off from the Lophiodontidæ in America during the Middle Eocene, in early Miocene times the genus Aceratherium migrated to the Old World, and there gave rise to the horned genera, which still live there, as well as the larger species which became extinct in the Post-Pliocene. The second line mentioned is represented by the curious genus Diceratherium. The third line is that of Hyracodon, small hornless animals of the Miocene. This retains the full set of incisors and canines in both jaws, but with rhinoceros-like premolars and molars. Many lophidont characters are still retained.

The Polymorphism of Alcyonaria, by Prof. Marshall.—The author directed attention to the occurrence of tentaculato-zooids in two members of the group Pennatulidæ—the first the variety of *Pennatula phosphorea*, known as aculeata, and the second a new species of Umbellula, *U. gracilis*, obtained in the Faroe Channel during the *Triton* dredging expedition in 1882. In the first case the tentacles, which vary from one to five in number, are fused together to form a conical spire strengthened by very stout calcareous spicules, and projecting a considerable distance beyond the mouth. In the case of *U. gracilis* the tentacle is single, and differs from that of all other pennatulid zooids in presenting a fringe of pinnules along each side identical with those of the typical polyps. The morphological importance of this unitentacular condition was discussed at some length, the single tentacle being shown to have constant anatomical relations and to correspond to the single tentacle present in the young embryos of *Actinia mesembryanthemum*. In conclusion, arguments were adduced against Prof. Kölliker's statement that Umbellula is one of the more primitive genera of Pennatulidæ.

The Differences between the Males and Females of the Pearly Nautilus, by Mr. A. G. Bourne, B.Sc.—The author bases his observations upon the dissection of two specimens, male and female respectively—both adult and well preserved—of *N. pompilius* obtained by Prof. Lankester for the museum at University College, and a specimen of *N. macromphalus* placed in his hands for examination by Prof. Hubrecht, of Utrecht University. The author regards the tentacular lobes as homologous with the arms of a Dibranch, while the tentacles probably represent the suckers, this view, which has already gained considerable ground, receiving very strong support from the hectocotylised condition which the author describes. Eight tentacular lobes may be recognised, four internal, two superior, and two inferior, the latter two being fused together, and four external, the two superior being fused to form the "hood," and the two inferior completing the external ring. In the male four tentacles of the left superior internal lobe become hectocotylised, while the corresponding four upon the opposite side exhibit an exactly similar modified condition, though in a very slight degree, forming a most interesting example of a "rudimentary organ." In the male the inferior internal lobes are present in a very much reduced condition.

Budding in Polyzoa, by Prof. Haddon.—This author asserted that according to most observers the buds in ectoproctous Polyzoa are derived solely from the endocyst, or according to Joliet, from the endosarc (funicular tissue). It is possible that a combination of these views may be the more correct, since the development of the bud itself appears to prove that several distinct tissues are implicated, and that as a matter of fact all the three embryological layers are concerned in this process.

On a Young Specimen of the Grey Seal (*H. gryphus*) *from Boscastle, Cornwall*, by Prof. Lankester.—Prof. Lankester had the good fortune to find a specimen of this seal at the above place about a fortnight ago. He carried it about a quarter of a mile to a more sheltered place, but found it in the original place the next morning. As the specimen was not more than twenty-four hours old, was very weak, and could not swim, it is very probable that the mother had carried it during the night. The animal was taken and fed on milk; he was sent to the Zoological Gardens, and when last heard of was doing well. Mr. Cordeaux said it was extremely interesting that this specimen had been found, as previously it had not been seen so far south. Prof. Moseley remarked that the dislike of young seals to the water had probably some connection with their descent from ancestors which inhabited the land quite as much as the water.

On Wool Plugs and Fertilised Fluid, by Mr. Duncan Matthews.—This paper describes in detail a series of experiments undertaken with the object of testing the filtering action of cotton wool plugs upon the atmosphere, and the consequent possibility of permanently preserving fluids sterilised in flasks plugged with wool. The author found, after a long series of experiments, that sprayed water carrying germs could pass through wool plugs as well as between it and the glass, when an inward current was produced by the cooling of the flask. He therefore sees no reason why air should not in the same manner carry germs through or alongside the wool. As an experimental fact, he found this to be so in a very large percentage of his experiments. All the experiments related to one kind of bacterium, the hay-bacillus.

The President then introduced the next four papers, which all related to various phases of the germ-theory, by a few remarks on bacteria. Micrococcus, bacillus, spirochœte, spirillum, and leptothrix were briefly spoken of, and the terms saprogenous, cromogenous, and pathogenous explained; the first of these papers was then read.

On the Germ Theory of Disease from a Natural History Point of View, by Dr. Carpenter.—Dr. Carpenter stated that many of the existing genera and species of animals and plants were altogether uncertain, that as fresh knowledge was gained, so it was found necessary to modify our accepted views; this especially holds good with genera which have great power of adapting themselves to various circumstances, and which consequently produce numerous variations. This power of modification, the author stated, was much more marked in the lower than in the higher forms of either kingdom, and was especially found in bacteria. The author then cited the case of the germ producing smallpox, in which he stated the germ had undergone such a modification, that whereas two centuries ago the disease was very severe, and known as "black-pox," it now existed only as a mild disease. During the last siege of Paris, however, the conditions were such that the germ reverted to its original form, and produced the same severe disease as two centuries ago. Many facts were brought forward to confirm this view.

On some Cell Contents in Coffee and other Plants, by Marshall Ward.—The author has for some time past been engaged in researches among the fungi, particularly those which attack living plants; and his attention was necessarily directed to cell contents of the host plants; among others the cells of *Coffea*, *Cinchona*, *Pavetta*, and *Canthium*, and one or two cryptogams have received special attention. The present paper refers particularly to one class of bodies found in the cells of the cultivated species of *Coffea*—*C. arabica*, *C. liberica*, &c. Certain fatty bodies, mixed with proteids, found in the endosperm, are traced into the embryo and seedling, and their reactions and changes noticed. In the leaves, cortex, and other soft parts of the mature plant are found "fat-bodies," under circumstances which compel the author to conclude that they are the result of constructive activity, and not products of destructive metabolism. These "fat-bodies" consist of varying mixtures of fats and other substances, probably, in part, proteid, and show considerable similarity to the fatty masses of the endosperm. Details are given of their reactions and changes, and the author believes that they represent temporary stores, to be worked up further in the construction of higher bodies.

On the Closed Condition of the Seed Vessel in Angiosperms, by Alexander S. Wilson, M.A., B.Sc.—Flowering plants may be divided into two classes, according as their seeds are contained within a closed seed vessel, or are exposed without any such covering. The former, having their seeds included in a pod or pistil, are called Angiosperms or cover-seeded; and the latter, on account of their naked seeds, Gymnosperms. The Angiosperms, which form by far the more important division, embrace most of the common plants which make up the bulk of our flora, and are universally regarded as the more highly organised of the two. Corresponding to the lower degree of organisation, Gymnosperms (yew, cypress, fir, &c.) appear earlier in the geological strata, and are largely represented in a fossil state. The pod of an Angiosperm, such as that of a wall-flower, is composed of metamorphosed leaves termed carpels. In nearly every instance these leaves are so united as to form a completely closed case enveloping the young seeds. At first sight it would seem as if the presence of such a covering were a disadvantage, for before the young seeds or ovules can develop to maturity they require to be fertilised. The process of fertilisation is effected by the agency of pollen dust, which is brought to the flower either by the wind or by insects visiting the flower in search of honey. Now in the case of Gymnosperms, where the seeds are exposed uncovered, this pollen dust,

if blown by the wind, simply alights on the surface of the seed and fertilises it directly. In plants with covered seeds, on the other hand, the pollen cannot gain direct access to the ovules, but can only fall on the surface of the envelope formed by the carpellary leaves. This covering has to be penetrated before fertilisation of the seeds can be effected. For this purpose several adaptations of tissues, modifications of structures, and changes in the position of the ovules are rendered necessary, all of which might easily be dispensed with were the seeds exposed as they are in Gymnosperms. It can hardly be supposed that all this specialisation, whereby the process of fertilisation so simply performed in Gymnosperms becomes complicated by being broken up into numerous subsidiary processes, should be called into play unless some very important end were to be attained by the presence of a completely closed pistil. What then is the *rôle* of the pistil? The young seeds are the most vital parts of the vegetable organism. Composed of delicate cells, containing much nitrogen and phosphorus, they may be said to constitute the chemical and physiological wealth of the plant. On this account they must be carefully guarded from any external influence that would degrade their chemical constitution or lead to a misappropriation of the nutritious matters they contain. Now it is well known that the leaves and stems of nearly all plants are subject to the attacks of parasitic fungi. The spores of these parasites germinate on the leaves of the plant on which they alight, and appropriate its juices to their own use, as, for example, in the case of the fungus which occasions the potato disease. All kinds of moulds, putrefaction, and fermentation are in like manner produced by the development of spores falling from the atmosphere which have found a favourable soil for their growth. Now a more suitable pabulum or nidus for the growth of mould germs can hardly be imagined than that which would be afforded by the immature ovules, seeing that in them is collected a large amount of easily assimilable matter destined for the nutrition of the embryonic plant. There can be little doubt then that the disadvantages which the pistil brings with it, and the higher organisation thereby entailed, are more than compensated for by the security which it gives against the entrance of fungus spores. The pea pod is in fact the counterpart of the hermetically sealed or stoppered flasks, in which Tyndal and Pasteur performed their well known experiments on the preservation of organic fluids against putrefactive changes. These observers found that it was possible to preserve beef tea or other organic infusion for any length of time, provided no air was admitted to the flask, or if care were taken to filter the air from all organic germs by passing it through cotton wool, &c., before allowing it to have access to the infusion. The pistil of a flower then may be regarded as analogous to the flask in these experiments. The loose cellular substance of the style, and the acid secretion on the stigma, may in like manner serve to filter the air before it reaches the ovules contained within the ovary. At any rate the air must pass through the substance of the carpels before it can reach the ovules.[1] When this fact is viewed in connection with the experiments of Van Tieghem, which show how difficult it is to effect the direct fertilisation of ovules with pollen, owing to the constant appearance of microscopic fungi, a new light is thrown on a vast number of vegetable and animal structures. The same principle operates not only among phanerogams, but even among the cryptogams; nor could a principle of such general application in the vegetable world have failed to play an important part in the animal kingdom. It is remarkable then to find that within the cup of the commonest wild flower we have the results of recent scientific research anticipated, the benefits of the antiseptic system as completely secured as by modern surgery, and a parallel between nature and art which agrees even to the minutest detail.

Protoplasmic Continuity in the Florideæ, by Thomas Hick, B.A., B.Sc.—The author has made an extensive series of observations on a large number of species belonging to the more important genera of Florideæ, with special reference to the question of protoplasmic continuity. He finds in all the species examined that there is such a continuity, and that of the clearest and most definite character. In the simpler filamentous types, such as *Petrocelis cruenta* and *Callithamnion Rothii*, the protoplasm of each cell is united with the protoplasm of contiguous cells by means of a fine protoplasmic thread. This obtains throughout the whole plant. In the more complex types, such as *Callithamnion roseum*, *C. arbuscula*, and *C. tetragonum*, the arrangements for continuity are of a more elaborate character. The contents of the axial cells are not only united with one another, but also with those of the cortical cells, however numerous these may be. The cortical cells also display continuity *inter se*. *Ptilota elegans* is a most instructive form, as here the connective threads may be easily traced from the tips of the ultimate branchlets to the base of the stipes of the frond. As the threads become older, they increase in thickness, thus showing that they are not merely temporary or effete structures. On the stouter connecting cords a sort of ring or collar is developed at about the middle point, and over this is stretched, *in some cases*, a delicate diaphragm. The behaviour of both ring and diaphragm when treated with microchemical reagents, is similar to that of the ordinary protoplasm.

On Peripatus, by Adam Sedgwick.—Mr. Sedgwick showed living specimens of this animal, and briefly described them.

Some newly-discovered Localities of the Rare Slug Testacella haliotoidea, by E. J. Lowe, F.R.S.—This rare and hitherto extremely local nest-eating slug has recently been found in various places in Monmouthshire and South Wales. Shirecester Hall, Shirenewton Village, Tatton Court, Hardwick, Chepstow, Cardiff, and various other places, were mentioned as producing more or less abundant quantities of this interesting creature.

Department of Anatomy and Physiology

On the Relations of Protoplasm and Cell-wall in the Vegetable Cell, by F. O. Bower.—After tracing the history of this subject, it was concluded that it has now been demonstrated with as much certainty as is possible by the use of microchemical and staining reagents, that in certain cases, the number of which is now constantly being increased, there is a direct connection between the protoplasmic bodies on opposite sides of cell-walls, and that this connection is established by means of fine strings of protoplasm which, in the cases observed, run nearly transversely through the walls. The question remains whether this is the *only* mode of permeation of the cell-wall by protoplasm. The author could not accept it as proved as yet that any further permeation of the cell-wall by protoplasm, as a reticulum or otherwise, really exists, but he brought forward certain grounds for regarding such a permeation as possible or even probable, taking into account chiefly those phenomena observed in *free cell-walls*, in order thereby to avoid any confusion with connecting strings, such as those already proved to exist:—1. The strings already observed vary greatly in thickness, from the well marked to the indistinguishable; thus we have evidence of the existence of strings which would probably not have been recognised were it not for comparison with other examples. Further, it has been shown, in the author's paper on plasmolysis, that protoplasm may be drawn out into strings so fine as to defy definition even by high powers of the microscope; thus there can be no objection on the ground of the small size of the hypothetical strings or reticulum. 2. Those cases in which a perforation of cell-walls has been demonstrated are those very cases in which a most efficient physiological connection is required. There is no reason why a less obvious permeation should be denied where the requirements are less, but by no means absent. 3. There is *a priori* probability of some form of permeation of cell-wall by protoplasm if Strasburger's account of the growth of cell-walls be correct. 4. A strong argument in favour of such general permeation of walls by protoplasm is found in the existence of important chemical changes in the substance of certain cell-walls *at points at a considerable distance from the main protoplasmic body*, *e.g.* formation of cuticular substance, wax, &c., which differ fundamentally from cellulose, are insoluble in water, and are apparently formed *in the wall itself*. The tendency of recent observations is to show more and more clearly how close the connection of protoplasm with the important chemical changes in the plant is; thus it appears probable that protoplasm is present in some form or other in the cell-wall. Reasons were also given for thinking that the exposure to air is not an important factor in the above changes. These and other considerations show that though this permeation of the wall cannot be accepted as proved as yet in any one case, still the subject deserves more close attention than it has yet received, while it may be expected that the application of new methods may produce definite results bearing on this very important question.

On the Occurrence of Chlorophyll in Animals, by C. A. MacMunn, M.D., F.C.S.—The difficulties attending the recog-

[1] This view of the function of the carpels is corroborated by the fact observed in the case of Reseda, the carpels of which open soon after fertilisation. After dry weather an accumulation of sand and dust frequently takes place within the ovary of Reseda.

nition of chlorophyll in animals was first referred to, and the writer stated that he had based his conclusions as to the identity of animal and vegetable chlorophyll on the fact that the wave-lengths of the centres of the bands of the same solutions of animal and vegetable chlorophyll are the same, and that the wave-lengths of the centres of the bands are the same when the same reagent is added to the respective solutions. Without committing himself to accepting the views of Kraus or Sorby, he applied the term chlorophyll to that colouring matter, or mixture of colouring matters, which can be extracted out of green leaves, such as those of *Primula*, by means of alcohol or alcohol and ether. The colouring matter, to which the writer has given the name "enterochlorophyll" (*Proc. Roy. Soc.* 226, 1883), and which can be extracted from the liver or other appendage of the enteron of invertebrates, was shown to be probably produced by, and in, the body of the animal, and for certain reasons (detailed at length) not food chlorophyll. The absence of parasitic algæ in sections of the livers of certain mollusks which yield enterochlorophyll shows that this pigment cannot be due to their presence. The writer further showed that Pocklington's observations, published in the *Pharmaceutical Journal* (1873), on the presence of chlorophyll in the wing-cases of Cantharides beetles, could be verified, and he had succeeded not only in verifying the presence of the principal chlorophyll band in the ethereal, chloroformic, and alcoholic solutions of the wing-cases, but the changes produced in the spectra of these solutions on the addition of certain reagents showed the presence of a body indistinguishable from vegetable chlorophyll. Hence Leydig's conclusion as to the presence of that colouring matter in insects was proved to be correct. However, in the case of green larvæ the mere occurrence of a band in red when a strong light is concentrated on the integument may be merely due to the presence of food chlorophyll in the intestine, for, on squeezing out the contents of the latter, the green colour and the band both disappear. The function of chlorophyll was then referred to: it was shown that it could hardly be of much use in respiration, as oxidising and reducing agents do not affect it; that for protective purposes or in mimicry a body of less complex chemical composition might answer equally well, except that the eyes of some invertebrates may be more susceptible to rays of light of a certain wave-length than our own, especially as Sir J. Lubbock has shown that ants perceive the ultra-violet rays of the spectrum which are invisible to us. It may possibly be the persistence of a pigment which was once useful in a remote ancestor in some cases, perhaps at a time when the atmosphere contained much more carbon dioxide than at present. Or again, it may be of use in absorbing the chemically active rays of the spectrum when occurring on the surface of an animal, especially as Zimiriazeff had shown that Langley's observations with the bolometer have proved that the point of maximum energy of the solar spectrum corresponds with the principal chlorophyll band between B and C. In the case of enterochlorophyll this colouring matter may be of use in furnishing material for the construction of other colouring matters, especially as this body and hæmochromogen exist side by side in the bile of some mollusks; and in the bile of the sheep and ox a body exists which fluoresces red and resembles chlorophyll closely, but possesses at the same time some properties which show that it is a hæmoglobin derivative, as proved by the writer (*Proc. Roy. Soc.* No. 208, 1880, and *loc. cit.*). The conclusions which had been arrived at gave support to the view which Prof. Lankester had maintained, namely, that chlorophyll may occur quite independently of the presence of parasitic algæ, as in *Spongilla* and *Hydra*, and that it is in some cases produced synthetically by and in the bodies of animals.

On the Intercellular Connection of Protoplasts, by Prof. W. Hillhouse.—In this paper the author gives the results of a large number of observations to prove the intercellular connection of protoplasm. Out of twenty-two plants examined, these connections were only found in the cortical tissue of *Ilex aquifolium* and *Æsculus hippocastanum*, the pulvinus of *Prunus laurocerasus*, and the winter bud pith of *Acer pseudoplatanus*; he, however, points out that these connections are easily broken in preparation, and that a single connection between a number of cells would be sufficient to produce a perfect unity of action. His conclusions are:—1. That protoplasmic threads connecting neighbouring protoplasts are present in such widely different and diffused structures as sieve-tubes, cortical parenchyma, leaf-pulvinus, pith of resting leaf-bud, and endosperm of seeds. 2. That in the contraction of the protoplast in natural plasmolysis these threads would normally remain unbroken. 3. That they *may* serve to transmit impulses from one cell to another, acting in this way somewhat like a nervous system. 4. That besides the perforating threads, equally widely spread and much more numerous, are threads which attach the protoplast to the cell-wall, whether at the base of pits or otherwise, and that these threads are often opposite each other. 5. That the closing membrane separating two threads often shows differentiation, which suggests permeability, if not "sieve perforation." 6. That in the contraction of the protoplast in natural plasmolysis these threads would naturally be unbroken. 7 That these threads may, when in extension, act upon the cell-wall and put it in a state of slight positive tension. 8. That the presence of minute perforations communicating from cavity to cavity of living cells *would* not, and when communicating with the intercellular spaces *need* not, be a hindrance to the turgipotence of the cells.

On the Continuity of Protoplasm through the Walls of Vegetable Cells, by Walter Gardiner.—The author, after briefly reviewing the work which has already been done in this department, goes on to describe his own experiments with Mimosa, Robinia, Dionæa, and other sensitive plants, and with thickened endosperms in general. In all organs of movement examined, the freely pitted parenchymatous cells were found to communicate with one another by means of delicate protoplasmic threads, which perforate the closing membranes of the pits. The author remarks that the existence of a communication between adjacent cells appears to be very wide, if not of universal occurrence. His own observations, extending over a series of fifteen species of palms and representatives of some thirteen orders, were all found to bear out the above researches, as in all cases definite and well pronounced continuity existed.

On the Muscular Movements that are associated with certain Complex Motions, by R. J. Anderson, M.A., M.D.—When a muscle contracts, one extremity or both extremities may move. When one extremity moves whilst the other is fixed, the fibre may describe a plane surface, as when the moving end lies in a right line or a cone, as when the moving extremity lies in the circumference of a circle or other plane curve. If the fibre lie in the plane of the circle, the cone will be reduced to a plane. Where both extremities move, the fibre may describe a plane, or a cylinder, or a ruled surface of a high order. It frequently happens that when one extremity of a fibre is fixed the other extremity moves in a circle, which itself experiences a movement of translation. The moving point then describes a trochoid, examples in pronator teres and pectoralis major. Muscle fibre may describe curves of a complex nature, although the muscles themselves form a simple surface, as in the two muscles already cited.

SECTION G—MECHANICAL SCIENCE

A Comparison of Morecambe Bay, Barrow-in-Furness, North Lancashire, West Cumberland, &c., in 1836 *and* 1883, by Hyde Clarke.—The writer gave an account of his plans and surveys in 1836 for forming a through line of railway from Lancaster, through Furness and West Cumberland, across the Solway to Dumfries, and thence to Glasgow, by the course now adopted by the Glasgow and South-Western Railway. The chief feature was the passage and embankment of the large estuaries called Morecambe Bay. The history of this undertaking was given, with details of the plans of Messrs. Hyde Clarke, George Stevenson, Hayne, Rastrick, &c., and the works carried out by Mr. James Brunlees. The plans of the Warton Land Company were described. The effect of the undertaking in the development of Barrow or Foudrey and the iron manufacture of Furness was illustrated. There were still 40,000 acres to be reclaimed, and capable of becoming good agricultural ground. If reclaimed it would enable a railway to be carried across the bottom of the Bay. There was now a population of 50,000 in Barrow, and although there had been great depression there were elements which pointed to a probable increase of from 100,000 to 200,000 persons.

The Term "Stability," as used in the Literature of Naval Architecture, by Prof. Osborne Reynolds.—The author explained that the origin of the paper had been the report and discussion which had taken place in regard to the lamentable disaster which happened to the *Daphne*. Stability meant a state of being able to maintain a particular position against any forces which tend to upset; or another way of expressing the same thing was a state of ability to maintain a position after being disturbed and allowed to go free to recover itself again. It appeared from

the report of Sir E. J. Reed on the *Daphne* disaster, and the discussion which resulted, that naval architects were using the term stability both in its proper sense, as meaning a tendency to hold a particular position, and also as meaning a tendency to change position in a particular direction. The writer of the paper proceeded to urge the desirability of using two terms, the one to express the greatest angle of disturbance from which a vessel would return to her normal position, and to limit the quantitative meaning of the term "stability" to the measure of that angle, using the term "stiffness" to express the moment of the upsetting forces necessary to produce any particular angle of disturbance. The adoption of that system, which was consistent and definite, would prevent the confusion into which it appeared naval architects had fallen, and it would then be seen that what were ill-called curves of stability would be well-called curves of stiffness.

On the Construction and Working of Alpine Railways, by J. B. Fell, C.E.—There are three Alpine railways in existence at the present time—the Mont Cenis and St. Gothard Railways, which have been made with long summit tunnels and with ordinary gradients, and the Brenner Railway, that has been made with similar gradients but without a long tunnel. The important question has now arisen, and has been taken into serious consideration by the Governments and local authorities interested, as to how far it may be possible to make other trans-Alpine railways, some of which are urgently needed, at a cost that would render them financially practicable; and to accomplish this object it has been proposed to effect a reduction of one-half or more of the cost, by carrying these railways over the mountain passes by means of steep gradients and the use of the centre rail system, as it was adopted on the Mont Cenis Railway. Upon these improved summit railways the same weight and number of trains could be run that are now running on the Mont Cenis Tunnel Railway, and with the protection of avalanche galleries and covered ways the regularity of the service would be maintained at all seasons of the year. The extra cost of working expenses caused by working over a higher level than that of a tunnel line would, if capitalised and added to the cost of construction, still leave a clear net saving of more than one-half in the cost of construction as compared with the cost of a tunnel railway. The result of the experiences of the last twenty-five years seems to point to the conclusion that a method of constructing Alpine railways with long, non-paying tunnels is a thing of the past. The future belongs to the best system that can be devised for overcoming the difficulties of trans-Alpine railways rather by adding to the powers of the locomotive engine and by other mechanical appliances for reducing the cost of traction on steep inclines, which methods are capable of indefinite improvement, than by burying in gigantic tunnels enormous sums of unproductive capital that, when once expended, are irrecoverably lost.

The Euphrates Valley Railway as an Alternative Route to India, by J. B. Fell.—The author described the proposed route, and gave the total cost as 8,500,000*l*. He stated that, when not only its commercial but also its strategical and political advantages were taken into account, it must be admitted that the Euphrates Valley Railway certainly has the prospect of being one of the most successful enterprises in the world. Canon Tristram detailed his experience in the Tigris and Euphrates valleys, and stated that he believed the former to be the preferable route.

On Injector Hydrants, by J. H. Greathead.—This paper described the method proposed for the author for meeting the serious increase of fires in the metropolis. A separate system of water supply at very high pressure would be laid under the footway with hydrants at short distances apart. The high pressure water would be used in conjunction with the ordinary water supply in the mains, and jets of water would thus be enabled to be raised to sufficient heights without the aid of fire-engines. The paper was illustrated by numerous diagrams, and elicited an interesting discussion, generally favourable to the author's views.

Nest Gearing, by Prof. Fleeming Jenkin.—This paper contained an account of a new friction gearing, the chief novelty being in the mode of obtaining any required amount of pressure between the wheels which roll upon each other. As many as thirty-two modifications have already suggested themselves, and the opinions expressed in the discussion were unanimously in favour of the invention as being a very valuable one.

Electric Launches, by A. Reckenzaun.—The paper commenced with a description of the launch *Electricity*, which made her first trip in September, 1882. The *Electricity* is 25 feet long, with a 5 feet beam, and draws 21 inches forward and 30 inches aft. Her speed is 8·3 miles per hour with ten passengers on board. Forty-five Sellon-Volckmar accumulators stored under the seats and decks forward and aft supplied the current to two Siemens D_3 Series dynamos placed side by side on the floor of the boat, with their axes parallel to the propeller shaft. A Carliss-Browne two bladed propeller of 20 inches diameter and 3 feet pitch was employed in these first experiments; straps and pulleys were resorted to in order to reduce the speed of the screw to 350 revolutions, whilst the motors revolved at 950 revolutions per minute. The two motors were coupled in parallel circuit, whereas the cells formed one series. Each machine had its own switch and ammeter, and the starboard machine could be stopped mechanically by means of a friction clutch on the countershaft. Both machines were tested with a Prony brake, and they gave 1·86 horse-power on the brake at 950 revolutions, consuming a current of 21 amperes and 100 volts. At 694 revolutions, 100 volts and 33·25 amperes, the brake horse-power rose to 2·78. With 47 cells on board, the current used by both motors running together was 46 amperes, and the propeller made 360 revolutions; when disconnecting one of the motors the current passing through the other was 33 amperes, and the speed of the propeller shaft fell to 250. Messrs. Siemens' dynamos lend themselves very readily to the purposes under consideration; the height of a D_3 machine is only 10 inches, length 28 inches, and width 23 inches. The two machines weigh together 632 lbs., countershaft, supports, and pulleys 180 lbs., total for the driving apparatus 812 lbs.

Electric Launches, by J. Clark.—This paper contained a very brief account of advances in this subject.

The Fire Risks of Electric Lighting, by Killingworth Hedges.—The author first drew attention to the great difference between the electric currents which have been in constant use for telegraphic purposes and those which are to be supplied by the undertakers under the Electric Lighting Act. The latter can only be said to be free from danger when the heat generated by the current is utilised in its right place, and not developed in the conductors or wires which lead the electricity to the incandescent lamps. The Fire Risk Committee have already issued rules for guidance of users of electric light; these can hardly be said to embrace all the salient points of the new subject, which can only be arrived at after years of practical work. The necessity of proper regulations has already been recognised by the insurance offices, both in the United States and Germany, and some of their special rules are given in this paper. The conductors must be properly proportioned for the current they have to carry; whatever resistance there is in the conductor will cause a corresponding development of heat, which will vary with the amount of electricity passing, and inversely as the sectional area. As the temperature in Dr. Matthiessen's experiments upon the subject was not increased over 100° C., the author has made some further experiments—heating the wires by the electric current from a secondary battery to within a few degrees of their melting-point. Various materials were tried—the wires and foils having such sectional area, and so arranged that, on the current being increased by 20 per cent., they were immediately fused. The total length of each experiment was twenty-four hours, during which time the current passing through varied slightly. The results of the experiments were then given.

SCIENTIFIC SERIALS

Archives of the Physical and Natural Sciences, Geneva, September 13.—Verification of some atomic weights (second memoir); zinc and magnesium, by M. C. Marignac. The atomic weight of zinc, fixed by Erdmann at 65·05 and by Favre and Jacquelain raised to 66, is approximately determined at 65·33, a figure which further analysis may show to be slightly too low. For magnesium, calculated by MM. Marchand and Scheerer at 24 and by others at 24·5, the number 24·37 results from the author's fresh experiments.—Essay on the protistology of Sardinia, with a description of some new or little-known lower animal organisms, by Prof. Corrado Parona. In the fresh and marine waters of Sardinia the presence is determined of as many as 228 species belonging to the families of Bacteria—Monera, Flagellata, Lobosa, Diatomea, Heliozoa, Ciliata, Acineta, and Catallacta. The paper is accompanied by seven illustrations.—Memoir on earthquakes and volcanoes (continued), by Prof. F. Cordenons. In this second and concluding part the

author expounds his own views, and argues against the generally accepted theory that underground disturbances of all sorts have their source, not in the upper but in the lowest regions of the earth's crust.—On a case of commensalism between a fish (*Caranx melampygus*) and a medusa (*Crambessa palmipes*), with two illustrations, by M. Godefroy Lunel. In this instance the fish appears as the parasite or guest of the medusa, taking up its abode in one of its cavities, which it enters and leaves at pleasure without apparent injury to the gelatinous substance of the sea-nettle. This circumstance, which has been fully verified, seems to throw a new light on the relations of a species of *Schedophilus* to the medusa, on which it is supposed to feed, and has accordingly, by Prof. Cocco, been named *Schedophilus medusophagus*. One of these is described by Günther in the *Transactions of the London Zoological Society*, October, 1882.—Meteorological observations with tables of temperature and barometric pressure made at the Observatory of Geneva and on the Great Saint Bernard during the month of August.

Rivista Scientifico-Industriale e Giornale del Naturalista, July 15 and 31.—On the measurement of altitudes by means of the barometer, by S. Paolo Busin.—Further remarks on a new experiment in electrolysis, by Prof. Eugenio Semmola.—On the comparative electric resistance of fixed and vibrating metal wires, by Prof. Angelo Emo.—An essay on some new applications of the hyperbolic functions to pseudo-spherical surfaces, with a description of Gronau's tables for all kinds of trigonometrical functions of cyclic and hyperbolic sectors, by Prof. Angelo Forti.—On the language of birds, by Prof. Luigi Paolucci.

SOCIETIES AND ACADEMIES

London

Entomological Society, October 3.—Mr. R. McLachlan, F.R.S., vice-president, in the chair.—Two new members were elected.—Mr. F. P. Pascoe exhibited several interesting British *Hemiptera*, and Mr. T. Wood exhibited a supposed new British species of *Malthodes*.—Mr. W. F. Kirby (on behalf of M. Wailly, who was present as a visitor) exhibited a large box of bred specimens of various *Saturniidæ*, &c., and some living larvæ of *Telea Polyphemus*, and *Hyperchiria Io*.—Mr. Billups exhibited specimens of the celery fly (*Tephritis cnopordinis*), and a small larva of *Meloe* (?).—Dr. D. Sharp communicated some proposed alterations of names in the genus *Batrisus*.—Mr. W. F. Kirby read notes on the Diptera of New Zealand, supplementary to Prof. Hutton's list of 1881.

Sydney

Royal Society of New South Wales, July 4.—The Hon. J. Smith, C.M.G., M.D., president, in the chair.—Ten new members were elected and sixty-three donations received. The following papers were read:—By the Rev. J. E. Tenison-Woods, F.G.S., &c., on the Waianamatta shales.—By R. Etheridge, jun., further remarks on Australian Strophalosiæ; and description of a new species of Aucella from the Cretaceous rocks of North-East Australia.—Prof. Liversidge, F.R.S., &c., exhibited specimens of tin ore; he explained that most of the tin worked in this colony was alluvial tin, though occasionally thin veins of crystallised tin had been met with. Those shown, however, were from a vein which had already proved to be of a width of ten feet, and the full width had not yet been reached. The tin, as could be seen, was disseminated through the felspar, and the specimen, which came from the Stannifer Bischoff Mine in New England, closely resembled the ore found in the St. Agnes Mine, in Cornwall, England.

August 1.—The Hon. J. Smith, C.M.G., M.D., president, in the chair.—Three new members were elected, and sixty-seven donations received. The following paper was read:—On plants used by the natives of North Queensland, &c., for food and medicine, by E. Palmer.—Mr. J. Trevor Jones, City Engineer, exhibited and explained the MacGeorge test, an instrument for determining the deviation in diamond drill bores.

Paris

Academy of Sciences, October 1.—M. Blanchard, president, in the chair.—On the slow upheavals and subsidences of the ground, by M. Faye. In reply to M. Issel of Genoa, the author revives the old theories of Elie de Beaumont, Cordier, and many others, and argues that the progressive cooling of the earth's crust goes on at a more rapid rate under water than on dry land. There is nothing hypothetic in this view, which might have been deduced from the thermometric soundings taken fifty years ago by the *Venus* in deep seas, and repeated with similar results in recent times. It follows that the solidified crust is much thicker under the oceans than on the continents. Hence also the liquid mass in the interior of the globe is subjected to far greater pressure under the seas than on the main land; and as this excess of pressure is diffused more or less rapidly in every direction, the less dense continental crust must yield to the pressure exercised on it from within. It is thus being everywhere continually upheaved, while the submarine crust, becoming denser and denser, is slowly subsiding.—Note on the recent attempts made by M. Delauney and others to foretell seismic disturbances, by M. Daubrée. The author concludes that the hitherto collected statistical data are insufficient to justify any theorising for the present on the future recurrence of earthquakes.—Separation of gallium (continued); separation from tantalic acid, by M. Lecoq de Boisbaudran.—Researches on the encephaloid cancer, by M. C. Sappey.—On the destruction and utilisation of the carcasses of animals dying of contagious diseases, and especially of charbon, by M. Aimé Girard.—Observations made at the Observatory of Marseilles, by M. Coggia.—On the calculus of perturbations, by M. A. de Gasparis.—On the approximate evaluation of integers, by M. Stieltjes.—On the interpretation of some phenomena of the solar spectra, by M. L. Thollon.—On the transport and distribution of electric force; experiments made at Grenoble by M. Marcel Deprez, by M. Boulanger.—On the presence of arsenic in certain wines in the absence of all foreign colouring matter, by M. A. Barthélemy.—Quantitative analysis of the chloroform in the blood of an animal treated with this anæsthetic, by MM. Gréhant and Quinquaud.—Researches on parasitic infusoria, with an account of fifteen new species of protozoa, by M. G. Kunstler.—On the marine lamprey, by M. L. Ferry.—On the caterpillar that feeds on the citron blossom, by M. Laugier.—On the position of a fœtus found in a *Pontoporia Blainvillei*, by M. H. P. Gervais.—On a meteor observed at Evreux on the night of September 23, by M. H. Dubus.

CONTENTS

THURSDAY, OCTOBER 18, 1883

WILLIAM E. LOGAN

Life of Sir William E. Logan, LL.D., F.R.S., First Director of the Geological Survey of Canada. By B. J. Harrington. B.A., Ph.D. (London: Sampson Low and Co., 1883.)

CANADA claims the honour of being Logan's birthplace. During his lifetime she fully appreciated how much he had done for her, how unweariedly and generously he worked for her material interests, and that the renown he achieved cast a reflected glory upon herself. After his death it was but fitting that the story of his life should be written in Canada, where his best years were spent and where his main work was accomplished. And yet he was personally so familiar on this side of the Atlantic, so universally known and loved, so linked with early geological associations and with the fathers of geology in this country, that there may be readers of the volume before us who will be surprised to learn that he cannot strictly be claimed as one of the illustrious phalanx of geologists born within these islands. They may console themselves, if they choose, with the reflection that, though not actually ushered into life here, he came over in boyhood, received the closing part of his education at Edinburgh, began his geological career in Wales, and had already attained eminence as an original observer there before he was called upon to undertake the Geological Survey of his native province.

Logan was one of the most lovable of men. Simple and unsuspicious as a child, he was always at the service of any friend who needed his help, and too often also of strangers who preyed on his time and good nature. And yet with this gentle side of his character, there were combined a sturdy independence, an indomitable perseverance, an inexhaustible enthusiasm, which carried him up to and even beyond the limits of his physical strength. What infinite humour twinkled in those grey eyes, as he quietly told his reminiscences of camp-life, or of more civilised travel, or his experiences of politics and politicians with whom he had to fight for the existence of his Canadian Survey! How delicately and good-humouredly his satire played round these Philistines, who cost him withal many an anxious hour by day and many a sleepless hour by night! There was a calm self-possession in him, a consciousness of strength that could be put forth if needed though usually kept out of sight in the background, a determination to do his own duty and to see that others in the same matters did theirs.

Those who knew him and who recall these distinctive characteristics of him will be glad to have Dr. Harrington's memoir. The picture it gives of Logan's boyish years, told mainly in his own letters, is delightful. His overmastering affection for his family, his interest in everything at home, his eagerness to hear of and from each beloved one, his lengthy descriptions of all he thought likely to interest the home circle, are graphically told. It is not difficult to see how such a boy should have developed into such a man.

Born at Montreal in the year 1798 of Scottish parentage, Logan was sent at the age of sixteen to continue his education at the High School of Edinburgh, where he so greatly distinguished himself that a brilliant career at the University was open to him. But he determined to enter upon commercial pursuits at once, and accordingly in the year 1817 took a place in the counting-house of his uncle, Mr. Hart Logan, in London. There he remained for fourteen years, during which there seems to have been nothing in his pursuits to develop the strong scientific bent that so completely dominated his later life, though we find that in his leisure he read books of science, especially in mathematics and chemistry, and asked to be supplied with some good work on mineralogy and geology. It seems almost by accident that he became a man of science. In the year 1831 he left London to take up his residence in Swansea, in charge of the books of a mining company in which his uncle was interested. But besides the books, he soon was called on to attend to the working of the mines and the smelting of the copper. Here at last he found an outlet for his love of nature and desire for scientific inquiry. He could not be content with the mere routine of his duties. Providing himself with the necessary surveying instruments, he began a geological survey of the Glamorganshire coal-field. He traced out the outcrops of the seams and positions of the faults with such minute care, that when some years afterwards De la Beche extended the Geological Survey to that region he found Logan's map so good that he gladly adopted it when it was generously handed over to him by its author. Logan's name accordingly appears on the published sheets of the Geological Survey of Wales, together with those of the members of the staff by whom the rest of the ground was examined. It was while looking after his uncle's coal-mines in this region that he was led to make his well-known observations on the rootlet-beds below coal-seams and to settle thereby the vexed question of the origin of coal.

There had been various efforts to establish a Geological Survey in Canada, but these had successively failed until 1841, when a sum of 1500*l*. was placed on the Parliamentary estimates. Next year the arrangements were completed, and the task of organising and conducting the Survey was intrusted to Logan. From 1842 till he resigned in 1869 he continued to be the life and soul of the Canadian Survey. The task he undertook was truly a colossal one. Almost nothing was known of the geology of the country. There were no maps on which geological lines could be traced. Thousands of square miles were unexplored trackless forest. Logan had not merely to find out the geological structure, he had to construct the very topographical maps on which it was to be delineated. He had to work with his own hands and train his assistants to work with him. He had to live among the wilds for months at a time, traversing hundreds of miles in canoes and on foot, with Indian guides and helpers. When winter made the further prosecution of field-work impossible, there were all the results of the summer to tabulate and to keep him fully occupied till it was time to start again. But his time was not always uninterruptedly given to these congenial labours. Though the Survey started under favourable auspices and with the support of the Government of the day, there were not wanting economists in and out of the Legislature who failed to see the usefulness of the enterprise and who objected to the

continuance of the annual grant for its prosecution. In nothing did Logan show his admirable tact and knowledge of men more than in the way he met these objectors, and turned them, if not into active friends, at least into passive though perhaps not wholly convinced spectators. Long before his death he had the satisfaction of seeing the establishment he had founded advanced in popular vour and equipped with a much more liberal endowment than he had been content with in its modest beginnings.

After the year 1851, when the reign of Universal Exhibitions began, Logan was frequently under the necessity of coming to Europe to look after the interests of Canada at the various capitals where the products of all nations were collected. There can be no doubt that though, so far as his proper scientific researches were concerned, these summers were entirely wasted, they were of the utmost service to the province. The collections of the Canadian Geological Survey were always one of the most interesting features in the Colonial galleries, and there can be no doubt that they did much to make the resources of the country widely known all over the world.

Of the value of Logan's services to science in general and to Canadian geology in particular, the best evidence and monument are to be seen in the voluminous series of Reports which he published, and in the truly admirable museum of Canadian minerals, rocks, and fossils which, with the aid of his colleagues, he formed at Montreal. As many of his letters show, he possessed no little literary faculty, but he never cultivated it, and indeed he hated the drudgery of writing. Had he been ambitious of fame, he might have attained a far wider reputation. His long years of exploration, his adventures and experiences by mountain, river, and forest, his felicitous power of rapid sketching, would have furnished him with ample materials for successive important and deeply interesting volumes, while his unflinching regard for truth and entire abhorrence of exaggeration would have lent to his pages a peculiar charm. But his ambition was to be at work in the field. There he continued at his post until after he had passed his seventieth year, when the confederation of the provinces extended the sphere of the operations of the Survey across the entire continent. Feeling himself no longer equal to the increased duties of his office, he resigned his connection with the Survey in the beginning of 1869, intending to devote himself more particularly to the investigation of some geological questions in which he took special interest, and to see more of his friends in Europe and of European geology than had previously been in his power. But he did not long enjoy the leisure he had so well earned. Retiring to Wales, where his sister lived, he spent there the autumn of 1874, but before the end of winter began to be seriously ill. He lingered until summer, and died on June 22, 1875.

The narrative of Logan's life by Dr. Harrington is simply and effectively given, much of its interest being derived from the extracts from the journals and letters. A few sketches are reproduced from the note-books. The only fault we are inclined to find with the book is that so few of these sketches have been given. We remember many a long year ago being allowed to peruse some of the note-books, and laughing heartily over the humorous delineations of camp life on the Canadian rivers. Those who knew Logan will be glad to have this pleasant souvenir of him. Those who never knew him may learn from it something of the charm that will keep his memory green in the affections of many friends, both in the Old World and in the New.

ARCH. GEIKIE

OUR BOOK SHELF

The Sea Fisheries of Great Britain and Ireland. By E. W. Holdsworth. (London: Stanford, 1883.)

NOTWITHSTANDING the extensive literary productions connected with the Fisheries Exhibition itself, many works, suggested by it, have already appeared. One such is the volume before us—an admirable digest of the question in hand. In the present state of our knowledge, the chapter devoted to Ireland is exceedingly welcome, and it may not be too much to hope that, in spite of the gradual decline of its fishing industry, that country may yet seize hold of this, at least one remaining hope. The book is written in excellent style, clear and concise, well balanced, and up to date; of convenient size to be carried in the pocket, and provided with a good index, we can strongly recommend it to the host of visitors who frequent our coasts, and so often find their way instinctively to the nearest fishing village. One prominent feature is the description of the various "rigs," a subject of great importance of late, but which does not appear to have been sufficiently dealt with in the Fisheries manuals. The universal outcry of want of statistics is raised, and the writer has made the best of such as he has gathered—largely, from private sources. Those relating to the gradual increase of larger and improved boats in our fishing fleets are interesting, as leading up, we may hope, notwithstanding acknowledged difficulties in the way, to the introduction of steam-power. The illustrations are good, but additional ones, setting forth the different "rigs," and doing justice to other nets, as does that given to the beam-trawl, would be acceptable.

Agricultural Chemical Analysis. By Percy Faraday Frankland. (London: Macmillan and Co., 1883.)

DR. PERCY FRANKLAND has done excellent service by the publication of his book. There is good reason to believe that practical agriculturists are rapidly becoming alive to the importance of a knowledge of the scientific principles which underlie their art, and the action of the Science and Art Department in fostering the study of those principles is calculated to increase the intelligent appreciation of their value. The relations of chemistry to agriculture have been indicated in times past in this country in the writings and teaching of such men as Davy, Johnstone, Ure, and others, and in more recent times in the elaborate reports which we owe to the patient industry and zeal of Sir J. B. Lawes and Dr. Gilbert. Nevertheless it is to be feared that these works have been practically sealed books to the great majority of even the more enlightened of our agriculturists. Farmers are proverbially, and perhaps not unnaturally, a conservative class, and apparently nothing but the pressure of competition will force them from the beaten track. But science is abroad even in Arcady. Farmers who buy feeding-stuffs and artificial manures soon show a very rational appreciation of the significance and value of such items as "albuminoids," "soluble phosphates," and "available nitrogen." They will find all about such matters, together with much else relating to the chemistry of their art, in Dr. Percy Frankland's excellent little work.

T. E. T.

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications.*

[*The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to insure the appearance even of communications containing interesting and novel facts.*]

"Elevation and Subsidence"

I HAVE only to-day found time to read Mr. Starkie Gardner's essay on "Elevation and Subsidence"; in the last paragraph Mr. Gardner states that the views he advances are not accepted by all geologists. As one who is entirely opposed to them, will you allow me to state as briefly as possible on what grounds I object to the theory he propounds, and how I account for the observed facts he mentions in support of it.

It is generally admitted that nearly the whole of the sedimentary rocks were deposited in shallow water and in slowly subsiding areas; no one who has examined the Cambrian deposits of the Longmynd or the Silurian deposits of the Ingleborough district, will dispute this assertion. Mr. C. E. Dutton, in his recently published monograph of the survey of the Grand Cañon district states, "Throughout the entire Plateau Province the strata are shallow water deposits."

That in areas so widely separated the same phenomena should occur must necessarily suggest that similar processes of subsidence and deposition were taking place. The sea-bottom in these areas was, I believe, subsiding, not as Mr. Gardner suggests "*pari passu*" with the deposition and in consequence of it, but at a *slower* rate than that of the deposition, as the result of forces actuating the crust of the earth, which are quite independent of either deposition or denudation.

The result of a slow subsidence and a more rapid deposition would be that in course of time the surface of the deposited matter would rise above low-water level and would be subjected to the levelling or denuding action of the tides, any accumulation of deposited matter above this level would be swept into deeper water, all the phenomena of ripple marks, sun-cracked surfaces, and worm trails would occur, and any tendency of the slow rate of the subsidence to lower the surface receiving the deposited matter would necessarily be continually neutralised by fresh accumulations.

The areas of subsidence would probably present the appearance of the large stretches of sand-banks which may be seen at the mouths of the Mersey and of most of our rivers; these banks are exposed at low and covered at high water.

The accumulation of strata would continue as long as the subsidence took place (providing material were brought down to the sea); if the subsidence ceased, the material resulting from denudation would be spread over a larger area, but no additional thickness or strata could be formed above the level just mentioned; on the other hand, if the deposition ceased and the subsidence continued, an area of deep sea would be formed, and probably a stratum of limestone would be accumulated.

Further, the elevation of areas over which large accumulations of matter have been deposited cannot have taken place in consequence of denudation resulting in a greatly reduced weight being distributed over the area of elevation, as suggested by Mr. Gardner, for denudation has necessarily followed elevation.

Every formation appears to me to contain evidence that subsidence took place independently of deposition, and elevation independently of denudation.

The Cambrian and Silurian rocks in this country appear to have been deposited over areas in which the rate of subsidence has at one time been less, at another time greater, than the rate of deposition; the Silurian limestones no doubt represent periods of subsidence during which no deposition of denuded matter took place. At the close of the Silurian era an upheaval must have occurred, the result of forces powerful enough to overcome the weight of both the Cambrian and Silurian formations, which appear to have been thrown into a series of vast anticlinal and synclinal curves. In the Longmynd district and near Ingleton the strata are either vertical or inclined at a great angle.

One result of this upheaval would be the formation of comparatively shallow lakes or inland seas, in which the Old Red Sandstone would be deposited; another would be the formation of a land surface of Silurian rocks which would be subjected to subaërial denudation.

According to Mr. Gardner's theory (in support of which he refers to the Himalayan range), the elevation of the Silurian rocks should have been continuous, for denudation would affect them in the same manner as it is said to affect that great range, and possibly the accumulating Old Red Sandstone would react on the Silurian land surface as Mr. Gardner suggests the subaërial deposits of the sub-Himalayan range react on the main mountain chain.

Instead, however, of the continued upheaval which theoretically should have taken place, a subsidence of the denuded Silurian rocks commenced apparently over a very large portion of this country, resulting in the formation of a deep sea in which the limestone, the base of the Carboniferous series, was deposited, in Derbyshire to a depth of nearly 5000 feet. Did this vast accumulation of limestone cause a further subsidence? No, the forces actuating the crust of the earth were in no respect interfered with; a period of elevation must have followed, and a comparatively shallow sea was filled up with the Yoredale shales and millstone grits, and a land surface formed represented by the lowest coal seam—a coal seam that may be measured by inches, nevertheless it was followed by a subsidence.

Surely the force producing this subsidence was as independent of the coal seam as that producing the previous upheaval was independent of the limestone.

Throughout the whole series of the coal measures comparatively thin accumulations of coal, representing periods of rest or perhaps slow elevation, were followed by prolonged periods of subsidence.

I do not think the depressed areas bounded by vertical cliffs seen by Mr. Gardner in Iceland at all help his theory; they are just the phenomena one would expect to find in a highly volcanic district; on a very small scale they may be seen in any district from under which material has been removed.

T. SINGTON

Grove Terrace, Kersal Moor, Manchester, September 22

I FEAR readers of NATURE must be weary and the courtesy of the Editor taxed by the demands on its space. Moreover but little actually new information has been elicited, though thanks are due to the Rev. Osmond Fisher and Dr. Ricketts especially for their contributions.

That depression of the earth's crust follows on the addition of weight and elevation ensues on its removal are facts that can no longer be disputed or explained away. Those gentlemen, however, who object that the cause and effect do not follow each other foot by foot are a little unreasonable, for resistance more or less stubborn must be encountered, which may check the process for a space, and then by yielding at last considerably accelerate it. Those again who will see nothing in the array of facts beyond fortuitous connection must be allowed to hold their opinion.

The matter rests thus:—The observations of many authors have induced a belief that sedimentation causes subsidence, through the increase of weight acting on and displacing a viscid layer underlying the solid crust. If this is so, the displaced matter must find room elsewhere, and it is only reasonable to suppose that a slight elevation or bulging of the crust must result in more or less adjoining areas, and chiefly under areas in which denudation had already weakened the resisting power by reducing the pressure. Applying the idea to coasts, where we have for the most part parallel lines of denudation by wave action and sedimentation through the deposition of material dislodged by the waves, I have endeavoured to show that their chief physical features accord with the "elevation and subsidence" theory, though more observations are greatly to be desired. But, even including coast lines, the examples are so far but local manifestations, yet, if true in less matters, why not in greater? Oceanic basins, if permanent throughout geological ages, as they probably have been, must have been areas of sedimentation on a stupendous scale, and the pressure they exert (increased as subsidence deepens the column of water) must give rise to corresponding displacements on a gigantic scale, and which would seek relief along the nearest existing lines of weakness. These lines would either be in the ocean and result in banks or ridges, or else be along their margins and result in mountain chains, and sometimes breaking through in volcanic outbursts. The larger would overcome the smaller, and a delta or coast line subsiding through its own sedimentation might occur along the line of upheaval and be forced upward, or a vast displacement or eruption might relieve the tension to an extent that would take ages of accumulation to reproduce. This

theory seems to me to be natural, and to accord with facts all round, but still it may be wrong. Those, however, who would assign all elevation and subsidence to secular cooling and tangential thrusts through shrinkage are revelling in their own imaginations, for there is no reason why the earth's nucleus should not have cooled as evenly as a cannon ball or piece of pottery, or other homogeneous body; and the records of the Palæozoic rocks, when we may suppose shrinkage would be more active, certainly show that its surface was then relatively level, and without deep seas or great elevations on land.

J. STARKIE GARDNER

P.S.—A good example of subsidence may be seen in the Tilbury Dock works in progress. So far as I could see, Thames mud is being cut through to a depth of some ninety feet, the upper part at least being filled with debris of reeds interstratified with peaty matter or decayed reeds massed together. The whole must have been deposited at or near high-water level, and so recently that at thirty feet depth the decayed vegetable matter still smells offensive.

The Apparent Disappearance of Jupiter's Satellites on October 14

RAIN fell without intermission on the afternoon and evening of October 14, but at 11h. it ceased and the clouds broke. Later in the night the sky cleared, but there were showers at short intervals.

At 15h. 15m. I observed Jupiter with a 10-inch reflector, power about 212, and saw that the third satellite was the only one visible. It was situated close to the east limb, and its disk appeared somewhat faint, as if much clouded over with spots.

15h. 55m.—The third satellite is entering upon the planet's disk at a point in the same latitude as the upper side of the great south equatorial belt. The fourth satellite is also seen coming off the west limb. It looks remarkably faint. At this time the configuration of the planet was extremely interesting with the two satellites hanging upon the limbs.

16h. 0m.—The fourth satellite appears to have completed its egress, and is evidently much in advance of the time given in the *Nautical Almanac*.

16h. 15m.—The first has now reappeared from occultation at a point in the same latitude as the north equatorial belt. This belt is a far more prominent feature than during the last opposition.

16h. 19m.—A large white patch on the planet's equator is crossing the central meridian. On its north side the equatorial belt is very dark.

16h. 30m.—The third satellite is visible as a very dark spot projected upon the south equatorial belt, which is the darkest belt upon the planet.

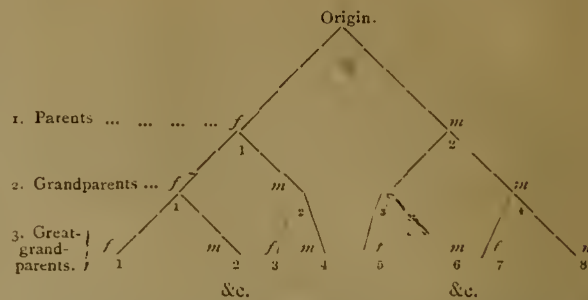
17h. 0m.—The second satellite is seen as a bright spot on the interior margin of the west limb, and will shortly begin its egress. It has crossed Jupiter in a latitude corresponding with the equatorial edge of the great south belt. The third satellite is now perceptible as a black spot pursuing its course along the south belt. The chief condensation of shading apparently lies on the south side of the satellite, but the telescopic image is not satisfactory.

The disappearance of the satellites on this occasion can hardly be said to have been complete, for at no time were they all included within the margin of Jupiter. While the third entered upon the disk the fourth released itself, and the two formed a curious configuration hanging upon the limbs. The third and fourth satellites were extremely faint when clear of the disk, and their surfaces are evidently very feebly reflective compared with that of their primary. It is significant that the third, though projected upon the darkest belt of Jupiter, was visible as a black spot. The fourth probably crossed the planet as a black spot also, though I made no attempt to distinguish it under this aspect, owing to frequent interruptions by clouds and rain.

Bristol, October 15 W. F. DENNING

Arithmetical Notation of Kinship

IT seems to me that the elegant arithmetical notation for ancestors proposed by Mr. Galton in his recent letter to NATURE (September 6, p. 435) may be further simplified. The modification consists in first counting the grades and then counting the species of the grades, as shown in the following diagram:—



Thus mother of mother of father is of the 3rd grade and of the 4th species, and may be denoted by 3, 4. Let q denote any grade and r any species, then q, r is a complete specification for an ancestor. The rule for analysing such a specification is—Divide r by 2, adding a unit when the dividend is odd, and repeat the operation $(q-1)$ times; then when r or a quotient is odd, substitute *father*, and when even *mother*.

Take, for example, 3, 5. We get 5, 3, 2, hence father of father of mother. Take 5, 5, we get 5, 3, 2, 1, 1, hence *f f m f f*.

If we compare together the ancestors of the same species number, we shall find that they have all the figures the same until we come to unity, and that the generic difference depends on the number of unities. The truth of this may also be seen by considering the mode of construction of the diagram.

Mr. Galton's 253 is, in this notation, 7, 126. Analysing we get—

126	63	32	16	8	4	2
m	*f*	*m*	*m*	*m*	*m*	*m*

The ancestor 7, 1 of my notation is 128 in his. The analysis of the latter is—

128, 64, 32, 16, 8, 4, 2,

while that of the former is evident by inspection, namely—

1, 1, 1, 1, 1, 1, 1.

With the double notation we know that when we come to unity, each of the following symbols must be f; and that when we come to a power of 2, each of the following symbols must be m, followed it may be by some f's.

ALEXANDER MACFARLANE

4, Gladstone Terrace, Edinburgh

A Green Sun

IN connection with the phenomenon recorded in your last number (p. 575), the following extract from my journal (Sunday, July 14, 1878), when just north of the Arctic Circle on board the *Jonas Lie*, may be of some interest:—

"To-night the sun sets in a sky as pure and cloudless as that of yesterday; but the colours are quite different. Now there is no crimson, but in its place orange, yellow, and molten gold. All this exquisite beauty of colour is limited to a particular part of the sky, and that not the west, but the north. Yes, strange as it may seem, the sun sets scarcely a single point from the north, and rises again nearly in the same place, barely two points apart. Some heights are lighted up with the glow, but for most of the time all around, save in the bright, favoured north, is cloudless darkness and gloom; which yet is not the darkness of night, but a grim, stormy, vague gloom in broad daylight. The afterglow that follows sunset dies out, and without any sensible interval of time, revives nearly in the same place; the colour brightens, and some small streaks of clouds grow brighter and brighter, until the sun—the GREEN sun—appears. A distant low range of rocks comes between us and its point of rising, and, as we glide on, an opening between them shows us the sun, *a bright emerald*, as pure and brilliant as ever gem that glistened; again we lose it, and again an opening shows it to us in its own golden light; and then once more it is the bright green: and now it rises higher, clears the ridge, and is once more the golden orb. This is what we saw, but another observer assures us that when first he saw it, the colour was a fiery red, which soon turned to green. Probably an optical effect of what is called polarisation of light, as these complementary colours seem to show." ('A Long Day in Norway," published in *The Month* in 1878-9.)

HENRY BEDFORD

All Hallows College, Dublin, October 13

"Zoology at the Fisheries Exhibition"

IN your issue of the 20th ult. (p. 489) a direct challenge is made upon several points as to the veracity of my former letter. "The Writer of the Article" states what he terms certain "facts,"—the first being that I informed the jury of Class V. that certain corals under my name, 813 *b*, were in the case with Lady Brassey's corals, and formed part of that collection. I beg entirely to deny this What I stated to one of the members of the jury in answer to his allusion to my exhibit was, that owing to my being so busy I was unable to exhibit my own corals, *and that all my energies had been thrown* into the arrangement of Lady Brassey's case. Whoever informed "The Writer of the Article" must have greatly misunderstood my statement. The second "fact" with regard to the opinion of experts I think I need not answer. Opinions may differ. With regard to "fact" three, that neither the series of corals in the British Museum nor those of the *Challenger* Expedition have been accessible for purposes of examination for some considerable time. It will be sufficient to say that my description and figures of Lady Brassey's corals were published in the *Annals and Magazine of Natural History*, vol. ix. No. 50, in February, 1882, some time after the publication of the volume of the *Challenger* Reports containing Mr. Moseley's monograph of the "Hydrocorallinidæ," *and a considerable time after the specimens themselves were exhibited to public view in the galleries of the British Museum.* Those specimens, together with the other corals of the general collection of the British Museum have only been withdrawn from public view within the last few months.

BRYCE-WRIGHT

Organic Evolution and the Fundamental Assumptions of Natural Philosophy

BY the principle of heredity we understand a tendency in an organism to reproduce in successive order the variations which appeared in its ancestors, the leaning being on the whole to fixity of direction: by the principle of variation the tendency of an offspring to vary in a degree more or less from its parents. Included in heredity, I suppose, are the tendencies to vary, as well as the actual succession of changes in the life-history of the ancestors of the organism: those tendencies to vary being conceived of as series of which some terms may modify or counteract others. If I am right in this description, it appears to me that heredity is an example of inertia acted on by a principle analogous to the first law of motion, and very closely allied with it, if indeed it is not the case that the laws of motion are special cases of wider principles or laws. Following the same line of thought, "variation" may be explained as action analogous to that of forces on a body drawing it from its straight line of motion or its rest. The forces (if I may use that term) which thus cause variations in any particular organism would be:—

(1) The resultant of difference of conditions cooperating with
(2) The resultant of inherited tendencies.

Again, answering to the second law of motion, we may perhaps assume that degree of variation is proportional to the forces acting, namely, proportional to the intensities of (1) and (2) above and to the correspondence or divergence of direction in which they tend. If the above hypotheses are accurate, we have, I think, an explanation of the possibility of protozoa, &c., remaining practically unchanged during great changes of conditions. For those genera which show a tendency to great variation in the individuals at the same time seem to present no fixed line of tendency. The result of heredity (as defined above) in these cases is not definite variation tending in certain fixed directions, but great individual or indefinite variability. And their phenomena of reproduction I think account in some degree for this.

On the same hypothesis we may explain, amongst other things, the plasticity of organisation of cultivated plants. One of the conclusions also that would follow would be that "reversion" would not happen except as a resultant of the tendencies in fixed directions having nullified each other, or having converged in the direction of reversion.

To carry the analogy further. Answering to the third law of motion, that action and reaction are equal and opposite, we may perhaps assume that alteration of some organs or properties in the organism, wrought by change of conditions together with heredity, brings about that modification of other properties or organs which Darwin spoke of under the name of "correlation."

I should feel indebted to any one who would point out any mistake that I have made in this, or would show if the assumptions I have made are untenable.

FREDERICK W. RAGG

Masworth Vicarage, Tring, September 25

Curious Habit of a Brazilian Moth

MR. E. DUKINFIELD JONES (NATURE, May 17, p. 55) may be interested to learn that the habit of *Panthera apardalaria*, which he describes, of sucking up water and discharging it simultaneously *ab ano*, is not confined to the moth noted by him, but is common to several other lepidopterous insects.

I have watched several species of *Callidryas* and *Pieris* both in Ceylon and Brazil, doing the same thing; also *Papilio Erithonius*, Cram, in the latter place, and its ally, *P. Demoleus*, L., at the Cape of Good Hope. I have seen the "white butterflies" in thousands, settling on the mud and damp places in the jungle paths, in all these countries, with their trunks thrust into the soil, drawing up copious supplies of water as if they were so many miniature Abyssinian pumps! The drops ejected were by no means "minute," were rather very large, and I should say 60 would much overtop the marks in a minim measure glass, or, roughly speaking, more than fill an ordinary teaspoon, said to equal sixty drops. I have somewhere (I forget where), among my fugitive writings, noted this fact: stay, I noted it once at least in the *Field* in 1873, February 7. A large moth (*Catocala?*), a fine "yellow underwing," visits the toddy vessels in Ceylon, and gets very drunk, not having joined the "blue ribbon" army, nor having the fear of Sir Wilfrid Lawson before his eyes! I suspect this fellow finds the liquor too good to eject so rapidly, but I have seen small moths of various kinds drinking ordinary sap, and ejecting it like the before-named butterflies.

British Consulate, Noumea, July 24 E. L. LAYARD

Meteors

DURING the month of August I watched the meteors from Logiealmond parish manse, Perthshire, but there the weather was most unpropitious for observing any celestial phenomena, so only a few were seen; whereas there in August last year I witnessed some gorgeous showers of brilliant meteors, which the readers of NATURE may recollect. Here in Paisley, on the last Sunday of August, about 11.50, a very large meteor blazed from due south to due east, with a long train, lasting for a minute, and keeping parallel to the horizon, nearly midway to the zenith. The month of September was unusually rich in meteors, but they were generally small and transient, and seemed to be very remote, some of them dashing straight up. A bright but momentary one shot up at right angles from the Pleiades. On September 24, at 10.55 p.m., a very peculiar meteor started from Aldebaran, and exploded in the head of Pisces, under the Square of Pegasus. It was dimly seen through a haze, with a long streak behind, giving unmistakable evidence of its large size; yet, though scarcely seen above its path, brightly shone the Pleiades, Aries, and the Square of Pegasus. On the 25th, at 11.56, a similar one, scarcely seen, sailed through a haze from Pisces to Altair, exhibiting a long trail of broad light, showing that it was a large one. September 30, at 0.12, a meteor considerably larger and brighter than Jupiter passed from the third bright star in Auriga (taking Capella as the first), and exploded close to the lower Pointer in the Plough, leaving a long train of reddish light behind it. On the night of the 29th and morning of the 30th, up to 4, the meteors were unusually large and of longer duration, and altogether more numerous than I have ever seen them in September. A few momentary meteors were seen on the night of September 30 and morning of October 1

DONALD CAMERON

Mossvale, Paisley, October 1

The Uselessness of Vivisection

YOU will probably permit me to point out that it would be only reasonable for Mr. George J. Romanes to possess a little information on the subject he is dealing with before he accuses another of being "in a quagmire of ignorance and inaccuracy." The pamphlet against which he levels his abuse does not deal with physiology, but with surgical questions. My efforts have not been directed so much against vivisection as against the mendacious statements which have been made concerning advances in surgery alleged to be due to its practice. These have been used to hoodwink the legislature and blindfold

the public, and they deserve the utmost condemnation. If physiologists can make out a case for themselves, I for one am prepared to give it the utmost attention, but they must not bring to their aid false illustrations from a branch of science with which I think I may be permitted to say I have had a large experience. LAWSON TAIT

Birmingham, October 6

IT seems sufficient for me to observe, in reply to the above, that before writing my review of "Physiological Cruelty" I took the trouble to acquaint myself thoroughly with the latest edition of Mr. Tait's pamphlet. GEORGE J. ROMANES

Breeding of "Hapale jacchus" in Captivity

MR. MOSELEY's Marmosets (NATURE, vol. xxviii. p. 572) are by no means the first instance in Europe, or even in England. Edwards, more than a hundred years ago, recorded a case in Portugal; and Frederic Cuvier had three born in Paris in 1819 (*vide* Sir William Jardine's Natural Library—Mammalia, vol. i.). A relative of mine brought a pair of this species from Pernambuco in 1863, and kept them in his kitchen at Surbiton. In April, 1865, I was shown two living young ones which had been born a few days before. In the *Proc. Zool. Soc.* for 1835, births of marmosets of an allied species (*H. penicillatus*) have been chronicled as occurring in this country. W. C. ATKINSON

Streatham, S.W., October 13

TELESCOPIC WORK FOR THE AUTUMN

WITH Mars, Jupiter, and Saturn so favourably visible in the sky during the ensuing autumn and winter months, we think it may be interesting to call attention to some of their more prominent features, and to ask amateurs and others who devote themselves to the attractive field of planetary observation to make a combined effort, not only to substantiate such facts as are already known with regard to the physical appearances of these bodies, but to endeavour to glean something new concerning them. For, notwithstanding the diligence with which these planets have been scrutinised in past years and the many curious facts that have been brought to light, it must still be confessed that there remains much to be done. Our knowledge is admitted to be extremely incomplete. The powerful instruments of the present day do not seem capable of rendering us efficient aid in this respect; indeed we shall find by a comparison of results that we owe most of our discoveries to telescopes of moderate aperture. The real explanation probably is that, with increase of aperture, definition, especially of the brighter planets, becomes less perfect. Faint markings are obliterated or seen unsteadily and uncertainly in large instruments owing to glare, the difficulty of getting a sharp, hard disk with so much light, and the constant undulations of the atmosphere. With moderately small instruments the conditions are in many respects more favourable. The image is sharply defined, and though the quantity of light may be somewhat deficient, there is an absence of glare and of that atmospheric interference which are inseparable from large apertures. Moreover, the eye is more capable of prolonged observation and is enabled to glimpse the faintest details on an image of moderate intensity. The deficiency of light in small instruments is therefore to some extent a recommendation when it is accompanied with extreme sharpness of definition and when the amount collected by the object-glass or speculum is sufficient to allow a power to be used which displays a fairly large disk without destroying the quality of the definition. Indeed one great desiderative in such cases is to utilise light and power in agreeable proportion, for this is a very essential requirement, which is, however, often neglected, and is frequently the source of disappointing experiences. Amateurs who are careful to consider these matters will be enabled, though their instruments may be of comparatively small reach, to do much useful work in many departments of observation, and particularly in that relating to planetary markings.

With regard to Mars, high powers are very requisite because of the small diameter of the planet. Hence a fairly large aperture is necessary, for, unless the disk is considerably expanded, it is impossible to trace the chief features satisfactorily. In the case of Jupiter the use of high magnifying powers does not apply with so much force, the apparent diameter of the disk being greater. But this planet is a somewhat difficult object to define satisfactorily. The best telescopes will often fail to show the contour of the disk with desirable sharpness. Hence it is that this object with large apertures is troublesome and to some extent disappointing. This is certainly the case when we consider how efficiently and successfully small instruments perform upon this planet, and with what readiness the faintest and more minute of the details are distinguished. As to Saturn, the conditions are somewhat different. Here there is less light and the telescopic definition is better, so that large glasses possess an undoubted advantage.

The ensuing opposition of Mars is not a favourable one, but many of the most interesting and now well-known features of the planet may be observed in good instruments. The curious network of "canals," as discovered by Schiaparelli, and their duplication, as seen by the same observer during the last opposition, in the winter of 1881-82, should be looked for, as some doubts have been expressed as to the reality of these phenomena. The question is naturally asked, How is it that they are now seen with so much distinctness again and again with a refractor of only eight inches aperture, when large instruments have utterly failed to reveal them? Schiaparelli, it is true, works in a climate highly favourable to such delicate work, and his telescope, though comparatively small, is yet of the finest possible quality. But even with the prevailing conditions so eminentlyconducive to the attainment of such important results, it must still remain matter for surprise that, as the celebrated Italian astronomer himself put it, "the greater number of canals and of their pairs were observed with comparative ease whenever the air was still, and only a few cases required a special effort on the part of the observer."

These so-called canals appear from Schiaparelli's charts to be very narrow dark markings, running generally in straight lines, and often intersecting each other so as to constitute a perfect network about the equator and in the region south of it. Many of these lines were seen to be double in January and February, 1882, and the inference is that, as these duplications had escaped observation during the more favourable opposition of 1879, they are subject to periodical variations, or in any case represent phenomena of temporary character. They undoubtedly exhibit a most extraordinary arrangement, and such as naturally to call forth some amount of objectional comment from those who, though familiar with the telescopic aspect of the planet, have never seen it as Schiaparelli depicts it. In fact his delineations give a boldness and definiteness of outline in the smallest details which no other observer is able to corroborate. The extreme delicacy of shading and softness of outline so characteristic of many of the features of this planet as displayed in our best telescopes seem wholly wanting, and we have presented to us an elaborate complication of hard, dark lines which bear little analogy to our own impressions.

It has been suggested that many of these so-called "canals" are the edges of half-tone districts on the planet, and possibly this may be so in certain cases. But we must not forget that the eminent author of these important discoveries expresses himself very confidently as to their existence, for he has seen them repeatedly, and at times when the conditions were not favourable to the detection of such difficult markings. Probably something

of them may be again observed during the ensuing winter, although the apparent diameter of the planet is very small and opposed to a critical investigation. Schiaparelli has, however, pointed out that "on January 1, 1884, the position of Mars with respect to its solstice will be identical with that which it had on February 13, 1882, and its apparent diameter will be 13″, that is to say, about equal to the apparent diameters which the planet had during the discovery of the parallel canals. Every telescope able to distinguish a dark line 0″·2 in breadth on a bright ground, and to separate the one from the other two such lines when the interval between axis and axis is 0″·5, could be employed for these observations and to study the duplications if these should be reproduced."

It is to be hoped that some of our best instruments will be devoted to this work during the ensuing winter, and that something of these curious features will be observed before the favourable opposition of 1892, when they will certainly be seen if ever. But there is the alternative that they are possibly variable and subject to periodical disappearances, though we can hardly consider this probable in view of the permanent character of the chief markings on the planet.

Jupiter has always been an object of great attention to the possessors of telescopes, not only on account of the readiness with which the satellites may be perceived, but also on account of the prominence and variety of his belt scenery and the marked changes which it undergoes from year to year. The well known red spot which appeared in the planet's southern hemisphere, and which first came prominently into notice in July, 1878, has proved a great stimulus to observation of late years. The large dimension of this spot, its intensely red colour, its definiteness of outline, and its durableness have combined to render it an object of extreme interest, and the phenomena of this planet, whenever it is described in future years, will never be complete without a reference to this marvellous feature. Persistently visible throughout a period exceeding five years, during which it has completed more than 4500 rotations, it would be imagined that it must afford an excellent means of fixing the rotation period of Jupiter with a degree of exactness far surpassing all previous efforts. But the spot has shown a retarded motion which constantly causes it to lag behind its predicted place. In other words, the rotation period has been lengthening. Mr. Marth found the average time to be 9h. 55m. 34·47s. from the observations between 1878–1881, but the spot now crosses the central meridian of Jupiter about two hours after its predicted times based on the period just referred to. In fact its time of rotation has lengthened fully three seconds during the past two years. But even had this spot been influenced by a perfectly accordant motion during the whole period that it has been watched, we could not regard its time as showing the true length of the Jovian day, for the disk of this planet exhibits a variety of spots which generally move much swifter than the red spot. On the equator the markings rotate in 9h. 50m. 7s. Both the dark and bright spots which alternate with each other on and near the equator, and are included in the two principal dark belts, participate in this rapid movement, and they show a fairly regular period, so that it is impossible to decide at present as to the true rotation of the planet's sphere. The brightest spot of all has been attentively followed since November, 1880, and it is found that relatively to the red spot it has, owing to its greater velocity, completed twenty-three revolutions of Jupiter. In other words, during 1026 days its swift proper motion has enabled it to sweep round, no less than twenty-three times, the vast circumference of the Jovian sphere! What phenomena can possibly have given rise to such a remarkable difference of motion in objects of fairly permanent character? The explanation is involved in mystery, and may never be forthcoming, but there can be no question as to the importance of following up these observations. The red spot now seems unfortunately to be in a state bordering on extinction. It has become so faint that it is only distinguished with care under circumstances of favourable definition.

Apart from the spots, which evidently offer a large field of very interesting work, there are the belts, which are constantly varying in colour, position, number, and intensity. At every opposition of the planet a series of sketches should be made of the aspect of the disk, for some important issue may result from the comparison of such drawings if existing over a long number of years. Some of the leading features may be found to recur at certain definite periods, for the drawings of the present day show many curious forms bearing a striking analogy to some observed at former times. In any case a reliable set of sketches for each opposition must give us an excellent basis for investigating the varying features of this planet, and may afford us a clue to some of the marvellous changes evidently progressing upon his surface. Telescopes of very moderate size can be usefully employed in these observations. I have seen sketches of Jupiter made with a 36-inch reflector, 18½-inch refractor, 18¼-inch reflector, and with many other instruments ranging from 12 to 18 inches. I have also examined a large number of similar sketches obtained with refractors of from 4¼ inches to 6 inches, and reflectors from 5¼ inches to 6½ inches, and carefully compared them together. The large instruments appear to have had no advantage whatever. Indeed, judging from the amount of detail presented in the sketches, and from the descriptions accompanying them, the small apertures would seem to have rather the best of the comparison!

One explanation may be that the detail rendered visible on Jupiter by large instruments is so extensive that it cannot adequately be delineated during the short interval available for sketching. The features change very rapidly, owing to the planet's swift axial rotation, and drawings made on different nights, when the longitudes are assumed to be coincident, are not reliable, because the different markings have become severally displaced owing to their differences of motion.

Saturn, though diversified with belts similarly to Jupiter, is less attractive as an object for telescopic study. The image of Saturn is beautiful as a picture, but there is a sameness about it which observation, renewed again and again, has a tendency to make monotonous. His belts are generally faint, and seldom show spots of decided character. During the last opposition the planet's southern equatorial belt was very dark and well defined, and exhibited differences in depth of shading, but these were not sufficiently distinct to be followed. On the equatorial margin of this belt the disk was very bright, and immediately contiguous to it are signs of white spots, which would suggest very similar phenomena to that discerned on Jupiter. Spots of sufficiently definite outline to be observed with certainty upon the globe of Saturn are extremely rare. Prof. Hall's white spot of 1877 affords one notable example of this kind, and doubtless the number of such observations would be greatly increased were more attention directed to this planet. Though sketches of Jupiter are frequent enough, we rarely see attempts to delineate Saturn, and thus we have comparatively few records as to the arrangement of his belts. This is to be regretted on many grounds. No doubt the rings of this planet monopolise observers to such a degree that they neglect other phenomena which, though less attractive, may be of greater significance and interest. Whenever such observations are practicable, particular attention should be given to the configuration of the belts, and a searching examination made for any definite markings which may be sufficiently obvious and permanent to be followed on successive nights, and thus enable a new determination of the rotation period. William Herschel found this to be 10h. 16m. 0·44s.,

and this has received excellent confirmation from Prof. Hall's observations in 1877, which indicated a period of 10h. 14m. 28·8s. Schrœter also glimpsed several spots indicating periods of about twelve hours, but these observations are probably erroneous, as they differ so widely from the corroborative results of Herschel and Hall. But it is possible that these distinctive markings on Saturn give anomalous periods similarly to the spots on Jupiter, though hardly to the extent of the differences between the existing observations. Were this planet more sedulously observed, it is certain that we should obtain some new and interesting facts with reference to his globe and rings. As to the belts, they are occasionally very plain; Grover has seen them with only 2 inches of aperture, and the writer distinguished them well in 1881 with a 2½-inch O.G. What, therefore, can be so readily seen in small telescopes ought to come out with considerable detail in the large instruments of the present day. As to the system of rings, it forms a complicated object for study. The marked differences in their tints and brightness should be recorded on all occasions. Cassini's division is always plain, but the outer and more minute division of Encke has sometimes defied the power of our best telescopes. It apparently varies both in position and intensity. Other faint subdivisions are sometimes traced, but they are very difficult objects, and seldom seen with certainty. The crape or gauze ring, together with other details, such as the anomalous shadow of the ball upon the rings, supply an endless store of curious appearances requiring further elucidation.

There is no doubt that, notwithstanding the mass of interesting facts gleaned in past years respecting the physical aspects of Mars, Jupiter, and Saturn, there remain many novel features to be distinguished, and many new facts to be descried. Observers, therefore, who make these observations a specialty should endeavour not only to confirm former results, but to make some advance upon our existing knowledge.

We have not space in the present paper to refer to the satellites of these several planets, but may possibly be able to do so on a future occasion. W. F. DENNING

THE INTERNATIONAL BUREAU OF WEIGHTS AND MEASURES[1]

II.

HAVING now given a description of the instruments used in the section of linear standards, we come to the apparatus belonging to the section of standards of weight.

As regards the essential instrument connected with the process of weighing, the International Bureau possesses one of the most remarkable collections of balances of precision existing in the world. Of these the principal ones have been constructed by the house of Rüprecht of Vienna The large engraving accompanying this (Fig. 3) represents in its entirety the spacious chamber in which they are set up. In addition we gave a special sketch (p. 465, Fig. 2) of the balances principally designed for comparison of standard kilogrammes. This balance is constructed in such a manner as to adapt it for being worked at a distance, whereby it is kept free from the disturbing influence always occasioned in the process of weighing by the proximity of the observer, in consequence of the variations of temperature which his presence close to the balance gives rise to. In the case of the instrument now in question, the observer, having prepared his weighing apparatus the preceding day,—that is, placed the weights he will have occasion to use in their right positions in the pans of the balance,—avoids any longer going near the balance. Standing in front of his telescope he performs all the operations involved in weighing that is, he puts the weights on the pans, releases the pans, and then the beam, and measures the oscillations of the beam; then changes the weights from one side to the other, placing that to the right which was at the left, and conversely, &c., the whole at a distance of four metres. For that purpose the balance is provided with a mechanism very ingenious and of perfect precision which works automatically by means of handles fixed to the extremity of long rods. The oscillations of the beam are read by the reflection of a graduated scale on a mirror borne by the beam; it is the image of that scale the observer sees displacing itself slowly in his telescope while the balance oscillates. He notes a certain number of successive oscillations, and thence calculates the position of equilibrium.

[1] Continued from p. 466.

Three other balances, of the same model but smaller, are intended for comparisons and adjustments of lighter weights. They have the same mechanism of transposition, only in the case of the two smallest, a little more simplified and less complete. In the centre of the large engraving (Fig. 3) are seen the large arms of the lever which allow the weighings to be made at a distance. These arms rest on three stone pillars, above which are placed the telescopes for reading the oscillations of the beam.

The following are some details of the mechanism of transposition employed in this balance. The pans of the balance have a shape altogether peculiar. Each is formed by a circular piece open at one point and extending itself inwards by four triangular plates or teeth directed towards the centre of the balance. A cross piece placed underneath can be passed between these plates. Suppose now that weights, say of one kilogramme each, are placed above the pans on each side; and to make the idea the more definite let the kilogramme A be on the left scale and the kilogramme B on the right. Taking hold of one of the four handles under his hand, the observer sets the mechanism in motion. This is what happens:—the cross piece placed under the pan mounts at first, ascends beyond the plane of the pan and consequently lifts the kilogramme resting above it. Arrived at a suitable height the cross piece shifts its place laterally, and disengaging itself from the pan it gradually gets deposited above one of the plates, attached right and left to the pillar of the balance. These plates follow an arrangement analogous to that of the pans. By continuing the movement the cross piece then commences to descend, and traverses the plane of the plate, depositing on it the kilogramme which it has raised from the scale. While these movements are effected to the left in the case of the kilogramme A, they are in simultaneous accomplishment to the right in the case of the kilogramme B. The two weights to be compared are thus transferred at the same time to the central plates. Then taking hold of a second handle the observer turns the two plates 180° round the axis of the pillar of the balance, a movement which shifts the plate which was at the left to the right, and conversely. All that is needed then is to cause the same evolution to be gone over again with the crosses which they have already done, but inversely, in order to bring back the kilogrammes to the scales of the balance; the kilogramme A is then at the right, the kilogramme B at the left, and the observer can proceed to the second part of the weighing.

The two other handles control—one the movement serving to release the pans, the other the movement lowering the fork and setting free the beam.

For a long time it has been known that the balance is an instrument of precision *par excellence.* By means of those here in question the minimum amount of error in the process of weighing has been reduced to an almost infinitesimal degree. The difference of two kilogrammes, for example, can by them be determined to a nicety reaching to the hundredth of a milligramme, that is, the weight of a kilogramme is ascertained down to within a hundred millionth of its absolute value.

In another part of the hall is shown the hydrostatic

balance serving to determine densities; and here, again, details of operations have been perfected up to the last limits compatible with the actual state of science. The water which is to be used for hydrostatic weighings, having been once distilled by an ordinary still, is re-distilled by means of an apparatus of platinum, and then re-collected in a platinum vase. The latter is placed under the balance employed for the weighings, and by a series of ingenious apparatus the weights can be plunged in the water and all the manipulations performed,—manipulations often very delicate, but indispensable if all possible chances of error are to be reduced to a minimum.

The weighing section contains, besides, a beautiful collection of weights,—in platinum, iridium, and in quartz for weights of the first class, and in gilt brass for weights of the second class.

Irrespective of the fundamental apparatus we have just mentioned, the bureau possesses a large number of different instruments; some intended for certain special labours, others necessary for accessory processes in close connection with operations of comparisons or of weighings. Among the former, one of the most remarkable, by reason of the exceeding delicacy of the method it sets in operation, is the Fizeau apparatus, by means of which expansions in small standards, or fragments of some millimetres in thickness, are measured by applying an optical process founded on observation of the phenomenon of the interference of light. This apparatus enables variations of distance between two points to be determined and measured down to some millionths of a millimetre.

The accessory instruments are cathetometers, spherometers, meteorological instruments, barometers, thermometers, hygrometers, &c. Fig. 4, for example, shows the normal barometer of the section of weighings, a splendid instrument combining all the most perfect contrivances for the measurement of atmospheric pressures with the utmost possible degree of accuracy.

The measurement of temperature plays an essential part in all operations which have to be performed with standards either of length or of weight; the studies in connection with the thermometer have likewise a place so important that they may be regarded as constituting a section by themselves, with instruments peculiarly their own.

The measurement of temperatures does not form a separate section in the International Bureau, but the importance of the operations connected with it and the precision of the apparatus employed for this end entitle it to a special description.

The air thermometer depends on a remarkable principle, the knowledge of which science owes to the classical experiments of Regnault on the expansion of gas. The illustrious physicist has in effect demonstrated that the increase of tension which gas suffers when heated while its volume is kept constant is sensibly proportional to the temperature.

It is easy to conceive that Regnault utilised this property for the measurement of temperatures. The first air thermometer of precision constructed by him consisted essentially of a glass globe filled with air, connected by a capillary tube with one of the arms of a mercurial manometer. Special contrivances allowed the mercury to be constantly maintained at the same height in this arm, while the globe was exposed to different temperatures. The tension of air at each temperature is then measured by the height of the mercurial column balancing it.

This instrument, generally employed by experts, has subsequently undergone numerous modifications, the most of which aim at imparting greater precision to the measurement of pressures. This measurement, which consists in determining the difference of the height of the mercurial level in the two arms of the manometer, really presents great difficulties, seeing that tubes of large dimensions are employed for the manometer, a condition necessary to avoid the capillary depression of the mercury. The surface of the mercury in the large tubes presents a plane superficies, so smooth that it is impossible to distinguish the level of the mercury when it is viewed horizontally. To attain this end, one makes use of movable points, bringing them gradually close to the surface. Observing, then, in a telescope the level of the mercury, the point is seen drawing nearer and nearer its image; the instant of contact between the movable point and its image indicating exactly the level of the mercury.

The apparatus represented in Fig. 4 is the one which has been constructed at the observatory of the International Bureau. The arrangement we have just spoken of has been adopted as much for the sake of the readings of the manometer as of the measurement of the atmospheric pressure in the normal barometer, B B. In the part to the left of the diagram, one distinguishes in A the arm of the manometer which is connected by a capillary tube with the globe placed in the interior of the warming apparatus. The latter has been placed in an adjoining position, in order not to expose the measuring apparatus to variations of temperature.

Having determined the normal positions of the instruments, one can proceed to the comparison of the mercurial thermometers, *t t*, the reservoirs of which are placed in the warming apparatus in proximity to the globe, A, of the air thermometer. The observations consist in reading on one side the temperature indicated by the mercurial thermometers, and on the other in measuring the difference of the mercurial level in the two arms of the manometer; the open arm of the latter experiences the atmospheric pressure, the manometer merely indicating the difference between the atmospheric pressure and the tension of air inclosed in the globe. To get the total pressure balancing the tension of the air in the globe, the barometric pressure must be added to that indicated by the manometer. The measurement of the pressures is effected by means of three horizontal telescopes, movable vertically through the length of the upright fixed to the pillar shown on the left side of the diagram. This upright is able to turn on its axis. Having adjusted the level of the mercury, one can turn the telescopes, without deranging them, round this vertical axis in such a manner as to read the graduations on the scale attached to the adjacent manometer. The telescopes fixed on the upright form to some extent, a pair of beam compasses, as they allow the difference of the mercurial level in the arms of the manometer to be transferred to the scale serving to measure it.

These instruments are easily adequate to the measurement of a hundredth of a millimetre. In the ordinary conditions of experiments three hundredths of a millimetre correspond with a variation of temperature of one hundredth of a degree Centigrade. To maintain the temperature constant within these limits, the steam of different liquids, such as water, ether, methylic alcohol, ordinary alcohol, the ebullition of which takes place at continuous temperatures, is used with advantage. The regular ebullition of these liquids being one of the conditions essential to the constancy of the temperature, the arrangement indicated in the diagram has been decided on. A vessel, *a*, placed in a water-bath, *c*, contains the liquid, the steam of which escaping by the tube *x x*, penetrates into the double cased heating apparatus. Having traversed all the parts of the apparatus, including the glass tubes in which the mercurial thermometers *t t* are placed, the steam issues by the tubes *y y* to liquefy in the condenser R, whence the liquid returns to the vessel by the tubes A. Other tubes serve equally to return to the vessel so much of the steam as becomes condensed on the way or in the apparatus, so that the same quantity of liquid will serve for a sufficiently long time. The water-bath *c*, which supplies the heat necessary for keeping up the boiling of the liquid employed, is itself heated by

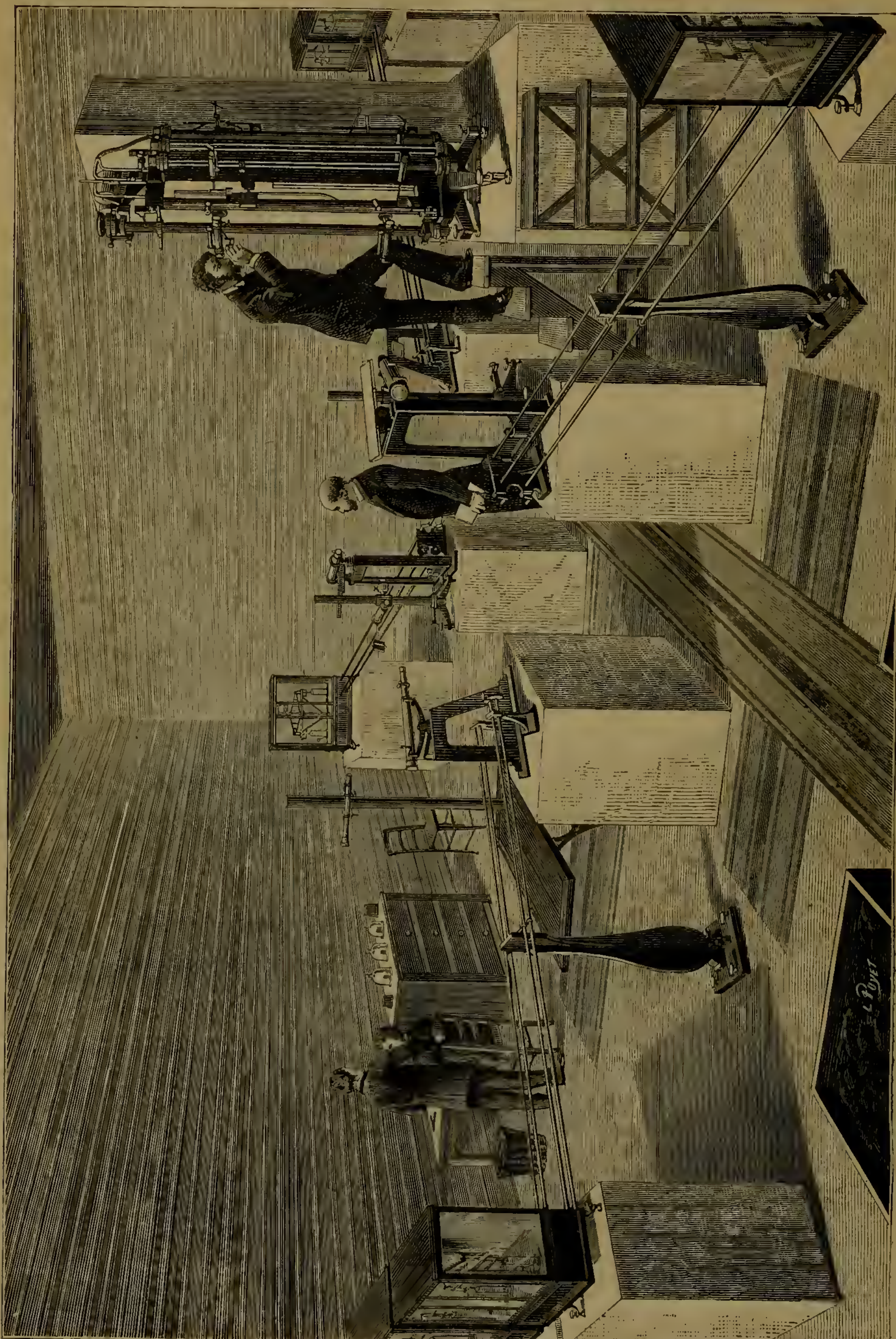


Fig. 3.—General View of the Great Hall of the Observatory.

the steam from a little copper, *f*, placed at some distance from it.

The arrangements we have just briefly described enable a difference of readings between the air thermometer and the mercurial thermometer to be determined to nearly the hundredth of a degree.

From the descriptive summary thus given it will be seen how important is the new and remarkable international establishment now really established in the neighbourhood of Paris. It remains to add a word regarding the benefits which the labours of this institution are calculated to yield and the phases they have actually assumed.

The signing of the Metre Convention of 1875 will necessarily be followed in the near future by the adoption of the metric system on the part of all the nations of the civilised world. The universal introduction of a uniform system of weights and measures, by establishing a new bond between people and people, and by promoting international relations, will undoubtedly prove a powerful factor in the interests of civilisation. This, however, is not the only, nor even the principal, interest of this international work. It was not necessary, it may be properly asserted, for the purposes of commerce and industry, to create a collection of such complex and perfect instruments and machines. More than anything else the interest of the labours of the bureau is scientific. Science will more and more cease to rest content with close approximations; in all possible branches it craves rigorous exactitude, it aims at precision. The International Bureau will furnish science not only with standards of measurement exactly controlled and verified, but also with a great number of physical constants determined with the greatest care and under conditions as perfect as possible.

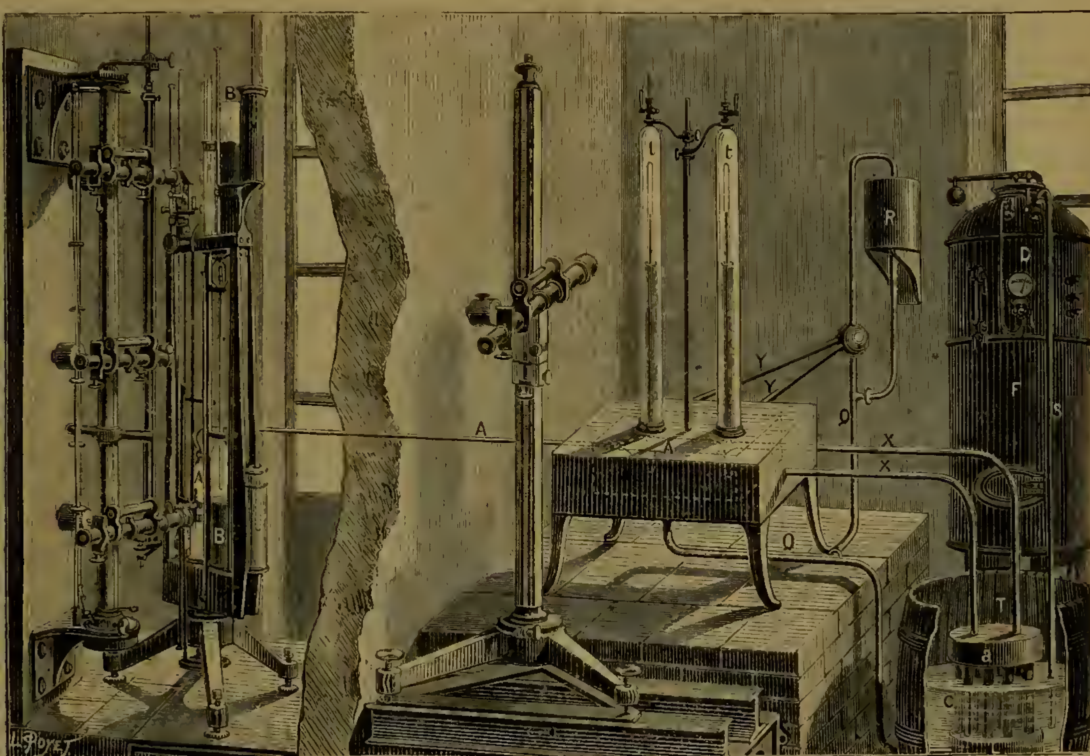


FIG. 4.—Apparatus for Measurement of Temperature and Barometric Pressure.

Among all the sciences the one which will reap the greatest benefit from the new institution is geodesy. In fact one of the greatest drawbacks to an exact knowledge of the figure of our globe is just the uncertainty still prevailing in regard to the relative values of the measures which, having been employed for the measurement of the different bases, have served as points of departure for triangulations executed on various points of the earth's crust. The minute study of these measures, centred henceforth in the laboratories of Breteuil, will assuredly cause the troublesome discrepancies to vanish, and will offer a surer basis for the labours of geodesists. As much may be said for the study of the variations of gravity by means of the pendulum. The International Commission have decided on taking as the point of departure for the new metric units the standards already existing, that is, the metre and the kilogramme of the archives of France in their actual state. This decision ought to receive unqualified approval. While rendering full homage to the great and valuable idea, formed at the end of last century, that the basis of universal measurements must be sought in the dimensions of the globe occupied by the human race, it ought also to be understood that for the present day the pith of the matter does not centre in the metre being a few microns (millionths of a millimetre) longer or shorter. The great point is that the whole world possess the same metre, and that the copies distributed be all perfectly equal to the standard, or rather rigorously determined in relation to that standard. To demand over and above that the length of the metre tally exactly with its theoretic definition would assuredly be demanding that the metre be subjected to periodical retouchings and modifications in order to make it keep pace with the progress of science, which would be the very worst of inconveniences for a fundamental unit.

This point settled, the next thing was to make an international metre and kilogramme,—copies, viz. as exact as possible of the metre and the kilogramme of the archives,

but presenting a greater guarantee than the originals in regard to their indefinite conservation and precision of comparisons, and which, being the common property of all the signatory nations of the Convention, should be preserved at the International Bureau, and serve henceforth as prototypes and points of departure in the system of weights and measures for the entire world.

The task next following was to make a sufficient number of metres and kilogrammes for distribution among the contracting Governments, after they had been compared with the international standards.

The choice of the material of which the new standards should be constructed, the form to be imposed on them, the nature and arrangement of the designs, the processes to be employed in the comparisons, and a host of accessory questions connected with the preceding matters—have all been the subject of long and learned deliberations, in which, besides the International Committee, the French section of the International Commission, composed of the best qualified French authorities, have played a great part. Even with the utmost possible brevity it would take too long to pass each of these points in review. Suffice it to say that the material adopted for the new standards, as much for the metre as the kilogramme, is platinum alloyed with a tenth part of iridium, which will impart to it greater hardness and resistance. The labours which the choice of this material has called forth have given rise to remarkable improvements in the mode of working and purifying the platinum and the metals with which it is found allied in the ore. It is impossible to recall them without at the same time bringing to remembrance Saint-Claire Deville, who with indefatigable zeal devoted the last years of his life to this pursuit.

The form fixed on for the metre is that of a bar, the section of which has the shape of an X or rather an H, the legs of which would straddle towards the top and towards the bottom. This form, calculated to supply a maximum rigidity for a given quantity of material, offers various other advantages on which we cannot now enlarge. It is 1·02 m. in length, and on the upper surface of the transversal limb (that is, on the neutral surface of a deflected beam) are traced two very fine lines, the distance of which at zero represents just the length of the metre. It is then a metre *à traits*. The metre of the archives, on the other hand, is a metre *à bouts*, that is, a bar measuring from one extremity to the other exactly the length of a metre. The metre is then defined by the distance at zero between the middles of the two terminal planes.

The comparisons between the international metre and the metre of the archives have been made at the *Conservatoire des Arts et Métiers*; those between the international kilogramme and the kilogramme of the archives have been made at the *Observatoire*. These labours, lasting no less than several months, have been performed by the care and under the direction of a mixed Commission composed of members of the International Committee and of members of the French section, under the presidency of M. Dumas, perpetual secretary to the Academy of Sciences, which represents France on the Committee. The fabrication of the national standards is in course of execution, and the definitive comparisons will shortly be able to be entered on.

NOTES

WE regret to announce the death of Dr. Oswald Heer of Zurich, the well-known palæontologist, at the age of seventy-five years. In his earlier years Dr. Heer devoted himself to entomology. We hope to give some notice of his life and work in our next number.

THE works in connection with the erection of the Ben Nevis Observatory are so far forward that the formal inauguration of the Observatory was to take place yesterday. With the view of stimulating public interest in the Observatory, there has just been published a small handbook giving an account of its origin and describing the objects it is intended to promote. Mr. George Reid, R.S.A., has contributed attractive drawings of Ben Nevis from the sea, and of the Observatory building; from Dr. Archibald Geikie have been obtained bird's-eye views of the scenery visible from the mountain top; and there is also inserted an excellent map, in which the new bridle-road is laid down and the configuration of the district indicated by numerous contour lines. From a statement given as to existing high-level meteorological stations in other parts of the world, it appears that America maintains two such posts—namely, Pikes Peak, 14,151 feet, and Mount Washington, 6286 feet; while France can claim four, ranging from 3989 to 12,199 feet; and Italy three, of which the highest is 8386, and the lowest 7087 feet. Russia has one as high as 3787 feet, and Switzerland two, of 7505 and 2875 feet respectively. The highest in this island, so far, would seem to be Hawes Junction, 1135 feet, and Dalnaspidal, 1450 feet. Ben Nevis gives an elevation of 4406 feet, and, as has been repeatedly explained, important results are expected from the comparisons it will enable meteorologists to make between the state of the atmosphere at that height and the conditions prevailing at sea level. No time will now be lost in commencing the work of the Observatory, which has been intrusted to Mr. R. T. Omond, with Mr. Angus Rankin, and another yet to be appointed, as assistant observers. During the winter months the summit of the Ben may for weeks together be inaccessible; but certain observations will be daily communicated by means of the telegraph now being laid by the Post Office.

THE announcement of the publication of the Berlin Catalogue of Zonal Stars will have the effect of postponing the publication of the French catalogue, for which a credit of 400,000 francs had been asked from the Budget Commission.

THE President of the Berlin Geological Society has received a telegram from the Pentland Firth announcing the safe return of the German schooner *Germania*, which carried the German Polar observing party from the Gulf of Cumberland, where it has spent a year in successful observation and research.

WE have received a telegram from Herr Augustin Gamél of Copenhagen, in which he informs us that the *Dijmphna* anchored at Vardö, Norway, on October 11, all being well on board.

THE Russian Geographical Society is taking an active part in the International Congress which is to be convoked by the United States for the unification of the meridian. Delegates from the Academy of Sciences and from the Russian Ministries of War, and Posts and Telegraphs, will constitute a Committee at St. Petersburg, and the conclusions of this Committee will be supported at Washington by one or more Russian delegates.

WE learn from the annual reports of the West Siberian and East Siberian branches of the Russian Geographical Society (published in the *Izvestia*) that the East Siberian branch busily continues the exploration of the very rich remains from the stone period around Lake Baikal. The valley of Tunka, which seems to have been an immense workshop for the fabrication of quartz, jade, and nephrite implements, has been visited again by M. Vitkovsky, as well as the valley of the Angara. This last consists of a succession of large plains separated by narrow gorges; the former was occupied during the Post Pliocene period by a series of lakes, and subsequently it was the abode of a numerous population of the Stone period. M. Agapitoff discovered also a place on the Steppe of Ust-Unga which must have been a large workshop for the fabrication of stone-implements, pieces of which cover the steppe over a space of more than twelve miles; thousands of implements could be collected

on the steppe. It is worthy of notice that the stone hatchets of the steppe are quite like the stone implements of the Chukches. The West Siberian branch continues the exploration of the less known parts of Western Siberia, and the last volume of its *Memoirs* contains several interesting papers:—On the Altaï, by M. Yadrintseff: and on the Narym region, its inhabitants, and their trades, by MM. Grigorovsky and Shostakovich.

PROF. NORDENSKJÖLD has presented a meteoric block, which he found in 1870 at Greenland, to the Helsingfors University, where it has just arrived from America. Its size is not great, only one foot in height, but it is very heavy. It bears the following inscription in English: "Terrestrial native iron. Ovipak, Greenland. Brought by A. E. Nordenskjöld." In presenting this unique specimen to the University the Swedish explorer writes:—"During my journey to Greenland in 1870, I found at Ovipak, on the Disco peninsula, several large blocks of iron which were brought home the year after by one of the naval steamers. On arriving there they were equally divided, as far as possible, into three parts, of which one became Swedish, the other Danish, and the third my property. To the latter belongs the block which I present to you, its weight being about 10,000 lbs. The same has, since 1876, when it was exhibited in Philadelphia, been deposited in Washington. As may be generally known, a fierce controversy has raged as to the nature of these blocks, some authorities maintaining that they were of meteoric, others of terrestrial, origin, a question on which opinions certainly may be divided. However this may be, it is certain that these blocks, whether as a specimen of the cosmic matter in the universe, or of the earth's interior, are of exceptional interest, and may be considered to be valuable gems in any museum. To me personally this discovery is enhanced in value, as it enables me to present a testimony of my gratitude and affection to the institution where I received my first scientific education, and passed the most important period of my life." The block is to be kept out in the open air, as it has been discovered that these stones waste away in a room.

MR. D. MORRIS has in the press a work which will be shortly published, entitled "The Colony of British Honduras, its Resources and Prospects; with Particular Reference to its Indigenous Plants and Economic Productions." This work will include the results of Mr. Morris's travels in British Honduras, and throw a new light on many points connected with the climate, the flora, and the resources of this little known British dependency. The publisher will be Mr. Edward Stanford.

MESSRS. W. H. ALLEN AND CO. will publish shortly "The Influence of the Sun on Natural Phenomena," by A. H. Swinton, author of "Insect Variety."

THE green sun referred to last week as observed in India was also observed in every part of Ceylon from September 9 to 12. One correspondent writes as follows to the *Ceylon Observer*:—"Puleadierakam, September 12.—I write this from the above place on my way to Trincomalee, being much interested to learn whether the same phenomena exist throughout the island. The sun for the last four days rises in splendid green when visible, *i.e.* about 10° from the horizon. As he advances, he assumes a beautiful blue, and as he comes further on looks a brilliant blue resembling burning sulphur. When about 45° it is not possible to look at it with the naked eye; but, even when at the very zenith, the light is blue, varying from a pale blue early to a bright blue later on, almost similar to moonlight even at midday. Then, as he declines, the sun assumes the same changes but *vice versâ*. The heat is greatly modified, and there is nothing like the usual hot days of September. The moon now visible in the afternoons looks also tinged with blue after sunset, and as she declines assumes a most fiery colour 30° from the zenith. The people are in terror at these phenomena, some even expecting the end. Can this be the result of the eruption in the Sund Straits?—P.S.—There is no light even though the sun is visible until nearly 7 a.m."

A TERRIBLE earthquake occurred on Tuesday near Cheshmeh, a small town on a peninsula on the coast of Anatolia, and about twelve miles from the Island of Scio. Of late there have been several earthquake shocks in the pashalic of Anatolia and in other parts of Asia Minor, but it is to be feared that Tuesday's event eclipses all recent shocks in the devastation it has caused. It appears that the whole peninsula, from Smyrna to Cheshmeh, together with the neighbouring Island of Scio, was violently convulsed. The greatest destruction has been wrought in the western half of the peninsula between Cheshmeh and Voulra. All the villages in this district are destroyed, being nothing more than heaps of ruins. The wretched inhabitants had no time to escape, and upwards of 1000, it is estimated, have perished, while many others are injured.

AT 11.20 p.m. October 9 a slight shock of earthquake was felt at Irkutsk, Siberia. Several shocks of earthquake were felt on the 10th in the afternoon throughout the whole of Northern Moravia. The oscillations lasted on each occasion from one to two seconds. The most violent shock occurred at Olmütz. Telegrams from Cilli, in Southern Styria, show that there were severe shocks felt there about an hour earlier than at Olmütz. On the same morning, too, there was a shock at Agram, lasting two seconds. A strong shock of earthquake, lasting fully eight to ten seconds, was also felt at Chios. The shock was felt at Syra, on the Dardanelles coast, and at Smyrna.

A WELL attended meeting of science and art teachers was held at the Birmingham and Midland Institute on Saturday last. On the motion of Prof. Tilden (who presided), seconded by Mr. E. R. Taylor of the Birmingham School of Art, it was resolved "That in the opinion of this meeting it is desirable to establish for Birmingham and the district a branch of the National Association of Science and Art Teachers." Among the objects of such an association it was mentioned would be the improvement of science and art teaching by discussions of methods of teaching and modes of demonstrating important scientific laws. A provisional committee was appointed, with Mr. C. J. Woodward as honorary secretary.

PRINCIPAL DAWSON asks us to state that in our report last week of his paper at the British Association (p. 579), the word *relatives* in the title should be *relations*, and that not *tin* ore but *iron* ore occurs in the Laurentian.

THE additions to the Zoological Society's Gardens during the past week include a Ruppell's Parrot (*Pæocephalus rueppelli* ♀) from East Africa, presented by Dr. George L. Galpin; a Malabar Parrakeet (*Palæornis columboides*) from Southern India, presented by Mr. F. W. Bourdillon; two Pileated Jays (*Cyanocorax pileatus*) from La Plata, presented by Mrs. J. W. Hammond; two Buzzards (*Buteo vulgaris*), a Hobby (*Falco subbuteo*), European, presented by Capt. H. Linklater; a Tiger Bittern (*Tigrisoma brasiliense*) from South America, presented by Mr. Joseph H. Cheetham, F.Z.S.; a Turtle Dove (*Turtur communis*), captured at sea, presented by Mr. W. M. Brown; five Long-nosed Vipers (*Vipera ammodytes*), a Viperine Snake (*Tropidonotus viperinus*), European, presented by Lord Lilford, F.Z.S.; a Macaque Monkey (*Macacus cynomolgus*) from India, a White-fronted Capuchin (*Cebus albifrons*) from South America, two Michie's Tufted Deer (*Elaphodus michianus* ♂ ♂), an Elliot's Pheasant (*Phasianus ellioti* ♂) from China, deposited; two Eyras (*Felis eyra*), a Red-vented Parrot (*Pionus menstruus*) from South America, a White-fronted Amazon (*Chrysotis leucocephala*) from Cuba, two Royal Pythons (*Python regius*) from West Africa, purchased; a Collared Fruit Bat (*Cynonycteris collaris*), born in the Gardens.

THE MOVEMENTS OF THE EARTH [1]

I.—*Measurement of Space*

IN proceeding to deal with the application of the various branches of physical science to the investigation of those phenomena which lie beyond the earth, there is a very large field from which to make choice of a subject which will show, now the application of one branch of science, and now the application of another, and bring us, in this way, somewhat nearer to the truths and the beauties which lie in the most distant realms of space for all who will take the trouble to look for them. But perhaps it may be more desirable to select that part of the subject which, so to speak, lies nearer home, and endeavour to point out how, by means of the application of principles, and methods, and instruments which are generally familiar, and which at all events are of daily use, the various movements with which our planet is endowed may be studied, not only with reference to the phenomena themselves, but with reference also to the causes which lie at the bottom of them.

The various branches of knowledge which will have to be drawn upon in furnishing the materials necessary for this inquiry were really started long before it was imagined that the earth had any movements at all; but still, on the whole, the growth of the knowledge of its movements has been so beautifully continuous, that we cannot do better now than consider historically the way in which those sciences have grown up, which enable us to make certain measurements, and to get out correctly certain quantities, which must necessarily lie at the bottom of any sound knowledge.

What particular things do we want to measure? It has been already said that when the sciences to which attention will have to be called later on were founded, very few people on this planet knew that it moved at all, but it is now generally known that the earth does move. It will be obvious however that, whether the earth moves or not (and that may be considered still a moot question), if we wish to form a basis for our judgment in any direction, we must be able to measure time and space. It has been well said that "time and space are the moulds in which phenomena are cast;" for when it is desired to gain any useful knowledge concerning any fact, the relation which it bears to the things around it, and the time of its occurrence must be known, and that is the only thing an astronomer tries to do when he is investigating that portion of his subject to which we must first turn our attention. We will begin then by considering those measurements of space which are of the first importance to the astronomer. I do not here refer to the ordinary familiar measurement of inches, yards, and miles, but to the measurement of angles, and it will be well to get a good notion of this angular measurement as soon as possible.

There is no special necessity for dividing the circle into 360 parts, but the greatest number of people have made that division, and it is still continued to be done. When the Chinese began to make circles they divided them, not into 360 parts, but into 365¼. Now there was a great advantage, and a great disadvantage about that. The advantage was that this number of divisions in the Chinese circle was the same as the number of days in the year; the disadvantage was that they were not dealing with whole numbers, and their 365¼ was not such a convenient number to halve and quarter, and so on, as is 360. In quite recent times it has been suggested that 400 parts should be taken instead of 360, but that is a suggestion which up to the present time has not been acted upon.

We have then an angle defined as the inclination of two straight lines starting from a centre; if we get one of these lines traversing an entire circumference, the other remaining at rest, the travelling line will have traversed 360°; we have what is called a right angle when one of the lines has been separated from the other through a quarter of a circumference—that is, 90°. This is the fundamental idea of angular measurement, the only measurement of space with which we shall have to deal at present.

For instance, if a little ivory rule be opened, its two parts become inclined to each other, and inclose what is known as an angle. That angle may be made large or small by opening and closing the two parts, A and B (see Fig. 1) of the rule. Suppose the rule to be shut, the point on which it turns being in the centre of the circle, CDEF, and that, whilst A remains at rest, B is made to travel successively through B and B^1 to B^2. It will then have travelled half the circumference of the circle CDEF, but civilised people, in order to get perfectly clear notions about this measurement, and to be able to tell each other what particular measurement they have made in this way, instead of talking of a circumference merely,

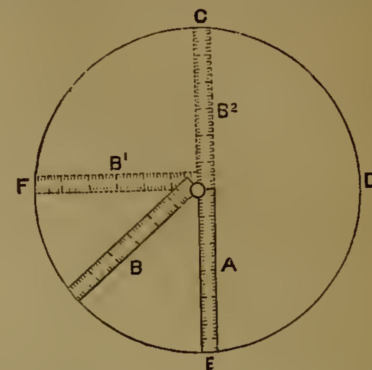


FIG. 1.—Use of a two-foot rule to explain angular measurement. With the part A at rest, the movement of the other to B, B^1 and B^2 gives us 45°, 90°, and 180°.

and of certain rough divisions of it, have divided all circles into 360 parts called degrees, and say that the travelling part, B, of the rule has travelled through not a quarter, or a half circumference, but through 90 and 180 degrees respectively.

Why are these measurements of space required? For the reason that when we are dealing with the heavenly bodies and seeking to define the position of any object, two facts at least are required to be known before its exact position can be determined. An observer going out at night upon an extended plain would see some celestial bodies near where the earth meets the sky all round, which is called the circle of the horizon, and he might happen to see another body exactly overhead, in what is called the zenith. In passing from this zenith to the horizon it will be obvious that a quarter of a circumference is traversed (see Fig. 2). That distance may therefore be divided into 90°.

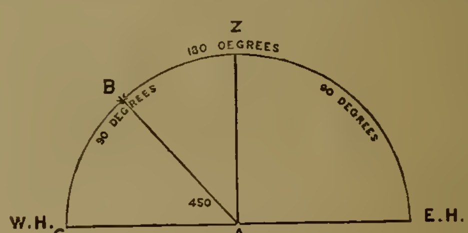


FIG. 2.—Measurement of altitudes.

Similarly in passing from the eastern horizon to the western horizon half a circumference is travelled over. This distance therefore is divided into 180° of angular measurement in the same way that the half of the circumference traversed by the travelling rule was divided into 180°.

Now if it can be ascertained of any body that it is exactly in the zenith, the position of that one body has been definitely stated for the particular time at which the observation is made. But consider the case of another body not in the zenith. Suppose that the lines, the one AB (see Fig. 2), passing from the observer to the object, and the other, AC, passing from the observer to the horizon, inclose an angle of 45°. This angle is called the star's altitude. But to say simply that the altitude of a star is 45° does not sufficiently define its position. Let the reader imagine himself to be standing in the Albert Hall. He knows that he may look up and see rows of panes of glass and ornamented work running around the hall at different heights above the floor. He may also notice, let us say, various series of ornamentation arranged vertically from floor to roof. Now suppose it were desired to define the position of any one pane of glass or piece of ornamentation in any one of these horizontal or vertical rows. It is obvious that to say of any pane of glass at one level that it is at a certain height above the floor will not suffice, for all the panes of glass in that row are at the same

[1] Report of Lectures to Working Men given at the Royal School of Mines by J. Norman Lockyer, F.R.S.

elevation. In like manner in defining the position of any one piece of ornamentation in the vertical series it will not be sufficient to say that it is at a certain angular distance from any one point, say a door, because all the pieces in the same row are at this angular distance from the door. But if these two methods of stating position be combined, if the height above the ground as well as the angular distance from the door be given, then a definite statement may be made both of the position of the pane of glass and the piece of ornamentation. Similarly with the stars. Imagine a horizontal circle passing from north to south, and thence to north again. A line from the zenith through any body will cut this circle at some one point, and the number of degrees included between that point and the north point will give the angular distance from the north point, or, as it is called, the azimuth. The whole of an imaginary line of bodies extending from the zenith to the horizon will have the same azimuth (see Fig. 3). In the same way we may imagine a whole ring of bodies

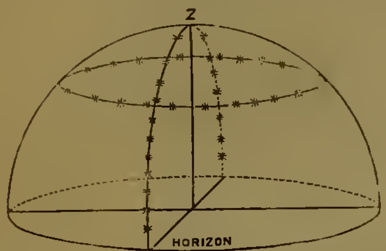


FIG. 3.—Stars with equal altitudes and stars with equal azimuths.

at the same height above the horizon, having the same altitude (see Fig. 3), but a particular altitude and a particular azimuth can be true of only one of those bodies. It is in this way, then, by a statement of the altitude and azimuth, that the position of a star or other celestial body can be indicated with reference to any one particular place of observation and any one particular instant of time.

It is by thus dealing with this angular measurement that the exact positions of the heavenly bodies have been determined.

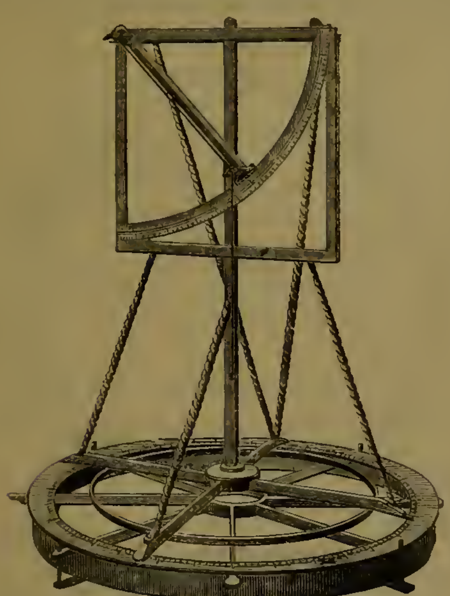


FIG. 4.—Tycho Brahe's altitude and azimuth instrument.

This point has been discussed at some length, because in making an historical survey it will be found, that the growth of that particular knowledge of which we shall come to speak, has been the growth of man's capability of getting finer and finer in this angular measurement. To go back to the time of the old Greeks, Hipparchus, one of the most eminent of ancient observers, even in his day could define the position of a heavenly body to within one-third of a degree. Since these 360 degrees into which circles are divided are each subdivided, first into 60 minutes, and each of these again into 60 seconds, the one-third of a degree to which Hipparchus attained may be called 20 minutes of arc.

Passing from his time to the middle ages, a most interesting instrument then in use claims attention. Fig. 4 is a copy of a photograph of the instrument.

The model, from which the photograph has been taken, is an exact copy of an instrument made by one of the most industrious astronomers that ever lived, Tycho Brahe, and shows how, even in the very beginning of this observational science, men got at a very admirable way of making their observations, considering the means they had at their disposal. First there was in this instrument a quadrant of a circle (see Fig. 4), which served their purpose just as well as a whole circle. Combined with this was an arrangement somewhat resembling the "sights" on a modern rifle. Remember this was before the days of telescopes. So they started with these sights and a little pinhole, that they might take a shot, as it were, at a heavenly body, putting the eye near the pinhole, and seeing the heavenly body in a line with the front sight. Then the instrument was provided with a plumbline to show the vertical. This plumbline was so arranged that when the sight lay along it, a body in the zenith would be observed, and an angle of 90° altitude recorded. With the instrument thus set, any smaller altitude could be read along the quadrant, according to the position of the line of sight passing through the eye, the centre of the quadrant, and the place of the heavenly body.

To get azimuth they used a horizontal circle, shown at the base, also divided into degrees and provided with a pointer. By sweeping the instrument round until the azimuth was such that the body was seen through the pinhole, and the altitude was such that it was seen in a line with the front sight, they fixed its position, as well as that instrument enabled it to be done. Supposing that their circles were properly divided, it was quite easy to determine a division as small as the quarter of a degree. This would put Tycho Brahe in only a little better position than Hipparchus. That is to say, from the time of the Greeks until about the middle of the fifteenth century, the only advance made with this angular measurement, was that a reading of one-third was improved into a reading of one-fourth of a degree.

Another notable improvement and advance towards a finer and more accurate measurement was made by Digges. He introduced the diagonal scale, the principle of which is shown in Fig. 5. The arrangement consists of a number of concentric

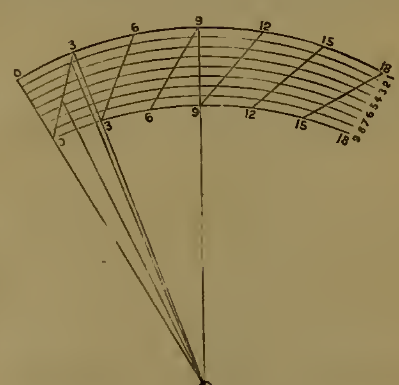


FIG. 5.—Digges' diagonal scale.

circles, in this case nine. The distance between the divisions of the inner circle is 3°. From each of these divisions diagonal lines are drawn to the outer circle in such a manner that the diagonal cutting the first circle at 0° cuts the ninth circle at 3°. That cutting the first circle at 3° cuts the outer circle at 6°. So with the other diagonal lines. Consider the diagonal passing from 0° on the inner circle to 3° on the outer. If the pointer cuts the scale at the former point, an observation of 0° will have been made; if it cuts at the latter point, an observation of 3° will have been made. But it may cut the scale at some intermediate point. Suppose it falls on the eighth of the nine concentric circles, then the value of the observation will be 7/8ths of 3°. Should the pointer fall half way between 0° and 3°, the reading

will be 4/8ths of 3°. So with the other intermediate points. In this way, then, Digges enabled a much greater accuracy to be attained in this circle reading.

The next great improvement after that of Digges was one made by M. Vernier, a Frenchman, who, in about the year 1631, invented the instrument which bears his name. The following is the arrangement. Let the scale on which the measurements are made be divided into a certain number of parts. Take a second

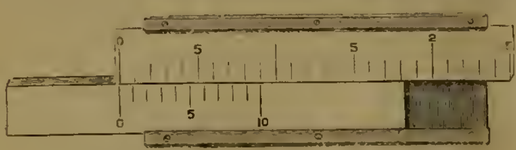


FIG. 6.—Vernier reading to tenths of divisions.

scale called the vernier, shorter than the first by the length of one of its divisions, and make the number of divisions in this vernier equal to the number of divisions in the scale. Then each of the divisions of the vernier, will be less than each of the parts of the scale, by a fraction having one for its numerator, and the number of divisions in the scale or vernier respectively for its denominator. Thus if the number of divisions be ten (see Fig. 6), and the vernier equal in length to nine of such

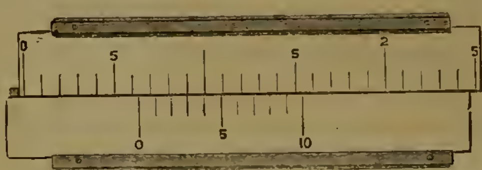


FIG. 7.—Vernier shown in Fig. 6 reading to three-tenths.

parts has also ten divisions, each of these divisions will be shorter by 1/10th than each of the parts of the scale. If the number of divisions be seventeen (see Fig. 8) the different parts, of the vernier will be less by 1/17th than each of the divisions of the scale. So when the number of divisions is thirty (see Fig. 9), the parts of the vernier will be less by 1/30th than the divisions of the scale. The arrangement, however, is not limited to straight scales. It may also be used for the determination of small

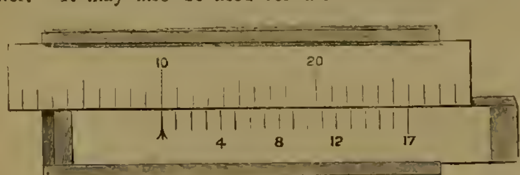


FIG. 8.—Vernier reading to seventeenths.

fractions of degrees on a circle. Fig. 10 represents a vernier giving tenths of degrees on a circle. It need hardly be said that the vernier may be constructed to give readings upon the inner as well as the outer edge of the graduation.

In using the vernier the observer looks along it until he meets a coincidence, that is for a point where one of the divisions on the scale coincides with a division on the vernier. If this occurs at the eighth division, then the observation is some whole num-

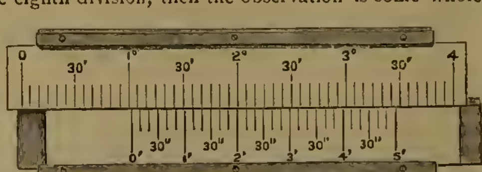


FIG. 9.—Application of vernier to circle reading to one-tenth of a degree.

ber, and 8/10ths, 8/17ths, or 8/30ths, according as the scale used is divided into ten, seventeen, or thirty parts. In Fig. 7 the coincidence occurs at the third division; the reading in that case would be some whole number and 3/10ths.

To the instrument of Tycho Brahe, then, the vernier, which can be adapted to it, has now been added. Of course by taking divisions enough the measurement may be made as fine as possible. A vernier of 100 divisions may replace the vernier of 10, of 17, or of 30 divisions. Seventeen divisions have been chosen to show that the principle is not limited to tenths. Any number of divisions may be taken. A very fine degree of accuracy can be attained then in angular measurement, owing to the introduction of the vernier, and that is why there is what is practically a vernier upon almost every measuring instrument in every workshop and laboratory. The question next arises whether with the introduction of the vernier the limit of accuracy has been reached, or whether it be possible to go beyond this. A negative reply may be made to this question. The

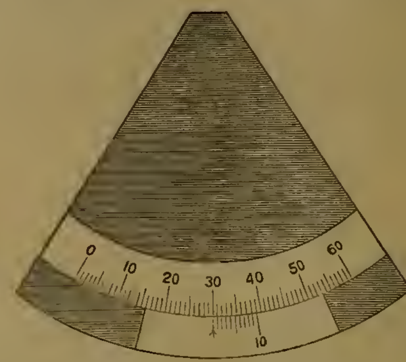


FIG. 10.—Application of vernier to circle reading to ten seconds of arc.

limit of accuracy has not here been reached. In order to get more accuracy in this angular measurement, it is only necessary to add some branch of physical science to those geometrical considerations by means of which circles have been so finely divided. The astronomer culls certain portions out of the science of optics, and uses them for his purpose. It is perfectly clear that the reason a limit is reached with an arrangement of the nature of the vernier is, that at last the divisions get so small that the eye cannot distinguish them, so that optical principles have to be appealed to to increase the power of the eye.

Before discussing this question of whether it be possible to

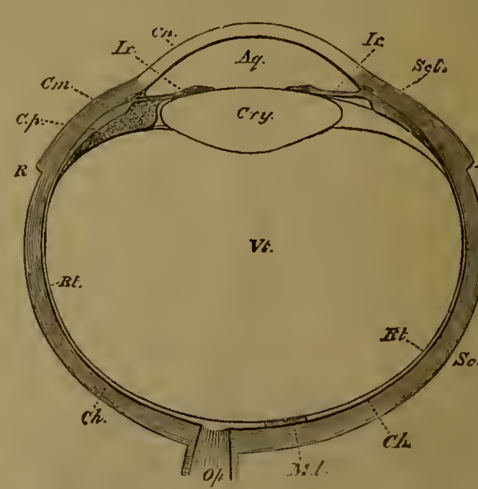


FIG. 11.—Horizontal section of the human eye.

select some principle of optics, by the application of which the power of the eye may be increased, it will be well to consider in what it is that that power consists. Fig. 11 will give a rough notion of those parts of the eye which specially relate to this matter. First comes the curved surface *Cn*, the cornea, and next *Aq*, the small anterior chamber which contains the aqueous humour. Behind this comes *Ir*, the iris, which limits the amount of light entering the eye, this being immediately succeeded by *Cry*, the crystalline lens. Then comes the large posterior chamber of the eye which contains the vitreous humour. Behind this the optic nerve enters the eyeball, ex-

panding itself into the delicate layer of nervous elements, *Rt*, which lines the inner surface of the vitreous cavity.

When any object is seen by the eye, the rays of light emanating from that body, impinging first upon the curved corneal surface, have to pass successively through *Aq*, *Cry*, and *Vt*, before they can affect the nervous retinal elements and cause the sensation of light. In passing through these portions of the eye, the rays of light are dealt with in a peculiar manner, especially perhaps by the crystalline lens, and are brought together to form what is called an image on the retina. This image influences the nervous elements of which the retina is composed in such a way, that a sort of telegram is sent to the brain through the optic nerve, and the brain becomes conscious of having seen something, the particular object seen being included in the message. Another diagram (Fig. 12) will perhaps make it a little clearer how this image on the retina is formed. At *A B* is an arrow; from it rays of light are marked going to the three different points on the retina. But it will be seen that those rays of light which come from the top of the arrow are, by the action of these three media, twisted downwards, and form an image of the top portion of the arrow on a low part of the retina. The rays of light proceeding from the bottom of the arrow are bent up, so that its image is formed on an upper part of the retina. The light coming from the middle of the arrow is not bent at all, and therefore forms its image on a middle portion of the retina. That is the way in which the eye deals with rays of light entering

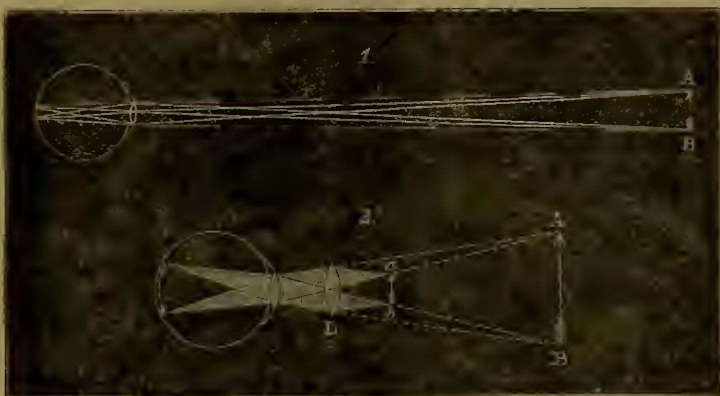


Fig. 12.—1. Diagram showing path of rays when viewing an object at an easy distance. 2. Object brought close to eye when the lens L is required to assist the eye-lens to observe the image when the object is magnified.

it. With this knowledge of the optics of the eye, it will be very easily seen how very wonderfully the construction of the eye has been imitated in a photographic camera. The front lens is practically the equivalent of those three refractive media of the eye, the aqueous and vitreous humours, and the crystalline lens; whilst the iris, which in the eye serves to limit the amount of light entering it, has its exact representative in the "stop," which serves the same end in the camera. The photographic plate is, it need hardly be said, the counterpart of the retina, and has consequently been beautifully described as "a retina which does not forget." Similarly there is just such an arrangement for focusing the light as exists in the eye. In fact a camera is a rather better machine altogether than the eye, because the range is greater, and the focusing power is not lost as age increases. Therefore the artificial eyes of our camera are never in need of spectacles.

1. *How Optics enables us to Read Fine Verniers.*—This knowledge, then, having been acquired, how is it to be utilised for the purpose of the measurement of angular space? It may be utilised in this way. The reason that we cannot clearly distinguish objects placed very close to the eye is, that the rays of light which flow from them are so extremely divergent that the crystalline lens cannot focus them on the retina. But by placing between the eye and the object a double convex lens, that is a lens like the crystalline lens of the eye, this extreme divergence is corrected; the crystalline lens is thus aided, and the rays of light are brought to a focus, as shown in the lower part of Fig. 12. Take the case of a vernier whose divisions are so fine that they are not visible at the distance of distinct vision, say about ten inches. If we attempt to correct this by making the divisions appear larger, by bringing the vernier close to the eye, we lose the power of focusing the rays which flow from it. But the introduction of a convex lens between the vernier and the eye enables the eye to see the division quite distinctly.

Of course the more nearly an object approaches the eye, the more powerful must be the lens, in order that the eye may clearly see it. In this way we see that the simple addition of a convex lens has enormously increased our power of observing and measuring small angles.

2. One can, however, go further than this, and use not one simple lens, but a combination of lenses. But before discussing the various combinations of lenses which are employed in various instruments, it is necessary to look a little more closely than we have yet done at the structure and action of our convex lens. Let us use a glass lens in conjunction with an electric lamp. Then we may get an image of the carbon poles thrown on the

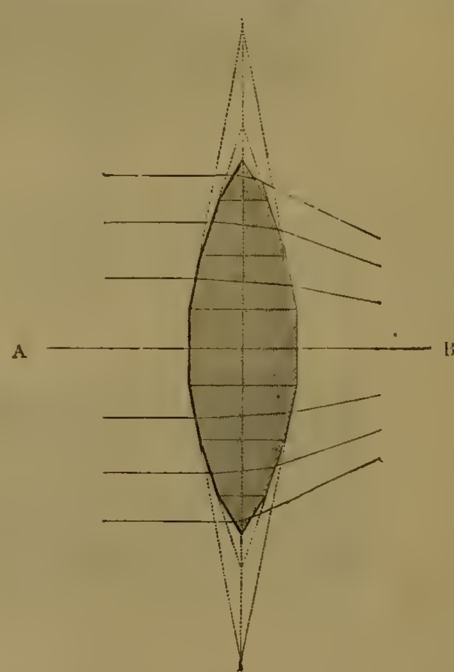


Fig. 13.—Formation of a lens from sections of prisms.

screen, in exactly the same way that the crystalline lens forms its image on the retina. But there would be this important difference, that while the image formed by the crystalline lens would

be a clear and distinct one, that formed by our glass lens would be a very bad one; instead of the poles of the electric arc being clearly and sharply defined, they would appear as if seen in a haze, and would be surrounded by coloured fringes of light, and not much could be made of them. Why is this? We find by experiment that this attempt to imitate the action of the eye by means of such a simple glass lens is an incorrect way of proceeding, the eye possessing certain qualities which the simple glass lens does not. Although a lens seems to be a very simple matter, its structure is really based upon some very complicated considerations. If a section of it be taken it will be seen that its surface is built up of sections of triangular pieces of glass, these triangular pieces of glass being called prisms, and how they deal with the light it is very important for us to know. If in front of the beam of light issuing from the lantern a prism be interposed, it will be found that whilst part of the light is reflected from its first surface another portion is refracted as it is termed, that is, bent out of its original course by the prism. Further, it not only suffers this deviation due to refraction, but it undergoes also what is called dispersion. In fact, where the light falls on the screen an infinite number of different colours are seen, these forming what is called a spectrum. This is one of the reasons why such a glass lens as we have used will not perform the finer work of the eye; the images of the poles are surrounded by a false glow, because it is difficult to give the lens the proper curvature, and there is this power of dispersion which breaks the compound white light up into a number of its different elementary colours. It is this power of deviation which the lens possesses which enables it to bend the rays differently according to their different distances from its centre, and causes them to form an image at what is termed the focus of the lens. The rays of light passing through the outer part of the lens undergo more deviation in order that they may be brought to a focus at the same point as the other rays. Now prisms which are made of different material, although they be of the same size and of the same angle, produce different deviations and different dispersions of the light which falls upon them. This fact has been taken advantage of in the construction of lenses. Let us take an illustration of the way in which this has been done. Imagine glass which gives a high dispersion and but slight deviation, set to work against glass giving great deviation with but little dispersion. It is obvious that it is quite possible by a combination of that character to keep the deviation and get rid of the dispersion, or to keep the dispersion and get rid of the deviation, as may be desired. By doing this an artificial eye of great excellence may be made. Suppose two different kinds of glass so combined as to form a prism, which should give a perfectly white image. Then the

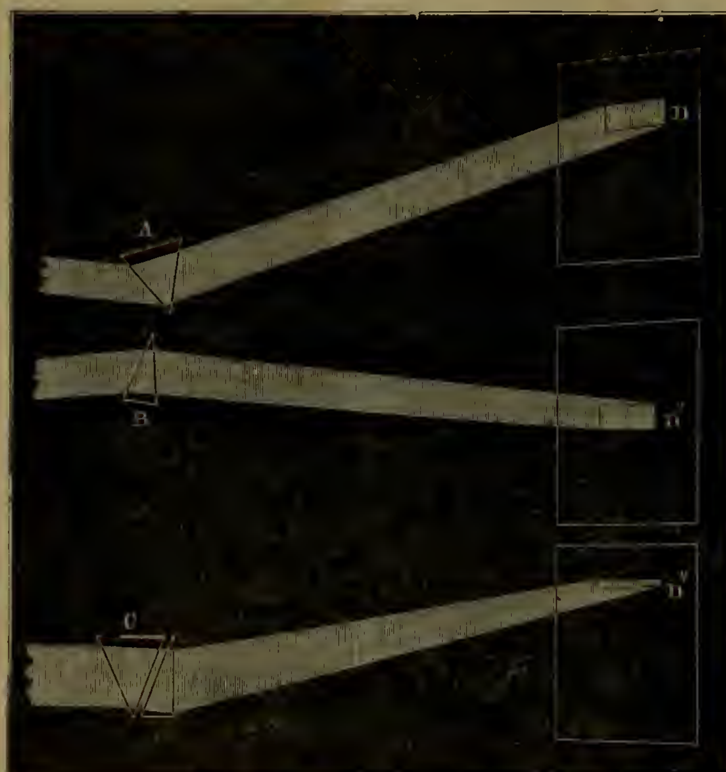


FIG. 14.—Diagram explaining the formation of an achromatic lens. A, crown-glass prism; B, flint-glass prism of less angle, but giving the same amount of colour; C, the two prisms combined, giving a colourless yet deviated band of light at D''.

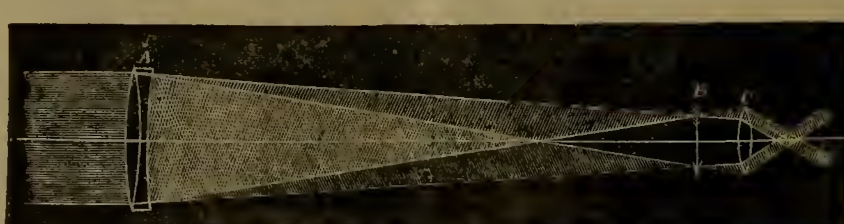


FIG. 15.—Telescope. A, object-glass, giving an image at B; C, lens for magnifying image B.

dispersion will have been got rid of, and the deviation will have been retained, and this is exactly what takes place in the modern compound, or, as it is called, achromatic lens. By building up a lens in this way we can get a much better image of the carbon poles of the lamp than before. This compound, achromatic lens, when used in a combination, is called the object-glass, because it is pointed to the object. But when it is a question of the combination of lenses, there is something else to be considered besides the mere formation of images. It is not enough to consider merely this, because when we spoke of the action of a convex lens in aiding us to read the vernier, we found that if an image was to be obtained the rays entering the eye must be practically parallel. In that case the rays always come to a focus at the same point. If the rays are not parallel, but divergent rays, then their focus will vary with the varying distance of the source of light.

In combining lenses together, then, it is important to bear in mind the fact that the rays of light which, after passing through the lenses ultimately reach the eye, must be parallel ones. Let us consider that arrangement which obtains in the telescope. In the simple form of this instrument, A (Fig. 13), representing the object-glass, receives the rays of light and forms an image of the distant arrow, from which they are supposed to flow, in exactly the same way that the lens we used just now formed an image of the carbon poles on the screen.

This image, then, having been observed, the eye views the distant object as if the object itself were placed at B. Remember now the way in which the eye was enabled to read the vernier placed close to it, and the action of the convex eyepiece of the telescope will be very obvious. In just the same way as the divergent rays coming from the vernier were grasped by the convex lens, and rendered parallel, so in this case the convex eyepiece of the telescope grasps the divergent rays from the image, reduces them

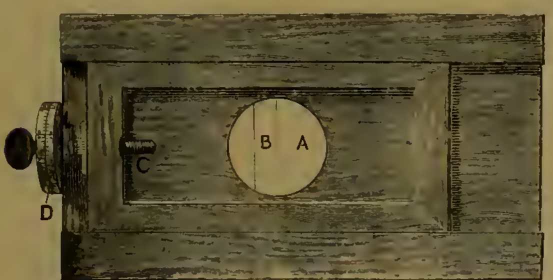


FIG. 16.—Model of Micrometer.

to the necessary condition of parallelism, and thus enables the image of the object to be clearly formed upon the retina of the observer's eye.

We have, then, got so far that by means of an object-glass we produce an aërial image, and by means of a convex lens we can view this image under conditions which enable another image of it to be formed on the retina. It is at once obvious that we can do something more than this, for if we place a concrete thing such as a cross wire at the same distance in front of the convex lens as the aërial image, or, in other words, at the focal distance of the object-glass, we shall see both the aërial image and the concrete thing, be it a cross wire or what not, both together. Now imagine that we can obtain an aërial image in this way of a star, and that side by side with this image of the star we observe the cross wire. It is quite clear that if we have any means of getting the cross wire to bisect the image of the star we shall have a much more accurate method of pointing at the celestial body, and therefore of measuring the angle between two celestial bodies, than was possible on the old system of sights without telescopes.

Suppose this telescope of ours to supplant the pointer of the old instrument of Tycho Brahe, consider the extreme accuracy of its observation as compared with that of the pointer in Tycho's quadrant, and it will be seen how vastly the application of these optical principles has added to the instrumental powers of the astronomer.

3. *How Optics enables us to Replace the Vernier by a Micrometer.*—But we have not yet done with optics. Its principles have been applied in yet another manner, but still, like these two applications which we have considered, tending to increase the power of accurately measuring minute angular distances of space.

Fig. 14 shows a simple model which has been designed to illustrate the principle of the instrument called the micrometer. This instrument places in the hands of the astronomer the power of measuring with extreme accuracy the most minute distances. It consists of two vertical wires, one, A, fixed, the other, B, movable by the rotation of a very perfectly cut screw, seen at C. The head of the screw, D, is divided into 100 parts, and read by means of a vernier to 1/1000ths.

This system of threads moving over certain small distances which can be accurately measured by means of a micrometer screw, can replace the cross wires to which we have just referred, and there are two very notable applications of this principle to which reference must now be made. When the object-glass is used for astronomical purposes, it is naturally arranged to bring the rays which fall upon it from a celestial body, and which are practically parallel, to a focus which represents the actual focus of the lens for such rays, and which is called the principal focus. But it is not necessary that the rays which fall upon such a lens should be parallel. The lens acts under other conditions with this proviso, that the more the rays diverge from the body in front of it, or, in other words, the nearer the object is to it, the greater will be the distance behind the lens of the point at which the aërial image is formed.

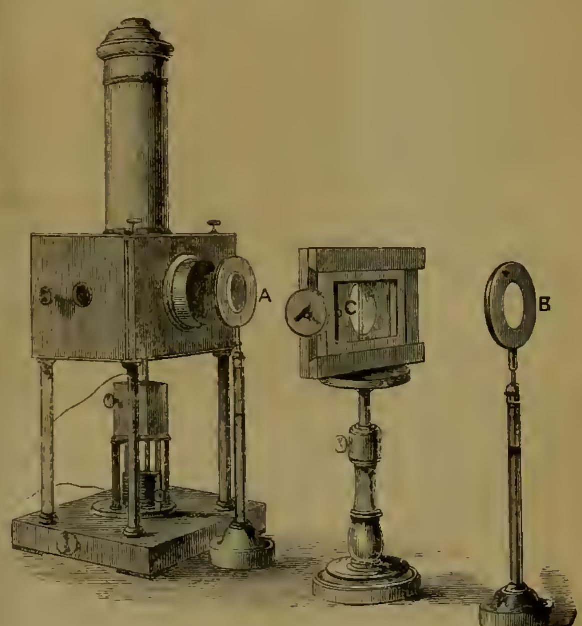


FIG. 17.—Micrometer arranged for demonstration with the electric light.

Here in a few words we have a statement of the arrangement used in the microscope, and a moment's thought will show that such an arrangement may be applied to the vernier instead of the small lens, to which reference has already been made. Nay, we can go further than this, it may be applied to the circle itself, and help us to measure small fractional divisions of its parts with yet greater accuracy than is possible by the aid of the vernier. The way in which this is managed is as follows:—The microscope is turned towards the circle, so that its divisions may be plainly seen in the field of view, and the position of the wire, on, or between any division may represent a certain position which is to be measured by means of the circle. The micrometer head may now be used to tell us the exact distance in 1/1000ths of a revolution between the position occupied by the wire in the first instance, and the position of the wire when it exactly lies on the next division. By determining, according to the graduation of the circle, the number of thousandths of parts as indicated by the micrometer which lie between each division, it is obvious that the exact angular distance between such a position and the next division of the circle can be accurately determined. Such an operation as this is called a "run," and practically such a

system as this is adopted in reading all large circles. But when it is a question of measuring smaller arcs, the micrometer may be used with the telescope itself, its wires appearing with the image of the object in the field of view.

A description of an experiment will perhaps convey a better idea of what can be done in this way. Fig. 15 represents the arrangement. The condensing lens of the lantern having been removed, the light is allowed to impinge upon a lens, A, placed at a slight distance from the lantern. Its action on the light causes a reversed image of the poles to be produced in the air. The light coming from this image is then made to pass through another lens, B; the reversal is corrected, and a magnified image of the poles of the electric arc is thrown upon the screen. The first lens which forms the image may be regarded as the object-glass of a telescope, whilst the other lens which throws the magnified image upon the screen is the counterpart of the telescope's eyepiece. Now if at C, where the image is formed in air, the micrometer wires are placed, they, with the image of the poles, will appear magnified on the screen.

In this manner bodies appear with the wires in the field of vision of the telescope, and their diameters and the dimensions of different parts of them may be most accurately determined. Up to the present time we have been concerned simply with accurately determining the positions occupied by the various bodies which people space. But with this micrometer in the field of view of the telescope something more than this may be done. We may now determine some measurements upon the bodies whose positions alone we have been considering up to now. For instance, the image of a planet may be grasped by the wires, one wire bounding one limb of the planet, the other wire lying along the other limb. Then, knowing how many complete turns, and 1/100th, and 1/1000th parts of a turn have been given to the head of a screw in order that the wires may be separated through such a distance, and knowing also the value of these divisions in seconds of arc, the diameter of the planet may be measured. In like manner, the heights of lunar mountains may be ascertained by measuring the lengths of the shadows thrown by them. Or it may be a question of the distance between two stars close together. The method is still the same. One star is made to lie along the movable wire, the other is seen on the fixed one, and the distance through which the wires are separated ascertained. Having attained to this, let us bring our inquiry into angular measurement to a close.

In passing, as we shall in the sequel, from the measurement of space to the measurement of time, it will be found that the difficulties have been grappled with very much in the same way. In this measurement of space we began with simple instruments, and only by a slow growth has the modern instrument been arrived at; yet notwithstanding the immense changes that have taken place, the pointer and circle of the old instrument are still represented in the new, whilst the vernier and micrometer, still preeminent, enable a degree of fineness to be attained quite unparalleled in the old days. But in passing from these older instruments, in which the circle was so prominent a feature, and the pointer so small, to the more modern instruments, it will be seen that, although both are still preserved, a great change has taken place. The pointer, represented by the telescope, is the prominent part of the instrument, whilst the circle is hidden away almost out of sight.

J. NORMAN LOCKYER

(*To be continued.*)

TABLE OF DIFFERENT VELOCITIES EXPRESSED IN METRES PER SECOND

THE following table has been drawn up by Mr. James Jackson, Librarian to the Paris Geographical Society:—

	Metres per second.
A man walking 4 kilometres an hour	1·11
,, ,, 5 ,, ,, ...	1·40
The comet of Halley in aphelion	3·25
A ship going 9 knots an hour (9 × 1852 metres)	4·63
Ordinary wind	from 5 to 6
A ship going 12 knots an hour (12 × 1852 metres) ...	6·17
A wave 30 metres in magnitude with a depth of 300 metres	6·81
A ship going 17 knots an hour (17 × 1852 metres) ...	8·75
A fresh breeze	10
A torpedo boat going 21 knots an hour (21 × 1852 metres)	10·80
A race-horse trotting an English mile in 2 min. 14 sec.	12
,, ,, galloping 900 metres a minute	15
An express train running 60 kilometres an hour	16·67
Flight of a falcon, of a carrier pigeon	18
A wave in a tempest at sea	21·85
An express train running 60 English miles an hour (60 × 1609 metres)	26·81
A tempest	from 25 to 30
The transmission of sensation by human nerves	33
A hurricane	40
Flight of one of the swiftest birds	88·90
Velocity of a point on the equator of Mercury	146·87
Propagation of the tide caused by the earthquake of Arica on August 13, 1868 (Arica to Honolulu), according to Hochstetter	227·38
Velocity of a point on the equator of Mars	244
,, sound in the air (+10° C.)	337·20
,, a point on the equator of Venus	454·58
,, ,, ,, the Earth	463
A cannon ball	500
Propagation of the movement of tides (North Pacific Ocean): maximum according to Whewell	922
The moon's revolution round the Earth	1012
Velocity of a point on the equator of Mercury	1034
Revolution of the second satellite of Mars	1157
Concussion of the earthquake of Viège (July 25, 1855); from Turin to Geneva in 126 seconds	1368
Velocity of sound in water (+8°·1 C.)	1435
Revolution of the first satellite of Mars	1833
Velocity of a point on the equator of the Sun	2028
Revolution of the fourth satellite of Uranus	3300
,, ,, eighth ,, Saturn	3738
,, ,, third ,, Uranus	3814
Velocity of a point on the equator of Uranus	3904
Revolution of the satellite of Neptune	4505
,, ,, second satellite of Uranus	4906
,, of Neptune round the Sun	5390
,, of the first satellite of Uranus	5763
,, ,, seventh ,, Saturn	5794
,, ,, sixth ,, ,, ...	6398
,, of Uranus round the Sun	6730
Proper movement of Vega	7000
Displacement of the sun towards the constellation of Hercules	7642
Revolution of the fourth satellite of Jupiter	8359
,, of Saturn round the Sun	9584
,, of the fifth satellite of Saturn	9741
Velocity of a point on the equator of Saturn	10,541
Revolution of the third satellite of Jupiter	10,869
,, ,, fourth ,, Saturn	11,516
Velocity of a point on the equator of Jupiter ...	12,491
Revolution of Jupiter round the Sun	12,924
,, of the third satellite of Saturn	13,038
,, ,, second ,, Jupiter	13,999
,, ,, ,, ,, Saturn	14,568
,, ,, first ,, ,, ...	16,425
,, ,, ,, ,, Jupiter	17,667
,, of Mars round the Sun	23,863
,, of the Earth ,, ,, ...	29,516
,, of Venus ,, ,, ...	34,630
Proper movement of Capella	40,000
Revolution of Mercury round the Sun	47,227
Proper movement of Sirius	51,000
Ordinary movements of the solar atmosphere	from 30,000 to 65,000
Proper movement of the 61st of Cygnus	71,600
,, ,, Arcturus	85,000
The comet of Halley in perihelion	393,260
Tempests of the solar atmosphere	402,000
Electricity; a telegraphic submarine wire	4,000,000
,, ,, aërial ,, ...	36,000,000
Velocity of light	300,400,000

THE BRITISH ASSOCIATION

REPORTS

Report of the Committee on Electric Standards, read by Mr. R. T. Glazebrook.—This comprised an account of the means

adopted at the Cavendish Laboratory for comparing resistance coils sent to be tested with the B.A. units.

Report of the Committee on the Harmonic Analysis of the Tides was read by Prof. Adams, who said that the Indian Government were entitled to great gratitude for carrying on tidal observations for many years on a thoroughly scientific system, and he thought they might be held up as an example to our own Government, which was very niggardly in these matters. He suggested that tidal observations should be made on the coast of the English Channel, as there were certain peculiarities in the tide which ought to be investigated.

Report of the Committee appointed to Cooperate with the Scottish Meteorological Society in making Meteorological Observations on Ben Nevis was drawn up by Mr. Murray for Prof. Crum Brown. Stations had been established at various points at which observations were made by the observer, Mr. Wragge, half-hourly as he ascended or descended the mountain. Simultaneous observations were made at Fort William. The diurnal curves for pressure, temperature, and humidity have been drawn, and show the insular character of the climate markedly. At the top the degree of saturation and its persistency is important. In 1881 this was a specially marked feature under the approach of numerous cyclones from the Atlantic; in 1882 anti-cyclones prevailed, and there were frequent changes from saturation to extreme dryness—a dryness comparable with that of the Sahara—occurring in very short intervals of time. This extreme dryness was accompanied by a very high temperature. The results are to be analysed shortly by Mr. Buchan for the Scottish Meteorological Society.

Sixteenth Report of the Committee appointed for the Purpose of Investigating the Rate of Increase of Underground Temperature Downwards in various Localities of Dry Land and under Water was read by Mr. J. Glaisher. The observations were made at an artesian well at Southampton, Dolcoath Mine, North Seaton Colliery, Ashton Moss Colliery, and Ashton-under-Lyne. The report concluded by stating that if we assumed 1° in 57·8 feet as the rate for the St. Gothard Tunnel, and also for the Mont Cenis Tunnel, the effect upon the general mean for all places will be to make it 1° F. in 60 feet, instead of 1° F. in 64 feet.

SECTION A—MATHEMATICAL AND PHYSICAL SCIENCE

On the Forms of the Sun's Influence on the Magnetism of the Earth, by Prof. Balfour Stewart.—The author endeavoured to show that there are two forms of solar influence:—

A. That which produces the diurnal variation, the range of which is greatest a little after the time of maximum sunspot frequency.

B. That which produces a magnetic change on the earth as a whole; and this, too, acts in such a manner that the intensity of terrestrial magnetism is apparently greatest after a maximum of sunspots.

It is with this second form of influence that the author deals, and he endeavours to show that it leads—

1. To simultaneous increments or decrements of the horizontal force at various states as observed by Broun.

2. To two maxima of horizontal force at the solstices and two minima at the equinoxes, as observed also by Broun.

3. It appears that the strength of solar action, when the sun is favourably disposed with regard to the northern hemisphere (June) is greater than when it is favourably disposed with regard to the southern hemisphere (December), a circumstance which leads to an annual variation.

4. It also appears that it is what has been termed the induction system of the earth that is thus affected by the sun.

On the Heating Power of the Sun's Rays at London and at Kew, by Professors Roscoe and Balfour Stewart.—The observations discussed were made by Campbell's method, in which a spherical glass lens had its focus along the surface of a hemisphere of wood, so that, whenever the sun shone, a portion of the wood was burned. The wooden hemisphere was renewed twice every year, namely, at the solstices. The following results were obtained from the observations, by a process in which the burned volume was filled up and the increase in weight accurately measured:—

1. The heat of the sun for the half year between the June and the December solstice is greater than that for the half year between the December and the June solstice, but this difference is more marked at London (Board of Works) than at the Kew Observatory.

2. The annual value of the sun's heat is greater at Kew than at London in the proportion of 100 to 58.

3. The annual value of the sun's heat is peculiarly great about the period of maximum sunspots, but there are indications of a double heat-curve for one of sunspots, so that there is a subsidiary maximum of heat about the period of minimum sunspots.

On Supposed Sunspot Inequalities of Short Period, by Prof. Balfour Stewart and William Lant Carpenter, M.A.—Putting aside in the meantime the question of true or nearly apparent periodicity, the authors exhibited certain results obtained by this application of a method for detecting unknown inequalities in a mass of observations. This has been applied to thirty-six years' observations of sunspots, which have been divided into three series of twelve years each. Two apparent sunspot inequalities of about twenty-six days come out very prominently by this treatment, appearing for each of the twelve years in the same phase, and to very nearly the same extent. If the average value of sunspots for each year be reckoned = 1000, this is reduced to less than 900 at the minimum of the inequalities, and increased to more than 1100 at their maximum, so that the average range is more than one-fifth of the average value, a result of very considerable prominence.

Prof. Chandler Roberts read a paper *On the Rapid Diffusion of Molten Metals*. The two metals chosen were lead and gold inclosed in a letter U-shaped tube, the lead occupying the lower portion of the tube, and the gold being put in at the top of one limb. After about forty minutes Prof. Roberts found that the two metals had been thoroughly mixed.—Sir W. Thomson called attention to the extreme importance of this with reference to metallic alloys, and remarked that it resembled the diffusion of gases or of heat in a gas rather than of a solid in a liquid. Salt would take years to diffuse in a similar manner through water.

Mr. W. G. Black described a simple form of marine anemometer, in which the pressure of the wind on a sail of known area was registered by a spring balance. The sail could be easily placed in any required position on the ship, and set by means of a vane to the proper angle with the wind.—The arrangement was criticised by Prof. Hele Shaw, who described a form of his own which he had used for measuring water currents. This consisted of two light vanes, movable about the same vertical axis, and pressed outwards by a spring. The wind tended to make the vanes close up together, and their motion gave an indication of its velocity.

Papers on *The Standard of White Light*, and on *The Relation between Temperature and Radiation*, by Capt. Abney, and Sir C. W. Siemens respectively. Capt. Abney suggests as a high temperature standard an incandescent lamp. The light of this is compared by means of the spectrophotometer with that from Prof. Vernon Harcourt's standard lamp, afterwards described. The green light in the neighbourhood of E should be about one and a half times that of the gas standard, while the red light should be the same in the two. In a recent paper Capt. Abney criticised some of Sir W. Siemens's experiments of a similar nature. Sir William had used platinum wire in air instead of carbon in a vacuum, and the paper read was a reply.—In the discussion Dr. Schuster pointed out that a similar method, free from many of the difficulties under consideration, had been suggested by the late Prof. Clerk Maxwell, and apparatus for making the experiments was constructed by him shortly before his death.

Prof. Vernon Harcourt gave a description of a lamp for producing a standard light. It was arranged for burning air and the vapour of petroleum, mixed in the proportion of three cubic inches of vapour at a temperature of 60° F. to one cubic foot of air. The mixed gas is allowed to escape from a hole of a quarter of an inch in diameter, and burn in a flame two and a half inches high. Prof. Harcourt showed that the height of the flame was an index of the proportion in which the gases mixed, and was constant when the mixture remained constant.

Mr. E. P. Culverwell read a paper *On the Probable Explanation of the Effect of Oil in calming Waves in a Storm*. He said when the surface of the sea had become smooth after a storm it was very common for long rollers to break on a sandbar. If there were no wind and the sea was glassy, these would not break until quite close to the shore, even though the ordinary theory pointed to their breaking earlier, unless there was a force directed opposite to that of their motion. When exerted on the waves, such a force might be supplied by the wind; but if it rose in any direction the waves broke much sooner. This result

was therefore due to some secondary effect produced by the wind pressure, and not directly by the pressure itself, and it was to the ripples produced on the surface, which disturbed the wave motion, that the speedy breaking was to be attributed. It was, however, a direct result of the theory that the ripples depended on surface tension for their propagation, and could not exist in large amount on the oiled surface. It was also evident that the hold of the wind on the wave was greatly decreased by the absence of ripples, and thus the oil acted both to prevent the wind having much effect on the surface, and also to modify the motion of the water in the wave.

Prof. Stokes read a paper by Dr. Huggins *On Coronal Photography without an Eclipse.* In a paper read before the Royal Society some time back, Dr. Huggins had shown that it was possible by isolating, by means of properly chosen absorbing media, the light of the sun in the violet part of the spectrum to obtain photographs of the sun surrounded by an appearance distinctly coronal in its nature. These researches have been continued, using a reflecting telescope by the late Mr. Lassell, and a film of silver chloride as the sensitive plate on which the photograph is taken. These plates are sensitive to the violet light only, and therefore it was unnecessary to use absorbing media which had proved a source of difficulty to sift the light. Fifty photographs in all were taken and examined afterwards by Mr. Wesley, who made drawings of them for the paper.—Dr. Ball, who was in the chair, examined some of the plates, and spoke of the interest and importance of this communication.

Prof. Schuster read a paper *On the Internal Constitution of the Sun.* He had calculated the volume of the sun from its mass, assuming that it consisted of a gas subject to gaseous laws and in the state of convectional equilibrium discussed by Sir William Thomson. The paper showed that, if the rates of the specific heats of the gas were less than 1·2, the volume of the sun would be immensely larger than at present, while, if greater than 2·0, the sun's volume would be far smaller than it is. The result that the rates of the specific heats must lie between 1·2 and 2·0 is so far in agreement with received theories of the constitution of the sun.

Notes on some recent Astronomical Experiments at High Elevations on the Andes, by Ralph Copeland.—These experiments were made during the first half of the present year at the cost of the Earl of Crawford. At La Paz, in Bolivia, 12,000 feet, with the full moon in the sky, ten stars were seen in the Pleiades with the naked eye, and also two stars in the head of the Bull that are not in Argelander's *Uranometria Nova.* The rainy season lasted roughly until the end of March, after which there was a large proportion of fine sky. At Puno, on Lake Titicaca, 12,600 feet, with a 6-inch telescope mounted on a lathe headstock, a number of small planetary nebulæ, and some stars with very remarkable spectra, were found by sweeping the southern part of the Milky Way with a prism on Prof. Pickering's plan. The most remarkable stars had spectra reduced almost to two lines, one near D, and the other beyond F, with a wave-length of 467 mm., and apparently identical with a line in *some* only of the northern nebulæ as observed by Mr. Lohse and Mr. Copeland. A few close double-stars were also found, amongst them β Muscæ.

At Vincocaya, 14,360 feet, the solar spectrum was examined with a somewhat damaged instrument. The chief fact noted was the relative brightness of the violet end of the spectrum. With a small spectroscope several lines were seen beyond H and H_2. The prominences were visible with almost equal facility in C, D_3, F, and H_8. Attempts to see the corona proved futile, nor were the prominences seen otherwise than in the spectroscope, the only difference being that the slit could be opened far wider than down at the sea-level. A most careful examination of the zodiacal light failed to show even the slightest suspicion of a line in its spectrum, which was continuous although short. Both at Puno and Vincocaya the air was very dry the relative humidity there and at Arequipa, 7700 feet, being as low as 20 per cent. At Vincocaya the black bulb at one time stood above the local boiling point, while the wet bulb was coated with ice. The author was of opinion that an observatory might be maintained without discomfort up to 12,000 feet, or even a little higher—the night temperature falling only slightly below the freezing point. At greater elevations the thermometer falls 1° for every 150 feet of height, the barometer sinking about 0·1 inch for the same change. At 15,000 feet it will thus be seen that arduous winter conditions are reached without any very material gain in the transparency of the atmosphere. From information received it seems possible to maintain a station for a short time in the early summer as high as 18,500 feet; later on the rains set in and render travelling very difficult. Railway and steamboat communication enable instruments of any size and weight to be carried as high as 14,660 feet, and as far as the Titicaca shore of Bolivia.

OUR ASTRONOMICAL COLUMN

Pons' Comet.—The following ephemeris is deduced from MM. Schulhof and Bossert's provisionally corrected elements:—

At Greenwich Midnight

1883.		R.A. h. m. s.	Decl. ° ′	Log. distance from Earth.	Log. distance from Sun.
Oct.	16	16 39 53	+55 37·7	0·2653	0·2723
	18	— 42 19	55 13·7		
	20	— 44 57	54 50·0	0·2520	0·2600
	22	— 47 47	54 26·5		
	24	— 50 48	54 3·3	0·2378	0·2472
	26	— 54 1	53 40·4		
	28	16 57 27	53 17·7	0·2226	0·2340
	30	17 1 5	52 55·3		
Nov.	1	— 4 56	52 33·1	0·2065	0·2204
	3	— 9 0	52 11·2		
	5	— 13 18	51 49·4	0·1894	0·2062
	7	— 17 49	51 27·6		
	9	— 22 37	51 5·9	0·1710	0·1916
	11	— 27 40	50 44·1		
	13	— 32 59	50 22·0	0·1513	0·1764
	15	— 38 35	49 59·6		
	17	— 44 29	49 36·8	0·1303	0·1607
	19	— 50 42	49 13·4		
	21	17 57 16	+48 49·1	0·1079	0·1445

The intensity of light will be three times greater on November 21 than on October 16, and will increase until near the middle of January. According to the experience of 1812, we might expect it to draw within naked-eye vision at the beginning of December, but it is not likely to attain a brightness at all comparable with the conspicuous comets of the last few years. It may rather be anticipated that when best seen, its light will be nearly that of stars of the third magnitude. We are of course assuming the comet not to have undergone material change since its last appearance. On the morning of August 18, 1812, the Paris astronomers have the note:—"La comète commence à être visible à l'œil nu; son noyau assez brillant, est enveloppé d'une chevulure et sa queue est d'environ 1½° à 2°." Employing MM. Schulhof and Bossert's final orbit, we find that at the hour of observation, about 2h. 30m. a.m. G.M.T., the comet was in R.A. 114° 24′, Decl. +40° 27′, distant from the earth 1·4713, and from the sun 0·9449, so that the intensity of light, expressed in the usual way, would be 0·52, which corresponds to that on December 1 in the present year. On the morning of September 14 it was remarked:—"La queue de la comète est divisée en deux branches parallèles; sa longueur paraît d'environ 3 degrès." At 4h. 30m. a.m. G.M.T. the comet was distant from the earth 1·2324, and from the sun 0·7778, whence, the earth's radius-vector being 1·0051, the angle at the comet was 54° 29′, and with Newcomb's solar parallax, the real length of the tail, if extending as most usual in the direction opposite to the sun, would be 7,600,000 miles, or a little over.

In announcing the discovery of this comet by Pons at Marseilles on July 20, 1812, Zach remarked (*Monatliche Correspondenz*, xxvi. 270) that it was the sixteenth (? fourteenth) comet which he had independently discovered within ten years. So indefatigable a worker in this direction well deserves that his name should be permanently associated with at least one of his discoveries, and none presents itself as affording a more fitting case than the comet of 1812.

Swift's Cometary Object.—It would appear from unsuccessful search at European and American observatories that Mr. Swift must have been mistaken in supposing he had observed a comet in the places published in *Astron. Nach.*, No. 2541.

The Cordoba Observatory.—Dr. B. A. Gould, director of the Observatory at Cordoba, passed through London last week *en route* for South America, after attending the meeting of the "Astronomisches Gesellschaft" at Vienna, and that of the International Standard Commission at Paris. We learn from Dr. Gould that the printing of the second volume of Cordoba Zones is nearly completed in London. The attention of this

indefatigable astronomer will soon be directed to the publication of another great work undertaken by him at the Argentine National Observatory, viz. the Cordoba General Catalogue of Stars.

UNIVERSITY AND EDUCATIONAL INTELLIGENCE

OXFORD.—The commencement of the Michaelmas Term has been marked this year by an event of happy augury for the advancement of science in Oxford. Prof. Burdon-Sanderson opened the physiological depar ment in the University Museum with an inaugural address, in which the aims and scope of physiology were defined with scientific accuracy and singular literary charm. The Professor showed the great importance of physiological methods for the advance of pathology, and ended by promising his future students something more interesting to study than "dry bones."

Prof. Sanderson gives a regular course of lectures on the "Mechanical Functions of the Animal Body," and the physiological laboratory is open for practical instruction under the Professor and two assistants.

Mr. Yule also has a class at Magdalen for Practical Physiology.

In the Department of Animal Morphology Prof. Moseley lectures on Comparative Anatomy, each lecture being followed by practical instruction. Mr. W. H. Jackson lectures on Mimicry and Parasitism; Mr. E. B. Poulton on the Fundamental Tissues, and Mr. W. L. Morgan on the Limbs of Vertebrata.

Mr. E. Chapman has a class at Magdalen for the study of Vegetable Histology.

In the Physical Department Prof. Clifton lectures on the Properties and Means of Measuring Electric Currents; practical instruction in Physics is given by Prof. Clifton and Messrs. Heaton and J. Walker. Mr. Heaton gives a course of lectures on Mechanics.

At Christ Church Mr. Baynes has a class for practical instruction in magnetic and electrical measurements. At Balliol Mr. Dixon gives an elementary course of lectures on Electricity and Magnetism.

In the Chemical Department Prof. Odling lectures on the "Naphthalene Family." Lectures on Inorganic and Organic Chemistry are given by Mr. Fisher and by Dr. Watts. Practical instruction is given by Mr. Fisher, Dr. Watts, and Mr. Baker. At Christ Church Mr. Vernon Harcourt forms a class for "Examples in Quantitative Analysis."

Prof. Prestwich lectures on the "Elements of Geology." Prof. Story-Maskelyne lectures on "Crystallography."

The Natural Science Scholarship at Exeter College has been awarded after examination to Mr. E. H. Cartwright, of Charterhouse School.

Natural Science Scholarships are offered for competition this term by Christ Church and Balliol Colleges.

CAMBRIDGE.—The outgoing Senior Proctor, Mr. Torry, in his address on laying down office, referred to the present system of granting M.A. degrees without examination, and suggested that all who had not already taken honours should be required to pass for M.A. in some specified portion of one of the honours examinations.

Prof. Darwin will lecture this term on gravitation, and consider some of the mathematical problems which arise in the theory of the figure of the earth, measurements of base lines and arcs of meridian, pendulum experiments, the Cavendish experiment, and cognate subjects.

The Demonstrator of Mechanism will take a class in rigid dynamics, with a view to its applications in engineering; and also a preparatory class in the differential calculus.

At the annual election to Fellowships at Trinity College, Mr. R. A. Herman, Senior Wrangler and First Smith's Prizeman in 1882, was elected a Fellow. Mr. W. R. Sorley was elected to the Fellowship given for mental and moral science.

The election to the Knightbridge Professorship of Moral Philosophy will take place on November 1. The electors are Professors Caird, Fowler, Hort, and Seeley, Drs. Campion and Todhunter, Mr. Leslie Stephen, Mr. Venn, and the Vice Chancellor.

Prof. Cayley lectures this term on higher algebra and the theory of numbers.

The Demonstrator of Comparative Anatomy is conducting an advanced class on the Protozoa and Cœlenterata.

Messrs. A. J. C. Allen (Peterhouse), and C. Graham (Caius), have been appointed moderators for the year beginning in May next.

Prof. Garnett, Dr. Vines, and Mr. Pattison Muir are appointed examiners for the first M.B. examinations; Prof. Milnes Marshall, Dr. Gaskell, and Dr. Shuter for the second M.B. examinations of the current year.

Mr. Stearn is lecturing on electrostatics at King's College, with special reference to theories of electric displacement, specific inductive capacity, and the strain in a dielectric.

SCIENTIFIC SERIALS

Bulletins de la Société d'Anthropologie de Paris, tome 6, série 3, 1883.—In a discussion on polyandry in Cashmere and Thibet, M. Olivier Beauregard maintained that this practice prevailed among the early Aryan races of Hindostan 2000 years before the Christian era, as shown in the first book of the Mahâbhârata, from which he made several interesting extracts bearing on this point. His views were strongly contested by M. Ujfalvy.—"Remarks on the character of the crania of native South Australians," by M. Cauvin, who made a series of anthropometric determinations while engaged at Sydney in prosecuting his researches into the morphological characteristics of the Oceanic races.—M. de Ujfalvy, in a communication on the "Traces of the Ancient Cults of Central Asia," described the various superstitions which point to an earlier Vedic faith, and to a fire-worship among races who now adhere either to Hindooism, or Islamism, while in the heart of Central Asia the majority of the tribes are still followers of the "Old Man of the Mountains," or the belief of the "Assassins." He believes that the introduction of *Mazdeism* and Brahmanism was probably contemporaneous, and that these ancient cults were preceded by a form of Shamanism in which the products of nature were worshipped.—On human sacrifices among the Khonds of India, by M. E. Reclus. The author regards these so-called *mériahs* as a survival of an early practice of the ancient agricultural tribes of Asia, who believed that blood was necessary to the fertility and nutritive qualities of the fruits of the earth.—On the population of Western Laos, by M. Carl Bock. This memoir is remarkable for its minute ethnographic details and for the number of its anthropometric determinations, and treats of the political and social relations of the six Laotian States which pay tribute to Siam.—A discussion on the supposed practice of the "*Couvade* among the Basques," in which M. Vinson, who has been twelve years resident among the people, denies the existence among them of any such custom, and gives his reasons for doubting the assumed affinity of this people with the Iberians. M. E. Reclus thinks the existence of such a practice might perhaps be connected with the transition from the metronymic to the patronymic principle of family government; and that from an ethnological point of view the question of its reality, to which many of the best known classical authors have given their testimony, is worthy of attention.—On the prehistoric lasso, by M. Chauvet.—Report, by M. Nicaise, on the discovery of human bones, associated with Quaternary animal remains and worked flints, in the alluvial deposits of the Marne Valley near Chalons, with plan of locality, &c.—On the significance of the principal humeral of the biceps, by M. Leo Testut, with special reference to the contradictory opinions of Hyrtl and Calori.—Report on the brain of Louis Asseline, by MM. Duval Chudzinski and Hervé, with diagrams of various aspects of the hemispheres. M. Duval's assertion that Asseline's brain presented various simian characters drew forth M. Foley's strongly expressed reprobation, while M. Dally considers that the more general admission of a close anatomical affinity between man and the lower animals would be conducive towards morality, by lessening the cruelties wantonly inflicted on the latter.—Report of commission for the preservation of megalithic monuments, on the remains of dolmens of Port Blanc (Quiberon).—On a prehistoric case of dental abnormality, by Dr. Bernard.—Report on the adjudication of the Prix-Godard for 1883, by M. L. Rousselet, who passed in review the several labours of M. Chantre, to whom the prize has been awarded in consideration of the merits of his palæolithic atlas of France, and for his work on the Iron Age, while M. Prengrueber receives a silver medal, with honourable mention, for his anthropometric measurements of the Kabyles.—On dental erosions in the dog, by M. Capitan.—On the steatopyga of the Boshman women, by Dr. Blanchard.

Journal de Physique théorique et appliquée, September, 1883.—On the critical point of liquefiable gases, by J. Jamin.—On the compressibility and the liquefaction of gases, by J. Jamin.—

Note on the coloured fringes in films of uniaxial crystals, and on their projection in monochromatic light, by A. Bertin.—Optical apparatus for verifying plane surfaces, whether parallel, perpendicular, or oblique, by Léon Laurent (with diagrams).—Theorem relative to ramified linear currents, by L. Thévenin.—Horizontal capillary electrometer, by Ch. Claverie.—Sonorous vibrations of solids in presence of liquids, by F. Auerbach.—A measurement of wave-length in the ultra-red of the solar spectrum, by E. Pringsheim.—Researches on the proportion of carbonic acid in the air, by J. Reiset.—Researches on the proportion of carbonic acid contained in the air, by A. Müntz and E. Aubin.—On the normal amount of carbonic acid in the air, by M. Dumas.—Experimental researches on the thermal conductivity of minerals and rocks, by J. Thoulet.—Analytical researches on the method of J. Thoulet relative to thermal conductivity, by A. Lagarde.—On the diffusion of an impalpable powder in solid bodies, and On pig iron transformed to steel by cementation, by Sydney Marsden.—On the electrolysis of hydrogen peroxide, by M. Berthelot.—Detection of hæmoglobin in the blood by optical methods, by E. Branly.—Measurement of the rotation of the plane of polarisation due to the magnetic influence of the earth, by H. Becquerel.—A new apparatus for determining specific heats, by W. Louguinine.—Reversal of line spectra of metals, by Liveing and Dewar.—Boiling points and vapour tensions of mercury, sulphur, and some complex carbon compounds determined by the hydrogen thermometer, by J. M. Crafts.

Atti of the Royal Academy dei Lincei, June 3.—Obituary notice of the late Rainardo Dozy, with a complete list of the illustrious *savant's* writings, by Sig. Amari.—Remarks on Giunti's researches on the influence exercised by some physical agencies on alcoholic fermentation, by Sig. Cossa.—On the rotatory power of the isomerous photosantonic acid $C_{15}H_{20}O_4$, by Sig. Nasini.—Two important results of Hall's electric phenomenon, by Sig. Blaserna.—On the spontaneous oxidation of mercury, by S. Damiano Macaluso.—On the equilibrium of elastic and rigid surfaces, by S. Giacinto Morera.—On a new method of anæsthesia, obtained by disassociating the motor and sensitive functions of the nervous system, by S. A. Moriggia.

SOCIETIES AND ACADEMIES

Paris

Academy of Sciences, October 8.—M. Blanchard, president, in the chair.—On the force of explosive substances, by M. Berthelot. In reference to the work in two volumes just published by him on this subject ("Sur la Force des Matières explosives d'après la Thermo-chimie, Gauthier-Villars, 1883), the author explains that the theory there advanced is the result of thirteen years' experimental researches reported from time to time in the *Comptes Rendus* of the Academy. The first part is devoted to general notions, and in particular to the development of his theory on the propagation of explosive phenomena and on the explosive wave, the discovery of which throws quite a new light on the whole subject. In the second part are recorded the various experiments and researches made by the author on the electric fixity of nitrogen. In the third part the principles and numerical data thus determined are applied to define in particular the force of detonating gases, nitro-glycerine, nitromanuite, dynamite, gun-cotton, picrates, and other powerful explosive substances. The history of the origin of gunpowder and other explosives is consigned to an appendix, and the work is enriched with full analytical tables and alphabetical indexes.—Report on the earthquake felt at Ischia on July 28, 1883, with remarks on the probable causes of seismic disturbances, by M. Daubrée. The author rejects Prof. Palmieri's view that the catastrophe was connected with the presence of old quarries and other cavities whose supports gave way and thus caused a sudden subsidence of the ground at Casamicciola. He holds, on the contrary, that it was due to the volcanic forces, by which the island has often been wasted, and notably in the years 1828, 1867, and 1881. On the general question of the nature and cause of these disturbances he holds with Dolomieu that they must be regarded as suppressed volcanic eruptions. Gaseous bodies formed in underground cavities, the vapour of water penetrating from the upper crust, subjected to great pressure, sufficiently superheated, and set in motion from time to time by a simple natural process, suffice to account for all the essential phenomena associated with earthquakes.—Reply to a note by M. Thollon on the interpretation of a phenomenon of the solar spectrum, by M. Faye. The author appeals to data supplied by Secchi and others in support of his views and against the reality of the velocity of 100 to 150 leagues per second usually assigned by spectroscopists to the movements of the hydrogen in the solar protuberances.—On the measurement of the forces brought into play in the various acts of locomotion (one illustration), by M. Marey.—On the coexistence in a specimen of guano of effervescent carbonate of ammonium with water and sulphate of potash, by M. E. Chevreul.—On the symmetrical character of the so-called adventive roots in plants, by M. D. Clos.—After the reading of this paper allusion was made by the President to the loss sustained by the Academy in the person of Dr. Oswald Heer, Corresponding Member of the Botanical Section, who died at Lausanne on September 27.—On the financial aspect of the great works of irrigation in France and the north of Italy, by M. Ar. Dumont.—Observations on the Pons-Brooks comet and the planets 142, 185, 221, and 234 made at the Paris Observatory (equatorial of the West Tower), with note on the remarkable variation in brightness of the Pons-Brooks comet, by M. G. Bigourdan.—On a remarkable peculiarity presented by the tail of the great southern comet of 1882, by M. L. Cruls.—On the approximate evaluation of integers, by M. Stieltjes.—On the induction produced by the variation of intensity of the electric current in a spherical solenoid, by M. Quet.—On the products formed in the fermentation of the sugar-cane due to the properties of the soil, by MM. Dehérain and Maquenne.—On the wheats of India, by M. Balland. The specimens of Indian wheaten flour examined by the author revealed the presence of about 3 per cent. of *Vicia peregrina*, *Cicer arietinum*, and other leguminous flours.—On poisoning by the bacilli of the jequirity (one illustration), by MM. Cornil and Berlioz.—On the influence of beet-pulp on the milk of the cow, by MM. Andouard and V. Dézaunay. From their experiments the authors conclude that the milk of cows fed on beet-pulp increases in quantity, but deteriorates in quality.—On the geological age of the serpentine rocks and ophiolithic formations of Corsica, by M. Dieulafait.

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THURSDAY, OCTOBER 25, 1883

A SCIENTIFIC CATALOGUE

Bibliotheca Historico-Naturalis et Mathematica. Lager-Catalog von R. Friedlander und Sohn. (Berlin, 1883.)

LIKE enthusiastic physicians who are charmed with a "splendid case"—of Asiatic cholera, it may be—which illustrates or disproves some theory which has engaged their attention, there are philologists who are so interested in tracing the growth of variations among dialects by watching for and marking fresh changes in a parent language under different circumstances that they do not consider what an inconvenience this polyglot condition of society is, and what a length of time and amount of labour all but a gifted few have to expend in order to learn even three or four of the principal languages of ancient and modern times. Some considerations lead us to hope that, following many other benefits that scientific study has unweeningly brought to man, a unification of languages also may be in store.

As science spreads and makes way, the more indispensable to inquirers in each country becomes the knowledge supplied by the phenomena or the intellect in all others. Knowledge cannot be largely produced by a kind of secret manufacture of which one country or one race only knows the process. It must be *patent* in the older sense! Inductive science requires such a variety of observation of positive fact, and is so largely helped by comparison of working theories, that hardly any subject can be thoroughly studied without consulting both the facts noted by observers and the hypotheses started by philosophers in other countries. Commerce has, no doubt, brought a large number of men from many parts of the world into oral communication, but they are not of the class who have the means or the ambition to guide a language, as a majority of scientific writers are,—men who must largely control the education of their country.

But we have not arrived anywhere near the harbour of a common language yet. The first step has no doubt been taken by the agreement to use Greek roots for all scientific terms, so many of which keep forcing their way into familiar language through the utilitarian purposes connected with them; and science may claim its share also in the recent and increasing disuse of the old black-letter type by the Germans, and the adoption of the more general Arabic character—as in this publication, to which we are much pleased to call attention as a step towards counteracting the inconvenience now laboured under through the results of the tendency of languages to diverge. We have in England a fair sprinkling of libraries in which tolerably complete collections of English works are to be found, and the narrow boundaries of our crowded population make the use of them pretty practicable to the working student. But only a very few indeed of these contain at all complete collections of foreign publications; and, without doubt, the cosmopolitan studies of the Germans, their numerous universities—each, as Prof. Ray Lankester reminded the British Association, with a Government endowment sufficient at least to allow an earnest worker to follow up any pursuit which has raised his enthusiasm, and each as a matter of course engaged to some extent in original research—make their country the home and the market for such a collection of books as this. The special characteristic of this list is that it is restricted to science. The publisher was a successful student at the University of Berlin, where natural science was his favourite branch An American friend persuaded him to continue his studies at one of the United States Universities. He made many friends there, but his father's death brought him back to Europe, and the large family of them which were left required him to give close attention to business. His knowledge, however, of science, and his connections in the United States, enabled him to get, and to execute with more than usual success, large orders for the different great libraries as they were successively founded there. He made it the work of his life to form as complete a collection as possible of all scientific books and publications, and the results are shown in this book, printed with a care which foreign writers would seldom find bestowed upon their names and upon the titles of their books in an English printed catalogue, and, although containing about 1250 pp., deserving to be classed as a handy volume. Nearly 50,000 entries of publications on science only are made, with the most full particulars as to illustrations, size, date, &c. These are divided into 169 catalogues of works upon as many different subjects, upon which very elaborate classification we must remark that while no doubt suiting any consulters who only wanted a choice of books upon a subject, the dividing and classifying must add immensely to the labour expended upon it, and nevertheless reduce the value of the catalogue to the very students for whose benefit we are told that the compilation was made, viz. workers who wanted to know what upon each subject had been written in all scientific countries. For, as any one who glances down the index of subjects would see, there are very many books which are equally appropriate to half a dozen of them; and, since the same work is not repeated in list after list, it is necessary to consult an unknown number of them before the catalogue has answered the purpose intended. Of course it is difficult also to bring so general a list down to any date close upon that of publication, important as that is to all scientific writers especially, and the omissions which may be traced in this great collection are a striking evidence of the wealth of modern scientific literature.

THE FISHERIES OF THE ADRIATIC

The Fisheries of the Adriatic and the Fish thereof. A Report of the Austro-Hungarian Sea-Fisheries; with a Detailed Description of the Marine Fauna of the Adriatic Gulf. By G. L. Faber, Her Britannic Majesty's Consul, Fiume. (London: Bernard Quaritch, 1883.)

NO comprehensive work has till now appeared in English on the sea-fisheries of the Austro-Hungarian Empire, and though Mr. Faber modestly refers to his volume as a Report meant to pave the way for a more general work on the subject, yet we cannot but regard it as a very valuable history of the marine fauna and fishing interests of the Adriatic. The volume contains a systematic list of the fishes, including the freshwater forms of the watershed of the northern and eastern shores of the

Adriatic, which has evidently been compiled with a great deal of care. The Italian local names in use on the Adriatic coasts and the Croatian names are also given; those of the latter dialect for the first time.

The author commences with a short description of the Adriatic Gulf. Its tides are inconsiderable, the normal rise and fall being only 1½ foot, and only one ebb and flow in twenty-four hours. The currents, however, are numerous, acting as modifiers of the effects of climate and influencing by their agency the diffusion of marine life. The nature of the sea bottom varies immensely, giving abundant choice to the various species of fish. The sea water proper is, in respect of the degree of saltness, about the same as the Atlantic under the tropics, but springs abound in some regions to such an extent as to render the surface water thereof quite fresh. With a for the most part moderate depth, yet 100 fathoms is reached near the islands of Zuri and Scoglio Pomo, and near the island of Meleda the bed has not been reached at 500 fathoms.

In the second chapter we find a history of the present state of the fisheries. The demand now exceeds the supply. The decrease is ascribed to the effects of trawling, though without the slightest reason. One great drawback to the preserving of fish seems to be the State monopoly of salt. Full details as to the fishing of Italian boats in Austrian waters are also given. In the third chapter the various fishing districts and their peculiar products are detailed; besides fish, sponges are obtained in the vicinity of Crapano and coral near Zlarin. Pola is the best district in Istria for the tunny; it is now a town of 20,000 inhabitants, in 1856 it was a village of but 600 inhabitants. In value the sardine fishery holds the first rank, being computed at about 40,000*l.* a year, while the tunny fishery yields about 15,000*l.*, and the red mullet take is calculated at 12,000*l.* The average annual value of squid (*Loligo sepiola*) captured is 12,000*l.* The various sorts of craft used in fishing are described and figured in Chapter IV., with calculations of their value, number of crew, &c. Nets, basket traps, fish weirs and ponds are treated of in Chapter V. The tanning process is effected by a solution of the bark of *Pinus maritimus* in sea water, but for very fine nets the leaves of the pistachio, shumac, myrtle, and heath (*Erica vulgaris*) are used. Chapter VI. describes the hooks and lines used. Chapter VII. treats of the names applied to fishermen and various modes of fishing. The fish markets of the Istrio-Dalmatian coast are described in Chapter VIII. The well-defined sorts which appear in these markets may be given at ninety fishes, often uniting under one name various species of the same genus, thirty mollusks, and ten crustacea, but in addition there will be found sea urchins (Echini), an actinia (*A. cereus*), and such an ugly form as *Ascidia microcosmus*. The methods of curing and cooking fish are detailed in Chapter IX. The curing of pilchards in oil after the fashion of sardines seems to have met with a well-earned success, but the tins have to be imported from England and the oil from Italy or France. Chapter X. is devoted to statistics; those of the Austrian sea-fisheries are compiled with commendable exactitude and completeness, and are regularly published in the *Austria*, the statistical periodical of the Austrian Ministry of Commerce. We wish that we could say the same for our British sea-fisheries. The very valuable appendix contains a catalogue of the Adriatic marine fauna, and the local names given to the best known forms. The typography and illustrations of this handsome volume leave nothing to be desired even in these days of luxurious editions. We agree with Dr. Günther in believing that to the great number of persons who annually leave our shores for the Mediterranean in quest of sport and recreation this work will serve as a guide to a field of pleasant research, hitherto much neglected. It is also a most important contribution to the knowledge of the economic resources of the sea-coast and rivers of a deeply interesting country, and we hope that one of the results of its publication may be to greatly develop a practical interest in the fish treasures of the Adriatic Gulf.

OUR BOOK SHELF

Practical Electrical Units Popularly Explained. By James Swinburne (late of J. W. Swan and Co., Paris; late of Brush-Swan Electric Light Company, U.S.A.). (London and New York: E. and F. N. Spon, 1883.)

THE title of this book will doubtless lead those to whom "ohms," "amperes," "farads," &c., seem so mysterious to hope that all difficulty in understanding what they are and whence they come will be removed. They will find, however, that though the relations between the practical units are given very clearly, and are illustrated by many numerical examples, yet the definitions are "definitions in a circle." Mr. Swinburne has neither shown how the C.G.S. units are derived, nor has he even given the relations of the practical to the C.G.S. units.

The mechanical units are fully described on the English system, which perhaps is better suited for purposes of explanation than the French, as being more familiar to most people. In speaking of the term "electric fluid," Mr. Swinburne uses this rather dangerous language: "Electricity can be looked upon as an imponderable fluid which, like a gas, is compressible, the volume varying inversely as the pressure, so that if the pressure be doubled the volume is halved." It does not appear at once to what this refers, but six pages on the meaning is explained, for we read: "It may seem strange at first that there should be a unit of quantity, and another of capacity to hold that quantity, when we do not need to call a pint measure by one name and the quantity of liquid it holds by another. It must be remembered that electricity corresponds to a compressible fluid; and though the pint measure holds, or is supposed to hold, a pint of liquid, the amount of gas it would contain would depend on the pressure." This is one of the many excellent concrete analogies by which Mr. Swinburne assists his readers to understand those actions which at first seem to many so unintelligible.

It is not clear in what way the following note will assist mechanical engineers or any one else to understand the nature of electromotive force. "Force is generally looked upon as what tends to move matter, and the term 'electromotive force' seems therefore a misnomer at first sight. Science does not know what electricity is, but it is supposed to be a kind of motion of molecules or of ether very closely related to heat and light. Science knows little about molecules or ether, and does not even know if there are such things, but thinks the next thing to understanding anything is naming it."

The book is not meant to be a scientific work, but is intended to help mechanical engineers and others to understand the units with which they may have to deal; for this purpose the simple language and the numerous examples will be sure to make it succeed. C. V. B.

The Fishes of Great Britain and Ireland. Being a Natural History of such as are known to inhabit the Seas and Fresh Waters of the British Isles, with remarks on their Economic Uses, and Various Modes of Capture. By Francis Day, F.L.S., &c. (London: Williams and Norgate, 1880-1883.)

THIS new work on the "Fishes of Great Britain and Ireland" is to consist of nine parts and about 200 plates. Of these the first six parts, bringing the pages to 176, and the plates to 132, have already appeared. Not only is the natural history of the marine and freshwater fishes given with very copious synonymy, but we find in addition the habits of the fish detailed, the means of their capture, the artificial breeding, the use for food, and the best methods of cooking given. The scientific merits of the book are such as we might expect from the author of "The Fishes of India," and from one who occupied the important post of Inspector General of Fisheries in India, while there is further, in the accounts of the habits of the fish and of their means of capture, an amount of most interesting details to the general reader and sportsman. The plates are from drawings by the author, and though uncoloured are very effective. In most cases where desirable the stomach and pyloric appendages, the air bladder or the mouth with the teeth are added to the portrait of the species. When completed the work will form a handsome royal octavo volume.

Parrots in Captivity. By W. T. Greene, M.A., M.D., and with Notes on several of the Species by the Hon. and Rev. F. G. Dutton. Coloured Plates. (London: George Bell and Sons, 1883.)

THREE parts of this well-ilustrated work on parrots kept in captivity have already been published, and considering the extent to which these splendidly coloured and interesting birds are to be found domesticated in our country, this treatise on their habits will no doubt be very acceptable to many of our readers. The directions given as to their food seem based on practical experience, and will be welcome to some who in this respect may have wrongly treated some favourite bird. The author insists pretty strongly on not characterising a species by the behaviour of an individual, fairly arguing that it is just as wrong to declare that all the cockatoos are noisy and spiteful or that all the lories are amiable and well-behaved as it would be to declare that all Englishmen are lively or all Frenchmen sad because persons of these nations had been met with having these characteristics.

Voyages of G. S. Karelin on the Caspian Sea. Memoirs of the Russian Geographical Society; Section of Physical Geography, vol. x. 497 pp. (St. Petersburg, 1883.)

M. KARELIN, who died in 1872, in the province of Orenburg, to which he was exiled in 1824, was well known to naturalists in Russia and Western Europe as an indefatigable collector in mineralogy, botany, and zoology, who supplied Russian and foreign museums with rich collections from Eastern Russia and Siberia. But, with the exception of a few papers in botany and zoology, none of his most valuable works have appeared in print. Most of his manuscripts are lost, and of his remarkable journey to the Altaï and Sayan, where he spent several years making his richest collections, only a few fragments of diaries have been discovered. Prof. Bogdanoff publishes now the two diaries that Karelin kept during his journeys to the eastern coasts of the Caspian Sea, performed in small vessels in 1832 and 1836. During the first of these voyages Karelin visited the north-eastern coast and the Gulf of Mertvyi Kultuk; four years later he visited the Gulfs of Astrabad, Krasnovodsk, Kara-Bugaz, &c., and penetrated also into the country, making an excursion into the Astrabad province, and another to the great Balkhan Mountains, where he entered into communication with the Turkomans. All these tracts have been visited and described since, but still the reading of Karelin's diary, which shows a fine observer of the physical characters of the countries visited, and of the people met with, is a real pleasure; while numerous remarks on the flora and fauna, scattered in the diaries, have lost very little, or nothing, of their interest from the more recent descriptions. Both diaries are followed by most valuab.e general descriptions of the flora and fauna of the shores of the Caspian; the lists of species met with, altogether exactly determined, have been revised by Prof. Strauch and M. Gobi, thanks to the numerous collections he made during his journeys. His remarks on the old bed of the A[illegible]u-daria, which he visited and mapped in 1836 as far as 37° E. long., are fully confirmed by recent researches; whilst his descriptions of the nature and inhabitants of the province of Astrabad and of the Turkoman coast, and his remarks on the falling of level of the Caspian, are still as valuable as if they were written to-day. The work is accompanied with maps of the Gulfs of Astrabad, Hassankuli, and Krasnovodsk, and of the Balkhan Mountains, which enable us to conclude as to the changes in the configuration of the coast line during the last fifty years.

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications.*

[*The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to insure the appearance even of communications containing interesting and novel facts.*]

The Green Sun

FOR two or three days we have been having a modified repetition of the phenomena respecting which I wrote you at some length by the last mail; while, curiously enough, if there is no connection between them, the telegraph announces fresh eruptions in Java on the 16th inst. This time, however, while there is apparently about the same smoky haze in the sky, it is much thinner, showing very plainly after the sun has set, but invisible while the sun is much above the horizon. There is also very little of the refracting medium to which I referred in my last, as there is only a slight discolouration of the sun before setting, and scarcely anything of the succession of colours afterward as compared with what we had two weeks ago. I send herewith a few clippings from Indian papers in regard to the matter. The curious appearance of two weeks ago, so far as I can learn, was not seen north of Masulipatam on this side, or Calicut on the west coast. W. R. MANLEY

Ongole, India, September 24

The following cutting, sent us by Mr. Manley, is from the *Englishman's Overland Mail* of September 23:—

Some excite[illegible]ent has been caused in Madras lately by the fact that many per[illegible] have observed that both the sun and the moon presented a g[illegible] appearance when near setting. Prof. Michie Smith thus e[illegible]lains the phenomenon in the *Madras Mail*:—The appearan[illegible] of a green sun is very uncommon so far as I can discover, [illegible] fortunately there is one recorded observation which throws [illegible]uch light on the subject. Lockyer once observed the su[illegible]he of a vivid green when seen through the steam of a h[illegible] paddle boat on Windermere. This at once points to the [illegible]lution of the difficulty, and shows us that the cause of the a[illegible]arance is due to water vapour in the atmosphere. That it is e[illegible]ly due to this I am not prepared to affirm, for some observat[illegible]s of Dr. Schuster point to an influence produced by su pended [illegible]atter in the air. This, however, I think we may neglect a[illegible]resent, and consider why the vapour which usually gives t[illegible]e red sunset tints should at present give green colours. To [illegible]tle this point I have made careful spectroscopic observ[illegible]ns, and, though I have not yet reduced them, I find t[illegible] they indicate a very marked absorption in the red end of th[illegible]pectrum extending nearly to B, with a great development [illegible]he "rain band" near D on the red side accom-

panied by a decided deficiency of the band on the green side, called by Piazzi Smyth "the low sun band." Hence we have less red than usual and more green. This is due, in part at least, to the sunlight passing through a more than ordinary dense stratum of aqueous vapour, for we know that the thicker the stratum of vapour the more is the red light absorbed. But this is not all, for we have quite as much vapour without this green colour, but in these cases the sun, I believe, is not seen at all, but we get strips of green sky which are often seen. The atmosphere then, I believe, contains at present a large amount of vapour existing actually as vapour, and not condensed into clouds; hence even a great thickness of it is transparent except to those particular rays which aqueous vapour absorbs. The green colour can be seen only at a particular altitude, for only there is the thickness sufficient to produce the necessary absorption. At higher altitudes the peculiar pale silvery white is exactly what we are to expect.

WILL you allow me to submit to the further consideration of the competent whether this phenomenon, seen at approximately the same time in Southern India, Ceylon, and the West Indies, could be due solely to the presence in the atmosphere of the vapour of water. Is not the air in these regions normally surcharged through a considerable period of every year with vapour of water? And yet not only is this an unusual appearance, but it has excited, wherever observed, both wonder and some alarm. In one respect the observation from Ceylon (NATURE, vol. xxviii. p. 597) is the most noticeable we have had yet, inasmuch as, even when the sun had attained "the very zenith," his light is said to have continued blue. My doubt is whether a phenomenon so rare could be due solely to a cause so everywhere common. HENRY CECIL

Bregner, Bournemouth, October 22

P.S.—When Mr. Lockyer saw his green sun through the steam on the boat, were there not also mingling with the vapour sulphurous fumes from the funnel?

[The sun has been seen green through mist on the Simplon. —ED.]

Snake Poison

TOUCHING the effect of *Crotalus* venom on vegetable life, I am anxious to repair an error which appears on p. 552 of my work on "Snakes," where Dr. Mitchell is made to affirm that some healthy vegetables inoculated with the poison were "withered and dead next day, as if scathed by lightning." In some notes which I made many years ago on a too cursory reading of Dr. Mitchell's paper,[1] I omitted the inverted commas, which denote that the experiment was tried by Dr. Gillman of St. Louis, in 1854, but which Dr. Mitchell thought was too limited and wanting in detail to be of scientific value. I had overlooked Dr. Mitchell's comments and his own experiments on vegetable life, by which he was driven to the conclusion that the plants were injured by mechanical wounds, and *not* by the venom inserted into them. When writing my chapter under pressure of time long afterwards, I trusted too confidently to those careless notes, and to an impression gained through the old Virginia writers that venom *is* injurious to vegetable life.

But in a most interesting series of experiments twenty-five years ago Dr. Weir Mitchell found that the venom did *not* interfere, nor did it arrest alcoholic fermentation and its accompanying growth of sporules. To test it on the higher vegetable life he wounded plants in various parts of their stem and in various ways, taking three or four plants of similar size and growth—geraniums, tradescantia, and others—both succulent and of woody fibre, inserting venom into some and not into the others which were identical in character, and carefully noting the effects on each, which, for the most part, were similar in the inoculated and the merely wounded plants, the symptoms being such as were produced from the injury to the tissue, the leaf, or stem, as might be. "In many successive efforts to poison other plants with venom," says Dr. Mitchell in summing up the results, "*I failed in every instance.*"

A more careful perusal of Dr. S. W. Mitchell's paper now enables me to offer this explanation of the misrepresentation of those exceedingly interesting experiments, fully detailed in vol. xii. of the "Smithsonian Contributions."

Cleveland, Ohio CATHERINE C. HOPLEY

[1] "On the Venom of the Rattlesnake," by Dr. S. Weir Mitchell, "Smithsonian Contributions to Knowledge," vol. xii. 1860. Washington, D.C., United States.)

Simultaneous Affections of the Barometer

MY thanks are due to Dr. Balfour Stewart for his kindly pointing out that simultaneous movements of the barometer, like those I had described in my paper of January last, and also in the "Brief Sketch of the Meteorology of the Bombay Presidency in 1881," written in August, 1882, were first observed by the late John Allan Broun. Owing to my connection with meteorological work being short—of only fourteen months' duration—my attention had not before been drawn to this fact. It is to me interesting to learn also that the late John Allan Broun considered that there was a connection between these movements and the earth's magnetism.

The *Proceedings of the Manchester Literary and Philosophical Society* for the last few years do not appear to have been received in Bombay, but they have now been applied for.

A. N. PEARSON

Meteorological Office, Bombay, September 14

Table of Different Velocities

IN reading over the interesting table of velocities drawn up by Mr. James Jackson, and published in NATURE to-day (p. 604), there is one item omitted, which the author may like to add to his list, viz. the rate at which detonation travels, as exemplified by a train of compressed gun-cotton. This has been computed by Abel and Nobel to be between 17,000 and 19,000 feet per second, or rather more than 200 miles in a minute. In Mr. Jackson's table, therefore, the detonation of gun-cotton would come in somewhere between the velocity of sound in water and the velocity of electricity. H. BADEN PRITCHARD

Woolwich, October 18

OSWALD HEER

WE briefly announced last week the death, on September 27, at Lausanne, of Dr. Oswald Heer, Professor of Botany in the University of Zurich, aged seventy-four years and twenty-seven days. He was born at Nieder Uzwyl, Glarus, Switzerland, August 31, 1809. His whole mind seems to have been imbued from an early age with an intense love of nature, and his devotion to it led him to prefer its study to the discipline of the Church, which he had entered. Heer's early reputation was made as an entomologist, and from 1834 forwards he published many works and papers on entomology, chiefly on Swiss insects, and more especially on Coleoptera, most of which treated exhaustively on the vertical distribution of species in the Alps. Possibly he is best known (as an entomologist) in this country by his monographic work on the beetles of Switzerland, which appeared in 1838–41. In this work he did for the Coleoptera of that country what Frey has more recently done for the Lepidoptera, but, of course, lapse of time has rendered Heer's labours out of date as compared with Frey's. This monograph appeared in two forms, but that which is best known was styled "Fauna Coleopterorum Helvetica," and extended to over 600 pages. But his attention was soon attracted, perhaps by some fortunate chance, towards the remains of plants which were being disinterred from the Tertiaries to the north of Lausanne and elsewhere on the Lake of Geneva, and his whole energy became absorbed in unravelling and restoring the vegetation of the past, and continued so until the close of a laborious life. In 1855 appeared the sumptuous "Tertiary Flora of Switzerland," a work which at once placed him in the first rank as a specialist; and being a prolific and imaginative writer, untiring industry, he has since contributed to palæontology a nearly uninterrupted series of works on his favourite subjects, terminating but last year with the sixth volume of the "Flora Fossilis Arctica." Few earnest workers have lived to see their work more highly appreciated, and the gratification he must have felt at the sub-

stantial honours that were showered upon him for many years, from grants of money to honorary distinctions of the highest order, must have gone far to compensate for a malady which had for several years left him bed-ridden. The high reputation he so suddenly acquired, more especially in England, was doubtless mainly due to the friendship of Sir Charles Lyell, who constantly quoted his works, almost to the exclusion of those of other writers on similar subjects. His quickness in seizing the characters of even fragments of fossil leaves, his aptitude in describing them combined with the boldness of his inductions and a certain grace of diction, centred attention on his work, and unconsciously diverted it from his eminent contemporaries, Unger, Goeppert, Saporta, and Ettingshausen. The place he occupied was unique, and his opportunities were proportionally great; his loss will be felt, for it will be difficult to find workers, as competent, either able or willing to dispose with such rapidity of the constantly increasing material brought from distant, and especially Arctic, expeditions

The subject which he had thus made his own is one of exceptional difficulty, both from its wide range and the character of the material to be dealt with, and the path which he trod with such assured steps will be trodden by others with doubt and hesitation. If imitation is the sincerest form of flattery, then was Heer most amply flattered, for nearly all works on the newer fossil floras have been modelled on his bases, and he has become the founder of a school which bids fair to monopolise for some time to come this branch of palæontology. It is no disrespect to the dead to mention the open secret that nearly all English botanists, and very many geologists, have doubted the possibility of determining, except in rare instances, the detached and broken leaves which make up nine-tenths of the Tertiary floras. It is fortunate that Heer's temperament was sanguine, and his belief in his power to interpret the material unlimited, else the marvellous Tertiary floras from the Arctic circle, which so profoundly exercise the imagination, would have remained a sealed book. His powers are the more surprising, as his health does not appear to have permitted much travel, a winter spent on the cultivated side of Madeira seeming to have been his only actual acquaintance with extra-European floras. Much of his work, too, was produced under conditions the reverse of favourable for exact determination and comparison: a friend relates that when calling to convey one of the numerous awards made to him by English scientific bodies, he found the Professor lying down with a small table arranged to cross the bed, upon it being specimens which he named while an assistant made drawings.

Besides the fossil floras and insect faunas of his own country, his works comprise, among many others, descriptions of the Carboniferous, Jurassic, Cretaceous, and Tertiary floras from round the Arctic circle, and from Germany, Austria, Italy, Portugal, and even more distant countries. In 1861 he was invited to England to describe the Tertiary flora from Bovey Tracey, and a work appeared upon it in the *Phil. Trans.* of the following year. It is certainly strange that this and nearly the whole of the fossil floras containing dicotyledons examined by him have been referred to the Miocene age, and certain prevailing types seem to recur in the greater part of them; but it is not our province to discuss the correctness of these views here. He was much in earnest and zealous in the extreme, and the importance and value of his work, including as it does figures and descriptions of species which may almost be numbered by the thousand, is incontestable. So much accomplished, in spite of ill health and probably with less extensive herbaria to consult than are available in this country, commands respect; and however the study of fossil plants may rank in the time to come, Heer's name will for ever be bound up with it as its great pioneer.

THE BACKWARD STATE OF CHEMISTRY IN ENGLAND

IN the address of the President of the British Association last year the report as to progress in one of the principal Sections, that of Chemistry, is certainly a very meagre one. It is indeed confined to a general statement of the value of materials derived from coal and coal-tars, &c., and cannot, strictly speaking, be termed chemical. Again, in the address of the President of the Chemical Section, the existence of such a branch or division of chemistry as that termed "organic," and in which more perhaps has been done during the past twenty years than in mineral chemistry during the century, is completely ignored. And unfortunately the reason does not seem far to seek, for very few of the papers presented to the section had direct connection with the chemistry of carbon.

But it is not only at the British Association meetings that this neglect of organic chemistry occurs, but even at the Chemical Society itself the number of contributions to this section of chemistry is very small. In 1881, out of more than eighty communications to the Society only about thirty are relating to carbon compounds. In 1882 the proportion is somewhat greater—thirty-one out of sixty-five. It would be perhaps very unfair to institute a comparison between our Chemical Society and a much younger one, that of Berlin, as they are somewhat different in constitution: and the feeding ground, if it may be so called, is more extensive in the one case than in the other, but still the disparity in number of papers is scarcely to be accounted for in this way.

Chemistry generally, and especially the so-called organic chemistry, appears to have been very much neglected for some years past in this country.

The cause of this lagging behind, especially in a science of such infinite practical applications as chemistry, by this country is somewhat difficult to understand.

We certainly have not the number of chemical schools in England as in Germany, but making allowance for that and comparing the past decade in the two countries we appear to be grievously behind, both in number of investigations as well as in their theoretical or practical importance.

The cause can scarcely be attributed to any want of energy or appreciation of the value of research on the part of our manufacturers, for they are in many instances obliged to seek assistance out of the country. It seems to some extent rather to be owing to a non-appreciation of the science by the general public, although in its more elementary stages it is more extensively taught than in any other country, and this non-appreciation reacts injuriously on the schools themselves. Although such an intensely practical science and capable of such varied applications, the chemical investigator must pass over many weary stretches of complex and to all appearance purely theoretical and useless work before reaching a brilliant practical result.

Unfortunately, until quite recently but few of our schools were so constituted and equipped that a student might work on anything like equal terms with his fellow in a German school.

Now, however, we have a goodly number of chemical schools rising up, with in many cases professors trained in German laboratories. But in many of these the professor labours under the great disadvantage, not only to himself and the science, but to the students, that he has too much mere routine teaching and too little time for that original work, or research, which acts so powerfully in encouraging and stimulating his students to get more than a mere insight into the working and mechanism of the science, and become investigators themselves.

The cost of working in an English laboratory is somewhat greater than in a German one, but this difference is

no longer great, and does not by any means account for the difference in results between the two countries. The German student as a rule works very much longer, that is, he is a student for a longer time than the English student, who too often commences his study of chemistry not as a mental training but as a means to an end: to become a public analyst or a works analyst, and who desires to learn only as much as is absolutely necessary for some particular line he has chalked out for himself; or, worse still, to "pass" some "examination."

The importance of chemistry, especially that more regular and systematic chemistry of the carbon compounds, as a philosophical training is not yet by any means recognised in this country. And it is to be feared that until this is remedied we shall still remain, in spite of new schools, in a backward position.

According to an authority like Prof. Wundt, even qualitative and quantitative analysis are, as logical methods, superior to mathematical.[1]

There is no reason to suppose that the ordinary English student is inferior to any other, and when this subject is put before him in a proper light, as a mental training of the highest order, and not simply a mechanical sort of process, more cheerful results may be looked for. But the students in our higher schools and universities should not stop at qualitative and quantitative analysis, but if possible do some synthetic work, as by this only is a real grasp of the science to be obtained.

When once we get a substratum of well-trained students, not simply analytical machines, or examination-passers, we shall not have long to wait for results of theoretical and also practical interest.

But our professors must also bestir themselves. In very few institutions in England are more than elementary courses of lectures given, generally the same thing one session after another. The professor should always be practically engaged in research work, so that his students may have a real example to follow. This of course can only take place when the present disproportionate amount of teaching is reduced. Certain it appears that the enthusiasm and rapid advance of the students working in a German university laboratory is in a great measure, probably entirely, due to the example of the professor's working.

[1] Wundt, "Philosophical Studies," vol. i. p. 473. 1883.

THE CHOLERA BACILLUS

THE Report in which Dr. Koch, chief of the German Scientific Expedition, embodies the results hitherto obtained by him and his assistants with regard to the cholera in Egypt, deals in a very guarded manner with the question of the discovery of a definite cholera bacillus. As the result of experiments carried out both on living and dead cholera subjects, it appears that, whereas no distinct organism could be traced in the blood and in the organs which are so frequently the seat of micro-parasites, yet bacteria having distinct characteristics and resembling somewhat in size and form the bacilli found in glanders were discovered in the intestines and their mucous linings; and this under circumstances which seemed to identify them with the disease from which the patients were suffering. Thus, their existence in the intestinal membranes was obvious so soon after death that they could not have been brought about by any *post-mortem* changes; they were present in the case of all patients who were actually suffering from the disease, and in the bodies of all those who had died of it, whereas they were absent in the case of one patient who had had time to recover from cholera but who had died of some secondary complication; and they were not discoverable in the case of patients who, during the cholera epidemic, succumbed to other diseases. And further, the same bacillus had been met with by Dr. Koch, a year previously, in the case of four patients who had died of cholera in India, and portions of whose intestines had been forwarded to him for examination.

From these circumstances Dr. Koch feels justified in provisionally holding the belief that these bacilli are in some way related to cholera, but as yet he is not prepared to say whether they are the cause or the effect of that disease. The number of cases which the Scientific Expedition were able to utilise for the purposes of their inquiry was very limited, and it is also suggested as possible that some of the experiments were vitiated owing to the circumstance that the disease was already subsiding in intensity when the investigations were commenced. Especially does Dr. Koch suggest that this may account for the invariable failure to produce cholera in any of the lower animals into whose bodies the intestinal secretions were inoculated; but as to this it must be remembered that human diseases are rarely communicable to other animals, and that, as regards enteric fever, a disease which etiologically and otherwise has many points of resemblance with cholera, every effort to communicate it to other mammalia has hitherto invariably failed. But the failure of infective power which may very possibly be associated with the declining stage of an epidemic would be very likely to interfere with experiments having for their object the isolation and cultivation of the bacillus, and hence we are glad to learn that Dr. Koch is to continue his investigations in India, where the varying stages of the disease can easily be met with. In the meantime, however, it will be well to remember that Drs. Lewis and Cuningham have, notwithstanding laborious microscopic and other researches in India, hitherto failed to identify any of the organisms they have met with as specifically related to cholera.

One point is set at rest by Dr. Koch's Report, and that relates to the actual nature of the disease which has been epidemic in Egypt. Both pathologically and otherwise he declares it to be identical with Asiatic cholera.

NATIONAL TRAITS IN SCIENCE[1]

THERE are at present three principal currents of scientific work—German, English, and French. The scientific writings of each nationality are characteristic, and, taken as a whole, offer in each case distinctive qualities. German influence is now predominant over the scientific world, as French influence was uppermost during the earlier part of this century; but the sway of Germany over Western thought is far more potent and widespread than was ever that of France. As students once gathered in Paris, so they now flock to Germany; and thence back to their own lands they carry the notions of German science, and labour to extend, imitate, and rival them. Thus German ideas have been spread abroad, and established in foreign countries. This has set a common standard for scientific work, which is accepted in most European countries. German influence is evident by its effects in Switzerland, Russia, Italy, Poland, Belgium, England, and America, and in degrees indicated by the order given: in France, Spain, and Portugal it is hardly noticeable. Holland and the Scandinavian countries have for many years achieved so much and so excellent work that their scientific development may be said to have accompanied rather than to have followed that of Germany.

German science has unquestionably distinctive qualities. Its pursuit is a special and honoured calling, attractive to the highest talent: its productions have the stamp of professional work. The German scientific man is first and principally an investigator: he is obliged to be so, otherwise he loses in the race. He wins his posi-

[1] From *Science* of October 5.

tion in the hierarchy of learning by the original researches he carries out. To succeed under these circumstances, a man must discover something which is a real addition to knowledge; and to do this, he must be thoroughly familiar with all that has been previously accomplished in his field. Moreover, to advance beyond his peers, the investigator must utilise every possible extraneous advantage; more especially must he have a mastery over the methods to be employed, and be familiar with all novelties and refinements therein. It cannot be gainsaid that these requirements are more fully answered in Germany than anywhere else. It is certain that, excepting of course a small minority, German scientific publications always contain something really new, and unknown before: each article is a scientific progress, which, however slight, still brings an actual increment to our store of information. Another result of this professional thoroughness is equally striking and characteristic. Being fully posted as to the status of his department, the German often displays a singularly just and keen appreciation of what problems are for the moment best worth studying, as being open for solution, and leading to something farther, or else filling a gap left. He is thus enabled to render his work efficient. It is sad to think how much scientific work is wasted because the labour is not wisely directed.

In German scientific writings the excellence of the matter usually contrasts vividly with the defective style and presentation. Indeed, the Germans, despite the superiority of their modern literature, are awkward writers, and too often slovenly in literary composition. Conciseness and clearness are good qualities, which may assuredly be attained by the expenditure of thought and pains; but these the German investigator seems unwilling, in many cases, to bestow upon his pen-work, but follows the easier plan of great diffuseness. Besides this, another defect is not uncommon,—the ill-considered arrangement of the matter. This occurs in all degrees, from a well-nigh incredible confusion, to be sometimes found even in elaborate and important essays, to a slightly illogical order. In this regard, a curious and not infrequent variety of this fault deserves mention. According to the headings of the chapters or sections, the division of topics is perfect; but under each head the matters are tumbled together as if a clerk was contented to stuff his papers in anyhow, if only he crammed them into the right pigeon-hole.

Speaking broadly, the German mind lacks conspicuously the habits of clearness and order. There have been celebrated exceptions, but they were individual. The nation regards itself as having a decidedly philosophical bent, meaning a facility at taking broad and profound views of the known. We venture to contradict this opinion, doing it advisedly. Their profundity is mysticism, their breadth vagueness, yet a good philosopher must think clearly. It is a remarkable but little heeded fact, that Germany has not contributed her share to the generalisations of science; she has produced no Linné, Darwin, Lyell, Lavoisier, or Descartes, each of whom bequeathed to posterity a new realm of knowledge, although she has given to the world grand results by the accumulated achievements of her investigators. The German's imperfect sense of humour is another obstacle which besets him on every path. He is cut off from the perception of some absurdity, like that of Kant's neumenon, for instance. One cannot explain this to him: it were easier to explain a shadow to the sun, who always sees the lighted side. To state the whole epigrammatically, German science is the professional investigation of detail, slowly attaining generalisations.

English science is the opposite of this,—amateurish rather than professional. Some might call it insular, yet we should hardly join them in so doing. In fact, the professional investigator has hardly been a recognised character in the English social organisation: until recently he was barely acknowleged, even by the universities, which sought instructors who knew and could teach, who might investigate and discover in a subsidiary, and, as it were, unofficial way. A large number of English scientific men were disconnected from the universities and colleges after their own student years, and were half or wholly amateurs; and their writings show the effects of this separation, not always, to be sure, but in many cases with painful evidence, by a lack of thoroughness, an imperfect acquaintance with other investigations, and a failure to grasp the essential part of the problem: in brief, such writings appear behindhand and superficial. Yet amid these poorer productions are to be found a right goodly number of the best scientific articles we possess in any language. Of late years the proportion of the good has steadily increased, and investigation is now more correctly appreciated than ever before. Indeed there is no more encouraging event in the recent progress of science than the sudden elevation of the standard of original research in England. The English are trained writers: their scientific articles excel the German in literary merit, being seldom slovenly either in arrangement or style, and rarely wearisome from sheer diffuseness. Very noteworthy is the fertility in generalisations of the English: this is with them the outcome of individual endowments, a single master attaining a broad conclusion—a process of individual effort quite unlike the German democratic method of generalising by the accumulations of many. Is it too much to say that the English and Scotch are the Greeks of modern philosophy?

French science is decidedly provincial: it is apart, having only an imperfect, uncertain acquaintance with the great world outside, and its international interests of original research. The French have lagged far behind the great movements of recent years. Consider only how backward they have been in the comprehension and acceptance of the Darwinian theory; and remember, too, that it were wiser to take out the mainspring from a watch than to eliminate evolution from biology. French scientific articles are well written, the matter is admirably classified, it is all very clear. The keen, artistic sense of the nation displays itself here; but it also deludes them into presenting a rounded survey of a greater field than is demanded by the actual discoveries they report. To satisfy this yearning for artistic completeness, elaborate and tedious disquisitions, and hackneyed principles, and facts long known, are interpolated; and even worse may be, when the imagination helps to create the completeness. Most scientific men harbour a little distrust of French work. This sentiment is further fostered by the almost systematic neglect of German research on the part of the French. Such a frank exhibition of rancour makes one suspect the impartiality of the French in science generally: indeed, we believe that science has never been so depressed in France as at present. Italy is above her; but Italy, with all her innate ability, is striving to learn from Germany, and has already risen high, and will rise higher. We trust and believe that the present phase of French science which abounds in inefficient work will soon end, and the people terminate their present voluntary isolation. The French stay at home: they used to travel abroad much. Let us hope that they will soon resume their ancient habit, and, above all, that they will re-establish mental intercourse with foreigners. There are *savants* in France who are esteemed throughout the scientific world: may their number rapidly increase!

America's contributions to pure science are by no means very extensive, or often very important: compared with the great volume of German production, they seem almost insignificant. We have never duly fostered research, for we have bestowed upon it neither the proper esteem nor office. There are, we suppose, at least six thousand

"professors" in the United States: are one hundred and fifty of them active investigators? The time seems remote when every American professor will be expected to be also an investigator; but among us is a little band of men who have before them the model of Germany, and who are working earnestly for the intellectual elevation of their country. Their first object is necessarily to render research more important in public estimation, and so to smooth the way for a corps of professional investigators. Every thoughtful person must wish success to the attempt.

THE GEODETIC CONGRESS

THE most generally interesting part of the proceedings of the Geodetic Conference which has been sitting at Rome during the past week is that connected with the selection of a common first meridian.

The report of the Permanent Committee of the International Geodetic Association recommends to the Conference the general acceptance of the meridian of Greenwich; it was referred to a Special Committee composed of one representative for each of the following—England, the United States, Germany, Italy, France, and Hamburg. The report concludes thus:—

"We terminate our report by proposing to the Assembly the following resolutions:—

"The seventh General Conference of the International Geodetic Association, held at Rome, and in which representatives of Great Britain, together with the directors of the principal astronomical and nautical almanacs, and a delegate from the Coast and Geodetic Survey of the United States have taken part, after having discussed the questions of unification of longitudes by the adoption of an initial meridian, and of the unification of time by the adoption of a universal hour, have come to the following conclusions:—

"Firstly, that the unification of longitudes and of hours is as equally desirable in the interests of science as in those of navigation, commerce, and international communication. The scientific and practical utility of this reform considerably outweighs the sacrifices and the trouble of arrangement to which it will put the minority of civilised nations. It should, therefore, be recommended to the Governments of all the States interested that it may be arranged and confirmed by an International Convention, so that henceforth one and the same system of longitudes may be employed in all the astronomical and nautical almanacs, in all the geodetic and topographical bureaux and institutes, and in all geographical and hydrographical charts.

"Secondly, that the Conference propose to the Governments to choose for the initial meridian that of Greenwich, inasmuch as that meridian fulfils, as a point of departure of longitudes, all the conditions required by science; and that being already actually the most extensively used of all, it presents the greater probability of being generally accepted.

"Thirdly, That the longitudes should be reckoned from the meridian of Greenwich in the sole direction of from east to west, and from zero to 360°, or from zero to twenty-four hours; the meridians on the charts and the longitudes in the registers should be indicated everywhere in hours and minutes of time, with liberty of adding the indication of the corresponding degrees.

"Fourthly, That the Conference recognises for certain scientific needs, and for the service of the great administrations of the means of communication, such as railways, steamship lines, telegraphs, and posts, the utility of adopting a universal hour, side by side with the local or national hours, which will necessarily continue to be employed in civil life.

"Fifthly, That the Conference recommends, as the point of departure of the universal hour and of cosmopolitan dates, the mean noon of Greenwich, which coincides with the instant of midnight or with the beginning of the civil day, situated at the twelfth hour, or at 180°, Greenwich. It follows that the universal time will correspond everywhere with the mean local time, reckoned from midnight, less twelve hours and the longitude of the place, and that the dates change at the antipodes of Greenwich.

"Sixthly, That it is desirable that those States which, in order to adhere to the unification of longitudes and of hours, will have to change their meridians, should adopt the new system of longitudes as quickly as possible in their observatories and official almanacs, in their geodetical, topographical, and hydrographical works, and in their new charts. As a means of transition it would be well that in new editions of old charts, on which it would be difficult to change the squares, the indications according to the new system should at least be inscribed alongside the enumeration of the old meridians.

"Seventhly, That these resolutions should be laid before the Governments and recommended to their friendly consideration with the expression of a hope that an International Convention confirming the unification of longitudes and of hours may be concluded as quickly as possible by a special Conference."

The Report is signed by the president, General Ibanez, and the secretaries, Professors von Oppolzer and Hirsch, the latter being also the reporter.

The paragraph in Dr. Hirsch's report, in which, after considering the question of the choice of an initial meridian, he emphatically conveys the opinion of the Permanent Committee in favour of that of Greenwich, merits quotation:—

"It cannot be doubted that the problem should be solved in favour of the meridian of Greenwich. It is by far the most extensively used, and, from the geographical, nautical, astronomical, and cartographical points of view, best answers the two conditions required. In fact, the immense British Empire, with its 20,000,000 of square kilometres and its 250,000,000 of population, extends over all parts of the world. Its mercantile marine, numbering 40,000 ships, with a total of from 6,000,000 to 9,000,000 of tons, and an equipment of 370,000 men, surpasses in importance the *ensemble* of all other navies. It must also be added that a great many other countries, among which the most important in respect of their mercantile marine are the United States, Germany, Austria, and Italy, equally use the Greenwich meridian in navigation, whence it may be affirmed that 90 per cent. of the navigators throughout long voyages calculate their longitudes by the meridian of Greenwich."

The Report of the Special Committee on the above resolutions was read on the 22nd before the general meeting of the Conference, and accepted, after a very animated debate.

Referring to the resolutions it is only requisite to state briefly that, according to the *Times* report, as sent back to the Conference by the Special Committee, they now stand as follows:—Numbers 1, 2, 4, 6, and 7 were adopted by the Committee without alteration; the other two were modified, or rather abbreviated, and now read thus:—

"Thirdly, that the longitude should be reckoned from the meridian of Greenwich, in the sole direction of from west to east.

"Fifthly, That the Conference recommends, as the point of departure of the universal hour, and of cosmopolitan date, the mean noon of Greenwich, which coincides with the instant of midnight, or with the beginning of the civil day, under the meridian situated at 12 hours, or 180°, from Greenwich; the universal hours to be counted from zero to 24."

To these seven resolutions the Special Committee have

added two others. The first, inserted between numbers one and two of those referred to, reads thus :—

"That, notwithstanding the great advantages which the general introduction of the decimal division of the quadrant for geographic and geodetic co-ordination, and the corresponding expressions for time, is destined to realise, scientifically and practically, reasons eminently sound appear to justify the passing by the consideration thereof in the great measure of unification proposed in the first resolution. Meanwhile, to satisfy at the same time important scientific considerations the Conference recommends on this occasion the extension, in multiplying and perfecting the necessary tables, of the application of the decimal division of the quadrant, at least for the great numerical calculations for which it presents incontestable advantages, even if it be desired to preserve the old sexagesimal division for observations, maps, navigation, &c."

The other, inserted between resolutions six and seven, is as follows :—

"The Conference hopes that, if the whole world is agreed upon the unification of longitudes and hours in accepting the Greenwich meridian as the point of departure, Great Britain will find in this fact an additional motive to take on her side new steps in favour of the unification of weights and measures, by joining the Metrical Convention of May 20, 1875."

The resolution as to the choice of the initial meridian was carried by 22 votes to 4 ; while Mr. Christie, supported by the French delegates, moved the substitution of Greenwich midnight for noon as the point of departure ; this amendment was negatived by 20 votes to 8. Finally, Dr. Hirsch made a motion, unanimously carried, to the effect that the Conference should request the Government of His Majesty the King of Italy to officially communicate the resolutions voted by the assembly to all the Governments, including those not represented at the Conference.

Among other reports read was one by Dr. Hirsch, on the works of precise spirit levelling carried out in different States during the last three years. Col. Perrier, one of the French delegates, recommended that those works should be continued, so as to connect the Atlantic with the Pacific, and to ascertain the difference of level between those two oceans. General Ibanez read a report on tidal studies with the mareograph. An interesting discussion followed as to the best means for obtaining the most exact results, and a proposal made by General Ibanez to exclude observations taken at times when the sea is agitated was accepted.

Col. Ferrero proposed to close the network of triangles around that basin of the Mediterranean of which Italy forms the eastern side, and invited France to connect the Algerian network with the Italian at Tunis as quickly as possible. Col. Perrier replied, giving assurances that France would commence the work next year, and then read his report upon the measure of bases and the instruments employed, which concluded with a request that the Geodetic Association would invite Germany to prevent the destruction of geodetic signals.

A Committee, composed of Col. Clarke on the part of England for Malta, Capt. Kalmar for Austria, Col. Perrier for France, and Capt. Magnaghi, Col. de Stefanis, and Prof. Pergoal for Italy, were charged with the establishing of an accord for the trigonometrical junction of Italy with France, and Austria and Sicily with Malta, and instructed to invite the co-operation of England in communicating differences of longitudes to be determined telegraphically between Malta and Bona, between Malta and Naples, between Naples and Corfu, &c.

The honorary president of the Conference was General Baeyer, and the acting president Col. Ferrero, President of the Italian Geodetic Commission. Mr. Christie, the Astronomer-Royal, and Col. Clarke, R.E., represented England at the Conference. The United States was represented officially by General Cutts of the Coast Survey, though Messrs. Hilgard and Peirce seem also to have been present.

LARGE AND RUDE PALÆOLITHIC IMPLEMENT

IN November, 1881, Miss Eleanor A. Ormerod, F.M.S., of Isleworth, found the remarkable instrument here illustrated, and kindly added it to my collection. It was found in the gravel and brick-earth thrown out of an excavation made for the new Hounslow and London Railway, immediately south of Osterley Park, near Isleworth. The excavation at this spot showed about three feet of brick-earth resting on eight feet of gravel, and at this depth the London clay was reached, a foot or two of which was exposed. The gravel showed horizontal seams

of fine sand, and agreed well with the well-known Thames gravel at Acton and Ealing.

The implement is engraved one-sixth actual size, and a front and side view are shown. It is exactly two feet in length, and weighs thirty-two pounds. It belongs to the gravel and sand, and is Palæolithic, as is proved by the ferruginous stains. Miss Ormerod, who saw that the flint had been trimmed to shape by human hands, took the instrument to be a huge club, the more attenuated end being possibly, she thought, designed for grasping in the hands; she also noticed that the more massive end was battered as if by use as a club. The more pointed end of this tool has been rudely but skilfully trimmed to a wedge-like point, and any one acquainted with flaking can see at a glance by referring to the illustration that the point is artificial. Towards the base at A (seen more distinctly on the right of left figure at same point) the battering is remarkably distinct. I do not think this battering has

arisen from the use of the tool as a club, but rather as an anvil, as pointed out more than once in reference to other stones observed by Mr. F. C. J. Spurrell and myself. Several flakes have been removed from the extreme butt, and a few small inconvenient asperities have been knocked off elsewhere. Greater part of the flint is covered with the original bark, and this bark is brownish ochreous, its colour proving its derivation from the ochreous gravel. The trimmed parts are lustrous, unabraded, and very slightly stained. The tool was no doubt made and used close to where it was found, and probably belongs to a "Palæolithic Floor," of which so many examples are known now that attention has once been drawn to them. The whole condition of the implement exactly agrees with the stone implements from Stoke Newington, Erith, and Northfleet. The tool appears to have been used as an instrument for thrusting, as well as in a horizontal position as an "anvil-stone." It would be idle to mention the possible uses of such a huge tool as this, but every one who has formed ideas of the mode of life of Palæolithic men will readily think of numerous uses to which such an implement could have been put.

In March, 1882, I had an opportunity of hastily walking through the railway cutting, and I not only lighted on several unabraded Palæolithic flakes, but I found a subtriangular somewhat abraded Palæolithic implement in a lump of concreted gravel, which had fallen out of the side of the cutting between Hounslow and Isleworth at six feet from the surface line. This implement, formerly 556 in my series, is now in the collection of Mr. John Evans at Nash Mills. I also found a large butt-end of an implement, broken in Palæolithic times, a little nearer Hanwell, and another implement in the cutting between Hanwell and Ealing.

Near Hanwell in this cutting fresh-water shells were abundant, and I do not think they have been recorded, with implements, before from this position. It is to be hoped the members of the Ealing Natural History Society collected and took note of them.

WORTHINGTON G. SMITH

AGRICULTURE, ITS NEEDS AND OPPORTUNITIES

PROFESSOR W. J. BEAL'S address on this subject, delivered before the American Association for the Advancement of Science, in August, is of interest to Englishmen from more than one aspect. In the first place its perusal gives us the means of knowing what is being done in the United States for the advancement of scientific agriculture. In the next place we are able to judge how far we excel or are excelled by our American relatives in matters connected with agricultural inquiry. Lastly, it is in such addresses that we may expect to find suggestions worthy of attention, and thoughts which in due course will develop into acts. Prof. Beal takes for his text—"Agriculture, its Needs and its Opportunities." So far as its needs go they are manifold, and its opportunities are certainly coextensive with its vast domain.

The first need is a very common one indeed—it is the need of brains. Agriculture needs brains to guide and counsel her. Prof. Beal is evidently a man calculated himself to supply this need so far as one man can so do. He invites the assistance of men of intellect to rescue agriculture, and he laments the fact that within a comparatively recent time but very little of the best thought even of civilised nations has been devoted to subjects intended to advance agriculture. He calls attention to the munificent aids granted by the United States Government for the encouragement of anthropology, astronomy, geological and mineralogical and other surveys, while but a small sum has been appropriated to agriculture. To illustrate the hesitancy of men to bequeath money for the promotion of agriculture he takes the following from an address given by President T. C. Abbot:—

"I met a very pleasant and intelligent gentleman, who, from his large wealth, was about to give some sixty or seventy thousand dollars for the advancement of higher education. He had been for some years, and was still, the president of a State Agricultural Society. He was a farmer. Did he then endow some Chair of Agriculture or Agricultural Chemistry, of Veterinary Science, or of Horticulture? Did he fit out an experiment station to analyse fertilisers, to study the value of cattle foods? None of them. This farmer gave his thousands to endow another workshop of astronomy."

The above sentences are couched in the language of indignation. They illustrate our own experience on this side of the water, for the public ever seem to take more interest in abstract science and fine art than in technical instruction. The interest in agricultural science has been always languid, albeit it has had its stalwart and enthusiastic supporters. But the public have hitherto failed to tangibly grasp the importance of the subject. It is allowed in a sort of languid and perfunctory manner, but without enthusiasm. We have recently passed through a fervid effort towards the attainment of better musical instruction by means of a College of Music. But when are Royal personages going to lead a movement in the direction of securing better instruction in agriculture? And is not agriculture as noble a subject whereon Royalty might bequeath its patronage and lavish its wealth as music?

We find then a certain unaccountable indifference to agricultural science on both sides of the Atlantic, and yet we ought not to forget that, while much more ought to be done, much has been done both in America and Europe.

The field as a field of research has not been so fruitful as at one time it was expected to prove. The old and time-honoured practices of the farmer have too often justified themselves when confronted by scientific objectors. The suggestions of the scientific man have too often been found impracticable and over-expensive by the practical farmer.

It is indeed very difficult to improve upon processes which have stood so many trials. A certain reckless assumption that old practices *must* give way to new has been the ruin of many good men. Agriculture is undoubtedly capable of improvement, but the improvement is generally most evident when established upon the old lines of good practice, and when heroic measures are avoided.

Limited production is the chief difficulty in the way of scientific agriculture. We cannot multiply our production by steam power or chemical fertilisers. We can only add to it, and that rather sparingly. We cannot increase the number of our crops. Harvest only comes once a year. Thus the examples of the printing press and of the loom fail to impress the farmer with what science is to do for *him*. Let it not, however, be thought that there is not scope for the application of science to agriculture. If we cannot multiply we can increase our produce and cheapen processes. The uses of fertilisers; the comparative values of foods; the improvement of instruments; the introduction of steam; the propagation of improved animals; the study of grasses and economic plants in general; the improvement of wool and of cereals; the introduction of new and cheap building materials, &c., are all worthy of attention, and all require the aid of science.

Prof. Beal points out the importance of meteorology to the farmer. He illustrates this by a quotation from Dr. R. C. Kedzie, who wrote in 1882, "If specific warnings had been given our farmers at that time (harvest), most of the wheat might have been safely housed, and the farmers of Michigan saved from a loss of $1,000,000." Another point made by the professor refers to our imperfect knowledge of those epidemics which from time to time visit our own flocks and herds, as well as those of America—

"Among the drawbacks that may be specially named is the ignorance of legislators, of executives, and electors on this subject."

With this English readers will entirely concur. The next subject proposed as a definite object of study is that of economic entomology. Such enemies as the Colorado beetle, the wheat midge, the turnip fly, the wire worm, or the locust are among the most formidable which the farmer has to contend with. The depredations of the wheat midge alone may be appraised at over 3,000,000 bushels annually in Great Britain. Our Royal Agricultural Society has taken up this matter, and Miss Ormerod's book upon injurious insects is full of information and suggestions concerning it. This certainly is a branch of knowledge which requires labourers. From it we are led to reflect upon the important bearing of the study of *entozoa* upon agriculture, and especially upon the pathology of farm stock. The improvement of the American pastures appears to be desirable. According to Prof. Beal, a dozen sorts of grasses probably cover nineteen-twentieths of all the cultivated meadow-land from Maine to Texas. As the grass family is large, containing from 3100 to 4000 or more species, it is naturally thought that a few more might be found suitable for various parts of this immense area.

Not only is much to be done in the introduction of new grasses but in selecting and propagating varieties of the same grass. "Plants of red clover vary amazingly in many respects." "I believe our fields of red clover today contain nearly or quite as great a variety of plants as would a field of Indian corn, if we were to mix in a little seed of all the varieties cultivated in any one State." This is startling, but quite in accord with the wonderful variations observable in plants of Italian and perennial rye grass and other grasses and clovers. Thus Prof. Beal indicates various paths for improvement, and urges the vast importance of agricultural experiment stations, where work bearing upon the various subjects enumerated may be carried on by competent persons. There is nothing very new in all this. The importance of agricultural research has been often declared in our own country from the days of Sir Humphry Davy until now; but too often the voice has been as of one crying in the wilderness. Some good has been done, and we may expect a more rapid development of these ideas ere long. Scientific agriculture was never more popular than at present, and the number of agricultural students attending systematic courses of instruction is greater now than at any previous time. Agriculture has become a recognised subject of the Science and Art Department, and we note a disposition on the part of scientific men to attach greater importance to the study of domesticated animals and cultivated plants than formerly. We owe much of this to Darwin. He, more than any other man, raised the scientific interest of these humbler subjects of zoological and botanical science, which had previously been passed by scientific men with scarce concealed contempt. Anthropology has also done much to throw a halo of interest around those animals and plants which have been associated for long ages with man. Hence there is a greater bond of union between the present generation of scientific men and the agriculturist than existed a generation since, and it is probable that this sympathy will increase and fructify for the benefit of all.

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THE GREAT NEBULA IN ORION

IT is a fortunate circumstance for students of nebular astronomy that within a short time that branch of science has been enriched by a monograph and a photograph, each perfect in its way, of one of the grandest objects in the heavens. The monograph is from the pen of Prof. Holden,[1] whose name is a guarantee of thoroughness; the photograph we owe to Mr. Common, who at one bound has distanced all predecessors, and has shown us that in the future we may hope for permanent records of the nebulæ as perfect as those of the surfaces of the sun and moon produced by Janssen and Rutherfurd.

In the present article we propose to refer to both these productions with a view of showing how terribly physical astronomers are losing time in not throwing all their energies into the production of photographic records whenever possible. In Prof. Holden's paper we are enabled to see how, two hundred years ago, time was lost and false issues raised because the astronomers of those days had never learned to draw; indeed it is terrible to look at the collection of rude, crude, and almost impossible sketches by Huyghens, Mairan, Picard, Long, Le Gentil, and others which he has brought together. Mr. Common on his part has shown us that it is possible to photograph, with about half an hour's exposure, all the details shown in the most careful drawings made by men with artistic training as the result of months—and we may almost say years—of labour; such drawings as we owe to Bond, Herschel, and Lord Rosse. Mr. Common's photograph, it should also be said, includes the whole nebula, while the monograph is confined to the central portions.

The most convincing argument, however, in favour of the more serious employment of photography in our observatories, that we can use is to show the relation of the photograph to the memoir. The latter commences as follows:—"The main object of this memoir is to leave such measures and descriptions of the brightest parts of the nebula of Orion as shall enable another person observing in after years with the same telescope, under like conditions, to say with certainty whether or no changes have occurred in those parts of this nebula."

To carry out this object everything touching the nebula written between 1618, when Cysat of Lucerne discovered it, and Holden's own observations of 1881 has been brought together and coordinated, and the labour and time this has required may be gathered from the fact that the list of the more important papers relating to the nebula consulted in writing the memoir cover four pages quarto and includes about two hundred entries.

Now it is not too much to say that in the case of an astronomer taking up the question a century hence, as Prof. Holden has now done, he would prefer the single photograph taken by Mr. Common in thirty-seven minutes to all the literature so admirably brought together by Prof. Holden; and if the world must in the meantime lose either the memoir and the records of the human effort of $2\frac{1}{2}$ centuries on which it is based, or the photograph, then it is to be hoped that the photograph will be spared. We say this the more readily because we are certain that Prof. Holden will agree with us.

But it is time that we should refer to the memoir separately. It is preceded by Bond's magnificent drawing, 1859-63, and an index chart giving the minute system of nomenclature necessarily adopted to distinguish the various bright masses, dark channels, spirals, &c., of the portion under notice. The nomenclature is that of Sir John Herschel, Lord Rosse, and Liaponoff in the case of the nebula proper, while the stars are laid down from Bond's catalogue. The index map is indeed the only part of the memoir with which we have any fault to find, for it attempts too much, and for that reason will be of restricted use to those whose optical power is not of the greatest.

The drawings and memoirs are considered in chronological order; the woodcuts bring up the drawings to the same scale, or nearly so. We have the woodcut first, and then extracts of the *ipsissima verba* of the observations. Everything touching the central portion is given

[1] "Washington Astronomical and Meteorological Observations," vol. xxv., 1878. Published 1882.

fully, generally in the words of the author, including discussions as well as original observation; so, as the author points out, the admirable *résumés* of Liaponoff, Struve, D'Arrest, and others are available for immediate reference.

Although Cysat of Lucerne discovered the nebula, it was not the first discovered, that in Andromeda having been noticed by Abdul Rahman al Sûfi, A.D. 950, nor were Cysat's observations of much value. The observational work really dates from 1656, when Huyghens independently observed it, and recorded roughly, as we have said, its shape and the included stars. After all we must not be too hard on the early observers, for, according to Arago, Huyghens' telescopes of 2⅓ inches diameter were 12 and 23 feet long, power 48; drawing *at the telescope*, therefore, was almost out of the question. This notable observation did not long escape our own keen-eyed Hooke, who added to the triad of stars in the central part of the nebula two others, and henceforward the little stellar group has been called the "trapezium," and is a test object dear to all amateurs. Hooke's telescope was 36 feet long, aperture 3½ inches. Huyghens, in his later observation (1694), also caught the fourth star.

After this time the nebula and the number of its included stars grew and grew with every increase of optical power.

FIG. 1.—The Great Nebula in Orion (from a photograph by Mr. A. A. Common).

The observations of Mairan (1731), Long (1742), Le Gentil (1758), Messier (1771), and Sir Wm. Hershel (1744-1811) follow next, the latter especially giving considerable attention to the nebula with his gigantic reflectors; and indeed it may be said he was the first to seriously study it, and among the results of his observation was the statement that the nebula had undergone changes during the time he had been studying it. The three points insisted upon by Herschel are carefully discussed by Holden, with the result that he considers none of them to be established.

Interpolated between the observations of Sir Wm. Herschel and his son (1824) are those of Schrœter (1794-98), Lefebvre (1799), Bode (1800), and Flaugergues (1802).

In Sir John Herschel's first memoir the accepted nomenclature is established; this is partly shown in the annexed sketch.

Like his father Sir John Herschel discussed the question of change. "To the reader who has never viewed this object through powerful telescopes, but who is familiar with the various representations which have from time to time been made of it (including my own of 1824), the number and complexity of the various branches and convolutions now first exhibited, and the different aspects under which the portions best known

are now presented, will no doubt tend to convey a strong impression of great and rapid changes undergone by the nebula itself. I am far from participating in any such impression. Comparing only my own drawings made at epochs (1824 and 1837) differing by thirteen years, the disagreements, though confessedly great, are not more than I am disposed to attribute to inexperience in such delineations (which are really difficult) at an early period—to the far greater care, pains, and time bestowed upon the later drawings. . . . Now there is only one particular on which I am at all inclined to insist as evidence of change, viz., in respect of the situation and form of the *nebula oblongata*, which my figure of 1824 represents as a tolerably regular oval. . . . No observer now, I think, looking ever so cursorily at this point of detail, would represent the broken, curved, and unsymmetrical nebula in question . . . as it is represented in the earlier of the two figures."

The enormous body of work done even in Sir J. Herschel's time, chiefly by Lassell, Bond, Liaponoff, Struve, and Lord Rosse, is so fully recorded that it is impossible to do more than refer to it in the space at our disposal, besides which much of it is so recent as to be still in the minds of all interested in such questions. But

FIG 2.—Sketch Map of the Huyghenian Region.—A, the Huyghenia (brightest) region; θ, the four stars of the Trapezium; B, subnebulou region; C, Sinus Gentilii; D, Rostrum; E, Regio Gentiliana; F Regio Messieriana—Brachium Messieri, Proboscis Major; G, Regio Derhemiana; H, Sinus Magnus; I, Schrœter's Bridge.

Mr. Holden, in his work on the nebula, has not contented himself with discussing this work merely. The Washington observations made by himself are given, and cover nearly 100 pages.

We may now deal with the results arrived at. Prof. Holden considers the evidence of change undoubted, but such change depends less upon actual change of form than upon shiftings of the maxima of brightness. The most undoubted changes are in the brightness of the first and second Schrœter's bridge, and in the appearance of the nucleus of the first. The changes in the brightness of some of the masses are established by the Washington observations alone. Among the results of these observations is a new nebulous patch seen from the time of its origin, when it was stellar in appearance, and faint, until now, when it is bright, and of measurable dimensions.

Another matter investigated by Prof. Holden is the question of the connection of the stars with the nebula. On this point spectroscopic work is brought upon the scene. The spectrum of the nebula was first studied by Huggins. The gaseous nature of some of the small stars near the trapezium is, Prof. Holden thinks, indicated by their peculiar behaviour under different magnifying powers; some of them are best seen with low powers.

The question of photography is introduced in an appendix giving the results obtained by the late Dr. Draper just before his lamented death. This photograph was exposed for 137 minutes, and, as may be easily imagined, lacks sharpness, but still, as Prof. Holden puts it, it worthily inaugurated nebular-photography. He also clearly pointed out that, since the eye and the salts of silver do not most strongly respond to light of the same wave-lengths, the intensities of drawings and photographs must vary, and he shows that they do vary. On the other hand, it is shown that the untouched photograph defends the best drawings against the charge of depending too much upon the personal equation of the observer, as over large regions the best drawings are justified by the photograph.

Mr. Common's photograph, a reproduction of which we give, is far finer than Dr. Draper's, among other reasons because it was exposed for about one-third the time. It is to be hoped that we shall have photographs as good as this taken for the future every year, not only on plates responding to blue light, but on plates responding to green and red. In this way most precious records will be secured for future discussion.

On this question we may make the following quotation from Mr. Common's communication to the Royal Astronomical Society:—

"To find if there is any change of form or relative brightness observable in a nebula with any degree of certainty, it will be necessary to compare photographs taken at some undetermined interval of time; and the best thing to do now seems to me to be to get as many photographs as possible to form the basis of comparison with those taken at some future time; and this I am now doing. . . .

"The light of this nebula is so different in intensity that for a proper exposure of the outer portions the central part is much over-exposed; it is therefore necessary to take photographs with different exposures. Thus an exposure of from one to three minutes gives the brighter portions of the central parts in such a way that they can be easily compared and their order noted; longer exposures giving portions less bright in a similar way, till, with a maximum exposure, the very faintest portions can be compared and noted in order. The stars in the nebula can be treated in the same way, the same photographs being available."

We have before referred to the fact that many of the stars in and about the nebula are variable, particularly the faint ones. Mr. Common has found that one of the brighter stars is remarkably variable, though what its period is he has not yet determined.

As the time of exposure can be easily extended to hours, it will evidently in the not distant future be easy to get stars invisible to the eye in the same telescope used for photography.

NOTES

AMONG the gold medals awarded by the juries of the International Fisheries Exhibition are the following in the department of Natural History:—INVERTEBRATA.—United Kingdom: James Cook, the Duke of Edinburgh, Prof. McIntosh, Thos. Bolton. Italy: Dr. Dohrn (2). Netherlands: Netherlands Zoological Society. United States: United States Fish Commission. New South Wales: Sydney Museum. Sweden: F. B. Wittrock, Baron Nordenskjöld, W. Lilljeborg. Russia: Dr. Oscar Grimm. FISH, &c.—United States: United States National Museum, United States Fish Commission, Prof. Alexr. Agassiz, Prof. G. Brown-Goode, Prof. David S. Jordan. New South Wales: Australian Museum, Sydney (2); New South Wales Commission, Hon. W. Mackay, E. P. Ramsay. Sweden: the Royal Swedish Academy, Dr. Oscar Dickson. Canada: Canadian Government. China: Government of.

Norway: the Government Geographical Survey. Tasmania: Commissioners of Fisheries. United Kingdom: Dr. F. Day (2), the Princess Louise, Rd. Collett, Dr. Francis Day, Mrs. Bleeker, Arthur Grevenstuk. Sweden: Prof. Baron A. E. Nordenskjöld, Prof. F. A. Smith, W. von Wright. Denmark: H. V. Feilder. Norway: Prof. G. O. Sars, Prof. H. Mohn. MAMMALS, BIRDS, &c.—India: Bombay Museum. Canada: Canadian Government. United States: United States Fish Commission. Sweden: Oscar Dickson, Stockholm Museum, Baron Nordenskjöld.

BEN NEVIS OBSERVATORY was duly "inaugurated" on Wednesday last week amidst a snowstorm. The ceremony passed off successfully, Mr. Murray formally accepting from Mrs. Cameron Campbell, the proprietress of the ground on which the Observatory is built and over which the road is made, the key of the buildings, which all present seemed to think well adapted for their purpose.

THE Home Secretary has acquainted the Meteorological Society that Her Majesty has been graciously pleased to grant it permission to adopt the prefix "Royal." The Society accordingly becomes the "Royal Meteorological Society." To all workers in meteorology, whether Fellows of that society or not, this public recognition of the importance of the science cannot but be most gratifying.

PROF. EDWARD HULL, F.R.S., Dr. E. Hull, and Mr. H. Hart have left London for Suez, under the auspices of the Palestine Exploration Society, to explore the valley of the Jordan. At Suez we understand that the party will have the advantage of the experience of a member of the firm of Messrs. Cook, under whose guidance the expeditionary party are then to proceed. During Prof. Hull's absence from Ireland the lectures on geology in the Royal College of Science, Dublin, will be delivered by Mr. A. B. Wynne, late deputy superintendent of the Geological Survey of India.

IT is suggested that the memorial to the late Prof. Jevons might take the form of a studentship, of the annual value of not less than 100*l.*, the holder of which shall devote himself to economic or statistical research; and that to commemorate the connection of Jevons with Liverpool, in which he was born, and with Manchester and London, in which so many of the best years of his life were spent, the election to the studentship be vested in representatives of University College, London, Owens College, Manchester, and University College, Liverpool, to be appointed for the purpose. Among the members of the committee formed for the purpose of forwarding the proposed memorial are—the Duke of Devonshire, the Duke of Westminster, the Marquis of Hartington, M.P., the Earl of Derby, the Earl of Kimberley, the Bishop of Manchester, Lord Windsor, Mr. Chamberlain, M.P., Sir R. Cross, M.P., Sir Charles Dilke, M.P., Mr. Childers, M.P., Mr. Fawcett, M.P., Mr. Mundella, M.P., Sir J. Lubbock, M.P., Sir T. Brassey, M.P., Mr. J. Cross, M.P., Mr. L. Courtney, M.P., Mr. Robert Giffen, Prof. T. E. Thorpe, Prof. Caird, Prof. J. S. Nicholson, Mr. W. Knight, Prof. Marshall, Principal Edwards, Principal Peterson, Rev. R. Harley, Prof. W. Dallinger, Prof. Adamson, Prof. Roscoe, Prof. Balfour Stewart, Prof. W. Wallace, Prof. G. H. Darwin, and Prof. G. Carey Foster. Subscriptions may be paid to the credit of the treasurer of the Jevons Memorial Fund, with the Manchester and Salford Bank (Limited), and will be received, among others, by Messrs. Williams, Deacon, and Co., London.

AN improvement so useful and suggestive as to deserve notice in these columns, has been recently applied by the proprietors of "Bradshaw's Railway Guide" to the map of Great Britain which accompanies that indispensable manual. It consists in ruling meridian lines at every $1\frac{1}{4}°$ of longitude, or every 5m. of time from Greenwich, so as to show at a glance, sufficiently nearly for practical purposes, the difference between the local time at every town in the United Kingdom, and Greenwich or railway time. The difference, it is true, is small enough to be neglected is the eastern counties; but is considerable enough to require to be remembered in the western half of these islands. A traveller leaving Falmouth by night train with an appointment in London for 10 a.m. the next morning, may be much inconvenienced if he forgets that 9h. 40m. by his watch is 10 a.m. for his purpose. We have only to suppose our traveller to be going from New York to Chicago or from Paris to Vienna, to see the great convenience of this unobtrusive addition to railway maps. But there is another advantage which will be realised whenever these time meridians replace meridians of longitude on school maps, as they are bound to do by degrees. It is that they tend to give clear ideas of longitude, of the earth's diurnal revolution, of time itself. Meridians, as such, are mere coordinates of position, and have no necessary connection with time, and the ideas of many even educated people are extremely hazy on their mutual relations. Messrs. Blacklock of Manchester probably make no pretension to be educational reformers, but in taking the initiative in this improvement, they are in fact, thanks to the great circulation of "Bradshaw," helping to prepare the public mind for the adoption of a universal first meridian, and giving great assistance to the schoolmaster.

THE red spot on Jupiter continues to be well visible. Mr. W. F. Denning writes that on the mornings of October 16 and 18 he observed the spot with a 10-inch reflector, power about 212, and found it a tolerably easy object, though it is very much fainter than the belts. At times the shape of the spot could be distinctly made out notwithstanding the constant vibration of the telescope by the high wind prevailing. Mr. Denning adds that with favourable atmospheric conditions this marking ought to be an easy object for telescopes above 6 inches aperture.

THE only changes proposed to be made in the constitution of the Council of the Mathematical Society for the ensuing session are the substitution of Messrs. W. D. Niven, F.R.S., and J. Hammond, M.A., in the place of Mr. C. W. Merrifield, F.R.S. (who, we regret to say, is obliged to resign on account of ill health), and Dr. J. Hopkinson, F.R.S.

SCIENCE CLASSES have been established in Warwick during the past week. A largely attended public meeting was held on the 16th, under the presidency of the mayor, when an address on the value of science teaching was delivered by the Rev. W. Tuckwell. Fifty names were announced for an immediate chemistry class; and it was proposed to form general classes in geology or botany, with a special working-men's class in practical geometry and elementary mechanics.

MR. J. G. BAKER of the Kew Herbarium, the president of the Yorkshire Naturalists' Union, who has already written floras of North Yorkshire and of Northumberland and Durham, is intending to print this winter a flora of the English Lake District, on which he has been long engaged, and will be glad to receive any contributions towards it.

FROM *Science* we learn that a number of gentlemen met in the library of the American Museum of Natural History in New York City, on September 26 to 28, and founded the American Ornithologists' Union. The membership consists of active, foreign, corresponding, and associate members. The active membership is limited to fifty residents of the United States and Canada; the foreign, to twenty-five non-residents of the United States and Canada; the corresponding, to one hundred residents of any country; the associate being composed of any number of residents of the United States and Canada. The officers of the

Union for the current year are: Mr. J. A. Allen, president; Dr. Elliott Coues and Mr. Robert Ridgway, vice-presidents; Dr. C. Hart Merriam, secretary and treasurer; Messrs. S. F. Baird, George N. Lawrence, H. W. Henshaw, and Montagu Chamberlain, councillors—these nine officers constituting the Council of the Union. The work of the Union for the present year was laid out by the formation of committees on the subjects of classification and nomenclature, of the distribution and migration of birds, of avian anatomy, of oology, and on the question of the eligibility or ineligibility of the European sparrow in America. The first-named committee, besides revising the current lists of North American birds, is expected to consider the subject of zoological nomenclature at large, and its labours may result in the formation of a code of nomenclature applicable to other departments of zoology, as well as to ornithology. It consists of Messrs. Ridgway, Allen, Brewster, Henshaw, and Coues.

THE following telegram from Lieut. Ray, commanding the Point Barrow observing party, appears in the American papers:—"San Francisco, October 7, 1883.—I report my safe arrival here to-day with party. Also brought down Lieut. Schwatka and party from St. Michaels. All work accomplished as ordered by chief signal-officer. Pendulum observation not made. *Leo* reached Ooglaamie August 22; was driven away by ice the same night; returned on the 24th; again driven away and damaged on the 25th; returned on the 27th, when party and stores were embarked; sailed on the 29th, vessel leaking badly; put into Unalaska, where she was beached and repaired."

THE *Dijmphna*, which we announced last week had arrived at Vardö, got free of the ice the day after the members of the Dutch meteorological expedition departed, but having the misfortune of breaking the blades of her propeller she became unmanageable, and was again frozen in for about six weeks. At last, on September 13, the vessel again became free, when Hovgaard succeeded in a week, by sailing and towing, in reaching the Kara Straits, after having passed through ice for some 120 miles. In the latter locality he weathered a terrific storm, and it was first on September 21, in 71° 17′ lat. and 55° 52′ long., that the vessel got quite clear of the ice, viz. half way between the Kara Straits and Vardö, where she arrived after a sixteen days' stormy journey. After repairing here the *Dijmphna* will proceed to Copenhagen. Lieut. Hovgaard and Dr. Holm have made some valuable scientific discoveries and collections during their wintering in the Kara Sea, of which locality a map has also been made.

THE new island which the Dutch Meteorological Expedition discovered near Waigatz Island, is situated in 70° 25′ 28″ lat., and has been named Buys-Ballot Island, after the eminent Dutch meteorologist of that name.

THE expedition despatched by Mr. Sibiriakoff, the Russian merchant, under Mr. R. J. Runeberg, in order to explore the River Angara, between Irkutsk and Yeniseisk, a distance of 1700 versts, has recently returned to St. Petersburg. For several years little ships, chiefly loaded with tea, have sailed down the river, and last year even a small steamer passed down, but shallow rapids have hitherto prevented vessels proceeding up the river. Mr. Runeberg reports that the latter can easily be removed, and a regular trade on the River Angara may therefore soon be looked forward to.

THE native town of M. Pasteur has done honour to the eminent biologist by placing a plate on the house where he was born, commemorating the fact.

VERY few lake-dwellings have hitherto been found in England, and therefore the recent discovery of what seem well-preserved relics of such structures at Ulrome, Holderness, Yorkshire, is of unusual interest. An article in the *Standard* of October 20 describes what has already been done to bring the remains to light, and ascribes the structures and their contents to the early part of the Neolithic age and downwards to the Bronze age.

MR. LEONARD COURTNEY, M.P., speaking at the distribution of the Science and Art prizes at Penzance last week, dwelt strongly on the benefits likely to accrue to the nation from the general study of science. He hoped the study of science would become such that even statesmen might feel the folly of endeavouring to work against the laws of nature.

THE general monthly meetings of the members of the Royal Institution of Great Britain will be resumed on November 5, at 5 p.m., for the election and nomination of members and the election of a manager in the room of the late Mr. William Spottiswoode, P.R.S.

MR. CLEMENT L. WRAGGE, late of Ben Nevis Observatory, sailed on the 18th inst. for Adelaide on the s.s. *Maranoa*. He takes a large equipment of meteorological, surveying, and astronomical instruments, including a fine equatorial telescope. The voyage will be made a scientific one in every possible way, and important results may be expected.

WE understand that the Committee appointed by the Lord Lieutenant of Ireland to inquire into the administrative arrangements of the Board of Intermediate Education will meet in Dublin this week. The Committee consists of Sir R. Kane, Col. Donnelly, R.E., and Mr. R. W. A. Holmes.

SHOCKS of earthquake were felt at a quarter to one o'clock on Friday night at Huelva, Cadiz, Medina Sidonia, Jerez, and other districts of Andalusia along the ocean coast. The movement was from east to west. Telegrams from Huelva and Cadiz say a rumbling noise accompanied the earthquake, which is said to have lasted three seconds in some places, and five seconds in others. In Cadiz the shock is described as strong enough to move doors and bells. The weather, which was unusually wet with cold winds early in October, had lately become very mild and warmer in the south of Spain. Several slight shocks of earthquake were felt on Saturday morning in Lisbon. A shock of earthquake, lasting three seconds, was felt at Tangier at half-past one on Saturday morning. Two shocks of earthquake were felt on Monday morning at Belluno, at the foot of the Dolomite Mountains. The first was at 3.35, the second at 4.15 a.m. Much alarm was caused, but no damage was done. It will be remembered that Belluno suffered great destruction from the earthquake of June 29, 1873. A slight shock of earthquake was felt at Malta at two o'clock the same morning. A slight shock, attended by an undulatory motion, was also felt at Trieste at half-past three the same morning. A despatch received by the Admiralty, dated Tchesme, October 21, states that slight shocks of earthquake continue, but do not cause more damage. Eleven towns and villages have been damaged or destroyed. Lyddia, Eritra, Reisderch are in total ruins Tchesme, Latyalka much damaged. About 90 people were killed, 200 wounded, and 3600 houses destroyed.

MR. H. CECIL of Bregner, Bournemouth, writes under date October 22:—"I perceived here on the morning of the 10th inst., just before the light was sufficient to show the hands of a watch, two distinct tremors of earthquake. A whatnot by my bedside trembled throughout, and a watch on its stand vibrated with a strong and regular pulsation. Nothing was passing at the time, and a heavy steam-roller has passed one morning since without affecting the whatnot or the watch."

A TELEGRAM from Calcutta of date October 22 states that Mr. Graham and his Swiss guides returned to Darjeeling on the previous evening. He pronounces the ascent of Kinchinjunga

rom the south impracticable; but he has succeeded in ascending another mountain 24,000 feet high.

THE *Sanitary Engineer*, which has for some time been pubished in New York, is now to be published simultaneously in England and America.

DR. KING's annual report on the Government Cinchona Plantations in Bengal for the year 1882-83, which is dated May 11 last, is a review of the work in the plantations down to March 31. The planting operations of the year show a grand total of cinchona trees on the Government estate at the last-mentioned date of 4,711,168 of all sorts; this is a decrease, we are told, of about 50,000 on the returns of the previous year, the falling-off being due to the uprootal of 20,000 hybrids, and 43,697 *Calisayas*, which were shown on analysis to have bark of rather poor quality. Dr. King says: "The removal of these inferior trees is in conformity with the policy which has been followed for some years of raising the standard of the produce of these estates by cultivating only the finest kinds of quinine yielders. In conformity with the same policy 160,085 red bark trees, which had to be uprooted in the ordinary rotation followed on the plantation, were replaced, not by red barks, but by yellow barks and hybrids. Ground was, towards the end of the year, broken at Runjung, in the new cinchona reserve across the Tiesta. A European assistant has been located there, and preliminary measures have been taken for planting out there, during the year now entered upon, a number of the best kinds of *Ledgeriana* and hybrid cinchonas." Regarding the crop of bark harvested during the year, Dr. King says it was the largest ever obtained from the plantations, and amounted to 396,980 lbs. of dry bark, 38,880 lbs. of which were collected on the young plantation at Sittong, and the remainder on the old plantation. The total crop was divided as follows: 372,610 lbs. of *Succirubra*, 22,120 lbs. of *Calisaya* and *Ledgeriana*, and 2250 lbs. of hybrid bark. The bulk of the crop was made over to the factory for conversion into cinchona febrifuge, 41,800 lbs. being sent home by order of the Secretary of State to be converted, it is understood, into various forms of cinchona febrifuge in this country for trial by the medical department. It seems that the plants yielding Carthagena bark have not thriven, only three plants being alive at the end of the year, and this notwithstanding every care that could possibly be given to them. The quinologist's report for the same period as the preceding is appended to it, and it shows that the net result of the manufacture of febrifuge for the year was 10,363¼ lbs. of ordinary and 300 lbs. of crystalline febrifuge, the cost price of which was lower than in any previous year. It appears that the year's working resulted in a profit of Rs. 66,284.9.5, which, it is stated, is equal to a dividend of 6½ per cent. on the capital, and may be considered satisfactory. On this point Dr. King says: "The quantity of febrifuge supplied to Government departments during the year was 4180½ lbs., and the cost was Rs. 68,988.8, an equal quantity of quinine at Rs. 96 per lb. would have cost Rs. 4,01,328. The saving to the State effected by substituting febrifuge of Government manufacture for English-made quinine was therefore Rs. 3,32,340."

MR. CHARLES F. PARKER, the curator in charge of the Academy of Natural Sciences of Philadelphia, died September 7, after an illness of several months. Mr. Parker had paid special attention to the botany of New Jersey, and, both in the completeness of his herbarium and the accuracy of his knowledge of it, he had few, if any, equals.

MR. F. E. SAWYER sends us reports of two papers in which he gives the results of his investigations on the folk-lore and superstitions of Sussex. There is also a paper by him in Part vii. of the *Folk-lore Journal* on St. Swithin and the rain water. The same number contains part 6 of Mr. Sibree's valuable collections on "The Oratory, Songs, Legends, and Folk-tales of the Malagasy."

AT the Upsala University a young lady, only seventeen years of age, has just taken the first degree of examination.

THE additions to the Zoological Society's Gardens during the past week include two Bonnet Monkeys (*Macacus sinicus* ♂ ♀) from India, presented by Mr. John Verinder; a Macaque Monkey (*Macacus cynomolgus*) from India, presented by Mr. W. H. B. Morris; a Bonnet Monkey (*Macacus sinicus*) from India, presented by Miss Stokes; a Geoffroy's Cat (*Felis geoffroii*), a Chilian Sea Eagle (*Geranoaëtus melanoleucus*) from Uruguay, presented by Mr. Charles S. Barnes; a Crested Porcupine (*Hystrix cristata*) from Africa, presented by the Earl de Grey; a Purple Gallinule (*Porphyrio cæruleus*), European, presented by Mr. Robert Dowling; a Golden-headed Conure (*Conurus auricapillus*) from South America, presented by Mrs. Robins; a Smooth Snake (*Coronella lævis*), British, presented by Mr. W. H. Pain; an Æsculapian Snake (*Coluber æsculapii*), European, two Redshanks (*Totanus calidris*), British, a Yellow Baboon (*Cynocephalus babouin*), a Gambian Pouched Rat (*Cricetomys gambianus*), a Slaty Egret (*Ardea gularis*) from West Africa, a Little Egret (*Ardea garzetta*), European, a Very Black Lemur (*Lemur nigerrimus* ♂) from Madagascar, purchased; a Brown Bear (*Ursus arctos*), North European, a Puma (*Felis concolor*) from America, a Patas Monkey (*Cercopithecus patas*) from West Africa, two Black-footed Penguins (*Spheniscus demersus*) from South Africa, a Cocteau's Skink (*Macrocincus cocteauii*) from the Cape Verde Islands, deposited.

OUR ASTRONOMICAL COLUMN

PONS' COMET.—Several observers have drawn attention to a remarkable fluctuation in the brightness of this comet in September. M. Bigourdan of the Paris Observatory says that on the 5th of that month it appeared as a faint nebulosity about equal in brightness to a star of the twelfth magnitude, and nearly round. On the 9th, with a power of 500, there was a small nucleus, ill defined but sufficiently distinct from the surrounding nebulosity; the comet's light had increased since the 5th. Moonlight and clouds interfered with observation till the 23rd, when the brightness was much increased, and in a small telescope was equal to that of a star of the eighth magnitude. On the following night, in a fine sky, the comet's aspect was still the same, and its diameter was nearly 2'. On the 27th a considerable change had taken place; the nebulosity was much fainter, and the nucleus distinct from it was from 10–11m. After that date the nucleus further diminished, and on October 6 was only of 12m., though the comet as a whole was more easily seen than at the beginning of September. Thus on September 24 the comet was of 8m., while its brightness, calculated from that of September 5, would have assigned it only 11–12m. It therefore had, as M. Bigourdan remarks, for some time a brightness thirty to forty times that given by theory, which, he says, it is difficult to reconcile with the opinion that comets have not a light of their own.

Herr Rumker, observing at Hamburg, noticed similar variation. On September 23 he had seen the comet as "ein sehr helles Object mit einer glanzenden Verdichtung." On September 27 and following nights, "glich der Comet einem sehr blassen, unregelmässigen, ziemlich grossen Nebel mit einem Kleinen kaum sichtbaren condensations-centrum." The contrast, he says, was so striking that on September 27 he at first doubted if he had the comet in the field.

Baron von Engelhardt found the comet fainter on October 1 than on September 28, but on the latter night it was much better seen with a 5-inch comet-seeker than with a power of 140 on the equatoreal.

THE GREAT COMET OF 1882.—The weather during the last moonless period appears to have been very unfavourable, at least in this country, and there was no opportunity for satisfactory examination of the position of the great comet of 1882, on the chance of glimpsing it with our larger instruments a the earth somewhat overtook it on its course. The theoretical in-

tensity of light during the next period of absence of moonlight is slightly less, but we continue an ephemeris from the elliptic elements calculated by M. Fabritius:—

At Greenwich Midnight

	R.A. h. m. s.	N.P.D. ° ′	Log. distance from Earth.	Sun.
Oct. 27	7 31 37	103 58·6	0·7637	0·7767
28	– 31 22	104 3·9		
29	– 31 6	— 9·1	0·7630	0·7782
30	– 30 50	— 14·3		
31	– 30 32	— 19·5	0·7624	0·7796
Nov. 1	– 30 14	— 24·6		
2	– 29 55	— 29·7	0·7618	0·7810
3	– 29 36	— 34·7		
4	– 29 16	— 39·7	0·7612	0·7824
5	– 28 55	— 44·6		
6	– 28 34	— 49·5	0·7606	0·7838
7	– 28 12	— 54·3		
8	7 27 48	104 59·0	0·7600	0·7851

THE VARIABLE STAR U CEPHEI.—A minimum of this variable was observed by Mr. Knott at Cuckfield, on the evening of October 20. The time was 8h. 34m. G.M.T., and the star's magnitude was 9·2. The minimum fell an hour later than Schmidt's elements (*A. N.* 2382) would predict. The divergence of Mr. Knott's observations has increased to that amount from nine minutes in 1881; at the same time he doubts if a slight increase of the adopted period would of itself completely satisfy the observations, and perhaps the period may be subject to variation.

Reckoning from the minimum on October 20, and using Schmidt's mean period, the next few minima will fall thus:—

October 30, 7h. 52m. G.M.T. ... November 9, 7h. 11m.
November 4, 7h. 31m. G.M.T. ... November 14, 6h. 50m.

PHYSICAL NOTES

AT the British Association meeting a paper by Prof. J. A. Ewing was read, on the magnetic susceptibility and retentiveness of iron and steel. This paper was a preliminary notice of some results of an extended investigation which the author had been conducting for three years in Japan. Experiments with annealed rods and rings of soft iron wire showed that that material possesses the property of retentiveness in a very high degree. As much as 90 and even 93 per cent. of the induced magnetism survived the removal of the magnetising force. The extraordinary spectacle was presented of pieces of soft iron entirely free from magnetic influence nevertheless holding an amount of magnetism (per unit of volume) greatly exceeding what is ever held by permanent magnets of the best tempered steel. The magnetic character of the iron in this condition was, however, highly unstable. The application of a reverse magnetising force quickly caused demagnetisation, and the slightest mechanical disturbance had a similar effect. Gentle tapping removed the residual magnetism completely. Variations of temperature reduced it greatly, and so did any application of stress. On the other hand, the magnetism disappeared only very slowly, if at all, with the mere lapse of time. The residual magnetism in hardened iron and steel was much less than in soft annealed iron. The maximum ratio of intensity of magnetism to magnetising force during the magnetisation of soft iron was generally 200 or 300, and could be raised to the enormous figure of 1590 by tapping the iron while the magnetising force was being gradually applied. A number of absolute measurements were made of the energy expended in carrying iron and steel through cyclic changes of magnetisation; and the effects of stress on magnetic susceptibility and on existing magnetism were examined at great length. The whole subject was much complicated by the presence of the action which, in previous papers, the writer had named *Hysteresis*, the study of which, in reference both to magnetism and to thermoelectric quality, had formed a large part of his work.

M. P. TIHON has lately shown at the Industrial Science Society of Lyons a new semi-incandescent lamp, giving the brilliancy of an arc light. This is attained by having two carbon rods, slightly inclined to one another, brought down on to a small prism of chalk, and separated from one another by a small rod of the same material. The current passes through the chalk rod making it incandescent. By this means the light is rendered steadier than an arc light, and it is said to have the same brilliancy.

MR. FRANK GERALDY has published some interesting statistics comparing the cost of the electric light with gas, both as to its actual cost and its cost per candle power:—

Installation.	Electric light system.	No. of lamps.	Candle power of lamps.	Motor.	Total cost per hour gas.	Total cost per hour electric light.	Cost per candle power gas.	Cost per candle power electric light.
Salle de Télégraphistes at Brussels (Nord) ...	Jaspar	3	235	Gas	1·86	3·82	0·0265	0·0054
Halle aux marchandises, Lyons Station (Paris) ...	Lontin	18	64	Steam	6·825	6·625	0·0273	0·0054
Spinnery at Riverside (United States)	Brush	71	75	,,	36·80	11·68	0·0353	0·0022
Ducommun Establishment at Mulhouse	Serrin	4	110	,,	—	6·64	0·044	0·015
Passage in the Friedrich-strasse (Berlin)	Siemens	10	50	Gas	9·14	6·45	0·040	0·013
Thames Embankment... ...	Jablochkoff	20	28	Steam	—	0·487	0·0150	0·018
Spinnery of E. Manchon (Rouen)	Sautter-Lemonnier	6	150	,,	9·550	7·387	0·0597	0·0082

This is only an extract from a longer list, but conclusively shows that in large instalments electric lighting is cheaper than gas on the total cost; whilst considered per candle power it is far away cheaper. An exception to the rule seems to occur in the first on the list; this is due to the smallness of the installation. In the case of the Thames Embankment the light is reduced by the use of ground glass globes. If we bear in mind the fact that the economy consists in having large installations, we shall be brought face to face with the fact that whereas gas is now made in as large quantities as is practicable, electricity has still to be brought to that state of economy. Thus we may still expect a greater economical advantage than is shown by the above figures.

M. J. JAMIN has a paper in the *Journal de Physique* on the "Critical Point of Liquefiable Gases," in which he discusses a new theory. He says: "I believe that gases are liquefiable at all temperatures when the pressure is sufficient." Describing Cagniard-Latour's experiment, he says: "According to known laws, the quantity of vapour above the liquid increases very rapidly, its density increasing at the same rate as its weight without known limit. Again, the remaining portion of the liquid expands at an increasing rate until it passes that of the gas (Thilorier); it is clear then, by the effect of these inverse variations, that at last a limiting temperature must be reached when the liquid and the vapour must have the same weight for the same volume. At this point they are inseparable; the vapour

does not rise nor the liquid fall, and the surface of the liquid disappears." Thus the critical point is the temperature when a liquid and its saturated vapour have the same density. From the experiments of Cagniard-Latour he deduces that at the critical point a liquid has no latent heat, and in summing up he says: "At the critical point there is no difference between a liquid and its vapour, neither in tension, nor density, nor thermal constitution, nor appearance, nor any property by which they can be distinguished."

PROF. H. S. CARHART, of Evanston, Ill., has made some researches on the effect on the magnetic field of the rotation of a pierced iron disk in front of the poles of a magnet. The result of this research is that he has found that an iron screen with a hole in it held in front of the pole of a magnet acts magnetically, as a screen with a hole in it held near a light acts optically, the shape of the hole being clearly defined; thus showing the difference of intensity of the field when the iron is there and when it is removed. He has made use of this property by placing a small coil capable of inductive action opposite each pole of a horseshoe magnet; in between the coils and the magnet he rotated a disk of iron with two concentric circles of holes a quarter of an inch in diameter, which came exactly opposite the two poles of the magnet. The inner circle contained thirty-two holes, and the outer contained sixty-four. The two induction bobbins he had connected up with a telephone. When the disk was rotated, he distinctly heard two musical notes produced which were an octave apart. The name given to this instrument is the magnetophone.

MR. E. VAN DER VEN has been making some researches on the use of phosphor bronze and silicon bronze wires for lines, the practical results of which are: that their resistances compared with copper of the same diameter are, phosphor bronze, 30 per cent.; silicon bronze, 70 per cent.; steel being 10·5 per cent. The stretch that can be given from pole to pole is, for steel, 2 mm. diameter, 130 feet; phosphor bronze or silicon bronze, 1 mm. diameter, 106 and 91 feet respectively. Another great point is that a bronze wire, on account of its elasticity, would coil up before it had fallen far if broken, thus preventing accidents from broken wires.

THE GREAT TIDAL WAVE

WE have received several communications on extraordinary tidal phenomena, which seem to be connected with the great earthquake disturbance in Java in August last. We bring these communications together here:—

I have received an interesting letter from Major A. W. Baird, R.E., who directs the Tidal Department of the Survey of India, on the results of the earthquake in Java. The extract from the letter, which I append, speaks for itself. It may be worth mentioning, however, that Negapatam in the Carnatic and Port Blair in the Andaman Islands are nearly in the same latitude and on opposite sides of the Bay of Bengal. It is to be hoped that Major Baird will communicate the results of his investigation to some scientific society in this country.

Trin. Coll., Camb., October 18 G. H. DARWIN

INDIA

Extract from a Letter from A. W. Baird to G. H. Darwin, dated Poona, September 27, 1883

"The wave caused by the volcanic eruption at Java is distinctly traceable on all the tidal diagrams hitherto received, and I am informed of great tidal disturbance at Aden on August 27; but the daily reports are always meagre in information. Kurrachee and Bombay also show the disturbance, and as far as I have examined the wave reached half way up to Calcutta on the Hooghly.

"Negapatam was most disturbed, and at Port Blair there was very great disturbance. I have reports from Port Blair of tremendous noises as if a ship was firing guns as signals of distress, and they sent out a steamer to look about. Similar reports come from the Nicobars, and I see by the papers the noises were heard at Tavoy and Mergui in Burmah.

"I am collating the information and getting up diagrams showing the tide curves of August 27 and 28, all in Port Blair time, and all on the scale $\frac{1}{10}$. I am of opinion that I can distinctly prove the first wave to be negative, and that it certainly extended to Negapatam, thus showing that there must have been at first an enormous depression or subsidence at the bottom of the sea in the Straits of Sunda.

"By proper handling of the records I hope to deduce the velocity of the wave from Java to Aden, and from Java up the Bay of Bengal. . . . .

"Unfortunately it will take a long time to get the information about the time the wave was generated; but I am leaving no stone unturned to get it. I have sent about twenty circulars to various port officers, and I have asked —— to ask for information from Batavia."

SOUTH AFRICA

Some tidal stations have recently been established, on my recommendation, along the coast of South Africa, and the results obtained from the observations will be ultimately discussed. Meanwhile I inclose herewith a tracing from the tidal diagram at one of these stations (taken at Port Elizabeth under Mr. Shield, harbour engineer), covering the period from noon on August 26 to noon on August 30. It will be seen from the diagram that till 4 p.m. on August 27 the curve was perfectly normal.

At 8 p.m. on August 27 an extraordinary oscillation, having a period of about an hour, commenced, and at 9 p.m. had attained a range of five feet. It then gradually but very slowly subsides.

Mr. Shield thinks, and I agree with him, that the tidal disturbance in question has its origin probably in the recent disturbance in the Straits of Sunda. Our information here is as yet very defective. Accurate data as to that disturbance would be of the greatest interest. DAVID GILL

Royal Observatory, Cape of Good Hope, Sept. 25

The Tidal Wave, South Africa.

MAURITIUS

In the Mauritius *Mercantile Record* of September 8, Mr. C. Meldrum gives a collection of data on the phenomena observed in the neighbourhood of Mauritius. One of the local papers, the *Progrès Colonial*, gave, in its issue of August 29, an account of a curious phenomenon observed in the harbour of Port Louis, by Capt. Ferrat, of the s.s. *Touareg*. The *Touareg* was moored in the Trou Fanfaron, near the Patent Slip. Towards 2 p.m. on August 27 Capt. Ferrat, who was then on board of his vessel, observed that in that part of the harbour the sea

suddenly receded, leaving the rocks in the neighbourhood dry. The level of the water, it is said, fell to the extent of four or five feet. About a quarter of an hour after, the sea regained its former level with extreme violence, causing the *Touareg* and other vessels to roll frightfully. An alternate lowering and rising of the sea-level continued till 6 p.m. By 7 p.m. all had disappeared. On the morning of the 28th, however, there were still strong currents. In its issue of August 31, the same paper reports that, while traversing the pass between Round Island and the Coin de Mire, on the 27th, a Government boat, though running before a strong breeze, was stopped by a current from the opposite direction, and that the *patron* observed the sea receding precipitately from the vicinity of Gabriel Island, leaving the reefs dry, but that in a few minutes they were, by a sudden reflux, covered with water to the depth of six feet. The *Mercantile Record* of August 30 reported that on the 27th the sea in the Trou Fanfaron went down every twelve minutes, leaving all the boats moored in front of the harbour workshed dry, and then immediately rose again. The *Touareg* and *Stella* seemed to be in a boiling sea. On the 27th the sea in Tombeau Bay suddenly fell five feet below its usual level, and fish were caught on the dry beach. A quarter of an hour later the sea returned and rose nearly five feet above its ordinary level. Similar phenomena were observed at the Morne Brabant. On the same day, remarkable atmospheric and magnetic disturbances were recorded at the Royal Alfred Observatory, Pamplemousses. It would thus appear that from at least Flat Island, about eight miles north of the mainland of Mauritius, to Port Louis on the west coast, and thence round by the Morne to Souillac on the south coast, a distance in all of about forty-six miles, an unusual perturbation occurred with regard to the level and motion of the sea-water, and that on the same day meteorological and magnetic perturbations were recorded at the Observatory. The interest created by these occurrences was heightened by the report that vessels which arrived from the eastward on August 28 had passed through fields of pumice-stone.

Mr. Meldrum then gives a short account of what happened at the Observatory, and relates what has been told him by eye-witnesses of what occurred in the harbour and elsewhere.

1. *Barograms.*—The barogram sheet for the forty-eight hours ending at 8 a.m. on the 28th shows a remarkable disturbance in the atmospheric pressure between 11 a.m. and 5 p.m. on the 27th. Under ordinary conditions the barometer invariably falls from a maximum at about 9.30 a.m. to a minimum at about 3.30 p.m. But on August 27 last this was not the case. Soon after 11 a.m. a slight disturbance began, as indicated by small successive indentations in the barogram. At 11.55 a.m. the mercury stood at 29·996 inches, and at 0.06 p.m. at 29·918; it then rose to 29·961 at 0.20 p.m., after which it fell to 29·916 at 1.10 p.m. From 1.10 to 3.00 p.m. it rose, and at the latter hour stood at 29·968. In the interval from 2.50 to 3.55 p.m. there were five wavelets, and the mean interval between their lowest points was 16 minutes. Upon the whole, however, the mercury continued to rise after 1.10 p.m. The sudden fall from 11.55 a.m. to 0.06 p.m., and the rise from 0.06 to 0.20 p.m., are shown by a projecting peak. This peak, the undulations from 2.50 to 3.55 p.m., and the fact that the minimum occurred fully two hours earlier than usual, are the principal characteristics of the disturbance. After 5 p.m. there was no trace of disturbance.

A smaller disturbance occurred between 9 p.m. and midnight on the 28th.

2. *Magnetograms.*—Towards 9 a.m. on the 27th the north end of the declination magnet began to move towards the west, at first slowly and then more rapidly, and at 11 a.m. it attained its westerly maximum position, the movement since 9 a.m. amounting to 7′ 37″ of arc. An easterly movement then set in, and continued till 0h. 15m. p.m., the north end being then 13′ 18″ east of its position at 11 a.m. A slight westerly movement of 3′ 18″ then occurred up to 1h. 20m., after which there was a rapid movement towards the east till 2 p.m., the decrease in declination since 1h. 20m. being 10′ 37″. The magnet then moved towards the west, recovering its normal position about 5 p.m., and all traces of disturbance ceased. From 10 to 11 a.m., and especially from 11 a.m to 1 p.m., there were several minor oscillations. The extreme range from the maximum westerly position at 11 a.m. to the maximum easterly position at 2 p.m. was 20′ 43″.

The dip, or vertical force magnetometer, as indicated by the curve, shows traces of disturbance between 8h. 15m. and 11h. a.m. on the 27th. At the latter hour a rapid decrease of force began and continued till 0h. 15m. p.m., the decrease amounting in parts of force to ·00086, and the south end of the magnet moving upward through an angle of 16′ 07″. From 0h. 15m. to 1h. 20m. a slight increase of force took place, amounting to ·00027, the south end of the magnet dipping to the extent of 5′ 06″. After 1h. 20m. a very rapid decrease set in, which continued till 1.50 p.m., the decrease amounting to ·00083, and the south end of the magnet moving upward through an angle of 15′ 38″. The force then gradually increased and recovered its normal value at 6 p.m., by which time it had increased to the extent of ·00104 parts of force since 1h. 50m., the south end of the magnet moving downward through an angle of 19′ 36″. The total decrease from 11 a.m. to 1h. 50m. p.m. was ·00142, during which interval the range of angular movement was 26′ 40″. There were several minor oscillations, particularly between 11 a.m. and 0h. 40m. p.m.

The horizontal force curve also shows a well-marked disturbance, but it was less than in the case of the other curves.

The principal features of these disturbances were the unusually large ranges of the movements of the magnets, and the differences between the epochs of maximum and minimum and the average epochs.

Magnetic disturbances occurred also on August 28 and 29.

Disturbances in the Trou Fanfaron.—Capt. Ferrat states that at some time between 1.30 and 2 p.m. on the 27th the water rushed inwards from the harbour with great violence, and rose above its former level to the extent of fully three feet. An alternate ebb and flow then continued till nearly 7 p.m., the intervals in time between low and high water being about 15 minutes. There was no wave or billow, but a strong current, the estimated velocity of which was about three knots in ten minutes, or eighteen knots an hour. The current appeared to be strongest towards evening. Similar disturbances, but less marked, were observed on the morning of the 28th.

On the opposite side of the Trou Fanfaron another observer noticed, about 2 p.m. on the 27th, that around the *Stella*, which was moored within about 25 yards of him, the water had a "boiling appearance." The water then receded about 20 feet from the shore, leaving some boats near him partly on dry land. About a quarter of an hour after the water rushed back and advanced about six feet farther inland than where it was at 2 p.m. The water then receded, and a series of oscillations took place till about 6 p.m., the intervals between high and low water being from 15 to 20 minutes, and the extent of rise and fall which was at first about three feet, becoming less and less after 4 p.m.

These statements accord with others previously made to the Hon. Mr. Connal.

Similar phenomena, but of a less violent character, occurred between 2.30 and 6 p.m. on the 28th.

The Trou Fanfaron, it may be remarked, is a narrow inlet on the north-east side of the harbour. Near its mouth, or entrance, its direction is south-west and north-east; it then turns towards the east, and throughout the greater part of its length (about 1600 feet) it runs nearly east and west. Its breadth is generally from 200 to 300 feet.

Similar disturbances were observed at Arsenal and Tombeau Bays.

On August 11 and 12 the *Idomene*, in 6° to 8° S., and in 88 E., passed through fields of pumice-stone, which may have been ejected from a volcano near the Straits of Sunda. At all events, that pumice-stone had no immediate connection with what took place in Mauritius.

"I will at present," Mr. Meldrum concludes, "make no attempt to explain the phenomena in question. Magnetic disturbances, properly so-called, are not produced by earthquakes, but are generally ascribed, in a measure at least, to cosmical causes. The disturbances of the magnets on this occasion, however, may have been mechanical effects of earth-waves, although no permanent change of level took place. The difficulty is to refer all the phenomena to the same cause."

THE MOTION OF WATER[1]

1. *Objects and Results of the Investigation.*

THE results of this investigation have both a practical and a philosophical aspect.

[1] "An Experimental Investigation of the Circumstances which Determine whether the Motion of Water shall be Direct or Sinuous, and of the Law of Resistance in Parallel Channels." Abstract of Paper read at the Royal Society by Prof. Osborne Reynolds, F.R.S.

In their practical aspect they relate to the *law of resistance to the motion of water in pipes*, which appears in a new form, the law for all velocities and all diameters being represented by an equation of two terms.

In their philosophical aspect these results relate to the fundamental principles of fluid motion; inasmuch as they afford for the case of pipes a definite verification of two principles, which are *that the general character of the motion of fluids in contact with solid surfaces depends on the relation* (1) *between the dimensions of the space occupied by the fluid and a linear physical constant of the fluid;* (2) *between the velocity and a physical velocity constant of the fluid.*

The results as viewed in their philosophical aspect were the primary object of the investigation.

As regards the practical aspects of the results it is not necessary to say anything by way of introduction; but in order to render the philosophical scope and purpose of the investigation intelligible it is necessary to describe shortly the line of reasoning which determined the order of investigation.

2. *The Leading Features of the Motion of Actual Fluids.*—Although in most ways the exact manner in which water moves is difficult to perceive, and still more difficult to define, as are also the forces attending such motion, certain general features both of the forces and motions stand prominently forth as if to invite or defy theoretical treatment.

The relations between the resistance encountered by, and the velocity of a solid body moving steadily through, a fluid in which it is completely immersed, or of water moving through a tube, present themselves mostly in one or other of two simple forms. The resistance is generally proportional to the square of the velocity, and when this is not the case it takes a simpler form, and is proportional to the velocity.

Again, the internal motion of water assumes one or other of two broadly distinguishable forms—either the elements of the fluid follow one another along lines of motion which lead in the most direct manner to their destination, or they eddy about in sinuous paths, the most indirect possible.

3. *Connection between the Leading Features of Fluid Motion.*—These leading features of fluid motion are well known, and are supposed to be more or less connected, but it does not appear that hitherto any very determined efforts have been made to trace a definite connection between them, or to trace the characteristics of the circumstances under which they are usually presented.

Certain circumstances have been definitely associated with the particular laws of force. Resistance as the square of the velocity is associated with motion in tubes of more than capillary dimensions, and with the motion of the bodies through the water at more than insensibly small velocities, while resistance as the velocity is associated with capillary tubes and small velocities.

The equations of hydrodynamics, although they are applicable to *direct motion, i.e.* without eddies, and show that then the resistance is as the velocity, have hitherto thrown no light on the circumstances on which such motion depends. And although of late years these equations have been applied to the theory of the eddy, they have not been in the least applied to the motion of water, which is a mass of eddies, *i.e.* in *sinuous motion*, nor have they yielded a clue to the cause of resistance varying as the square of the velocity. Thus, while as applied to waves and the motion of water in capillary tubes the theoretical results agree with the experimental, the theory of hydrodynamics has so far failed to afford the slightest hint why it should explain these phenomena, and signally failed to explain the law of resistance encountered by large bodies moving at sensibly high velocities through water, or that of water in sensibly large pipes.

This accidental fitness of the theory to explain certain of the phenomena, while entirely failing to explain others, affords strong presumption that there are some fundamental principles of fluid motion of which due account has not been taken in the theory; and several years ago it seemed to me that a careful examination as to the connection between these four leading features, together with the circumstances on which they severally depend, was the most likely means of finding the clue to the principles overlooked.

4. *Space and Velocity.*—The definite association of resistance as the square of the velocity with sensibly large tubes and high velocities, and of resistance as the velocity with capillary tubes and slow velocities, seemed to be evidence of the very general and important influence of some properties of fluids not recognised in the theory of hydrodynamics.

As there is no such thing as absolute space or absolute time recognised in mechanical philosophy, to suppose that the character of motion of fluids in any way depended on absolute size or absolute velocity would be to suppose such motion outside the pale of the laws of motion. If, then, fluids, in their motions, are subject to these laws, what appears to be the dependence of the character of the motion on the absolute size of the tube and on the absolute velocity of the immersed body must in reality be a dependence on the size of the tube as compared with the size of some other object, and on the velocity of the body as compared with some other velocity. What is the standard object and what the standard velocity which come into comparison with the size of the tube and the velocity of an immersed body are questions to which the answers were not obvious. Answers, however, were found in the discovery of a circumstance on which sinuous motion depends.

5. *The Effect of Viscosity on the Character of Fluid Motion.*—The small evidence which clear water shows as to the existence of internal eddies, not less than the difficulty of estimating the viscous nature of the fluid, appears to have hitherto obscured the very important circumstance that *the more viscous a fluid is the less prone is it to eddying or sinuous motion.* To express this definitely, if μ is the viscosity and ρ the density of the fluid, for water $\frac{\mu}{\rho}$ diminishes rapidly as the temperature rises; thus at 5° C. $\frac{\mu}{\rho}$ is double what it is at 45° C. What I observed was that the tendency of water to eddy becomes much greater as the temperature rises.

Hence, connecting the change in the law of resistance with the birth and development of eddies, this discovery limited further search for the standard distance and standard velocity to the physical properties of the fluid.

To follow the line of this search would be to enter upon a molecular theory of liquids, and this is beyond my present purpose. It is sufficient here to notice the well-known fact that $\frac{\mu}{\rho}$ is a quantity of the nature of a product of a distance and a velocity.

6. *Evidence from the Equations of Motion.*—In this article it is pointed out that the equations of motion afford definite evidence of a dependence of the dynamical equilibrium of a fluid on the value of $\frac{c\rho U}{\mu}$, c being the diameter of the pipe and U the mean velocity of the fluid.

7. *The Cause of Eddies.*—There appeared to be two possible causes for the change of direct motion into sinuous. These are best discussed in the language of hydrodynamics; but as the results of this investigation relate to both these causes, which, although the distinction is subtile, are fundamentally distinct and lead to distinct results, it is necessary that they should be indicated.

The general cause of the change from steady to eddying motion was, in 1843, pointed out by Prof. Stokes as being that, under certain circumstances, the steady motion becomes unstable, so that an indefinitely small disturbance may lead to a change to sinuous motion. Both the causes above referred to are of this kind, and yet they are distinct; the distinction lying in the part taken in the instability by viscosity. If we imagine a fluid free from viscosity and absolutely free to glide over solid surfaces, then comparing such a fluid with a viscous fluid in exactly the same motion—

(1.) The frictionless fluid might be unstable and the viscous stable. Under these circumstances the cause of eddies is the instability as a perfect fluid, the effect of viscosity being in the direction of stability.

(2.) The frictionless fluid might be stable, and the viscous fluid unstable; under which circumstances the cause of instability would be the viscosity.

It was clear to me that the conclusion I had drawn from the equations of motion immediately related only to the first cause. Nor could I then perceive any possible way in which instability could result from viscosity. All the same I felt a certain amount of uncertainty in assuming the first cause of instability to be general. This uncertainty was the result of various considerations, but particularly from my having observed that eddies apparently come on in very different ways, according to a very definite circumstance of motion, which may be illustrated.

When in a channel the water is all moving in the same direction, the velocity being greatest in the middle, and diminishing to zero at the sid , as indicated by the curve in Fig. 1, eddies

showed themselves reluctantly and irregularly; whereas when the water on one side of the channel was moving in the opposite direction to that on the other, as shown by the curve in Fig. 2, eddies appeared in the middle regularly and readily.

8. *Methods of Investigation.*—There appeared to be two ways of proceeding, the one theoretical, the other practical.

The theoretical method involved the integration of equations for unsteady motion in a way that had not then been accomplished, and which, considering the general intractability of the equations, was not promising.

The practical method was to test the relation between U, $\frac{\mu}{\rho}$, and c; this, owing to the simple and definite form of the law, seemed to offer, at all events in the first place, a far more promising field of research.

The law of motion in a straight, smooth tube offered the simplest possible circumstances and the most crucial test.

The existing experimental knowledge of the resistance of

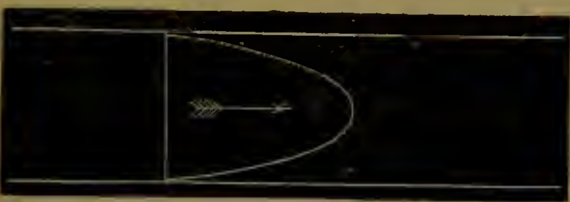


Fig. 1.

water in tubes, although very extensive, was in one important respect incomplete. The previous experiments might be divided into two classes—(1) those made under circumstances in which the law of resistance was as the square of the velocity, and (2) those made under circumstances in which the resistance varied as the velocity. There had not apparently been any attempt made to determine the exact circumstances under which the change of law took place.

Again, although it had been definitely pointed out that eddies would explain the resistance as the square of the velocity, it did not appear that any definite experimental evidence of the existence of eddies in parallel tubes had been obtained, and much less was there any evidence as to whether the birth of eddies was simultaneous with the change in the law of resistance.

These open points may be best expressed in the form of queries to which the answers anticipated were in the affirmative.

(1.) What was the exact relation between the diameters of the pipes and the velocities of the water at which the law of resistance changed: was it at a certain value of c U?

Fig. 2.

(2.) Did this change depend on the temperature, *i.e.* the viscosity of water; was it at a certain value of $\frac{\text{U}}{\mu}$?

(3.) Were there eddies in parallel tubes?

(4.) Did steady motion hold up to a critical value and then eddies come in?

(5.) Did the eddies come in at a certain value of $\frac{\rho c \text{U}}{\mu}$?

(6.) Did the eddies first make their appearance as small, and then increase gradually with the velocity, or did they come in suddenly?

The bearing of the last query may not be obvious; but, as will appear in the sequel, its importance was such that, in spite of satisfactory answers to all the other queries, a negative answer to this in respect of one particular class of motions led to the reconsideration of the supposed cause of instability, and eventually to the discovery of instability caused by fluid friction.

The queries as they are put suggest two methods of experimenting:—

(1.) Measuring the resistances and velocities for different diameters, and with different temperatures of water.

(2.) Visual observation as to the appearance of eddies during the flow of water along tubes or open channels.

Both these methods have been adopted, but as the question relating to eddies had been the least studied, the second method was the first adopted.

9. *Experiments by Visual Observations.*—The most important of these experiments related to water moving in one direction along glass tubes. Besides these, however, experiments on fluids flowing in opposite directions in the same tube were made; also a third class of experiments which related to motion in a flat channel of indefinite breadth.

These last-mentioned experiments resulted from an incidental observation during some experiments made in 1876 as to the effect of oil to prevent wind waves. As the result of this observation had no small influence in directing the course of this investigation, it may be well to describe it first.

10. *Eddies caused by the Wind beneath the Oiled Surface of Water.*—A few drops of oil on the windward side of a pond during a stiff breeze having spread over the pond and completely calmed the surface as regards waves, the sheet of oil, if it may be so called, was observed to drift before the wind, and it was then particularly noticed that close to, and at a considerable distance from, the windward edge, the surface presented the appearance of *plate glass*; further from the edge the surface presented that wavering appearance which has already been likened to that of sheet glass, which appearance was at the time noted as showing the existence of eddies beneath the surface.

Subsequent observation confirmed this first view. At a sufficient distance from the windward edge of an oil-calmed surface there are always eddies beneath the surface even when the wind is light. But the distance from the edge increases rapidly as the force of the wind diminishes, so that at a limited distance (10 or 20 feet) the eddies will come and go with the wind.

Without oil I was unable to perceive any indication of eddies. At first I thought that the waves might prevent their appearance even if they were there, but by careful observation I convinced myself that they were not there. It is not necessary to discuss these results here, although, as will appear, they have a very important bearing on the cause of instability.

11. *Experiments by Means of Colour Bands in Glass Tubes.*—These were undertaken early in 1880; the final experiments were made on three tubes, Nos. 1, 2, and 3.

The diameters of these were nearly 1 inch, ½ inch, and ¼ inch. They were all about 4 feet 6 inches long, and fitted with trumpet mouthpieces, so that water might enter without disturbance.

The water was drawn through the tubes out of a large glass tank in which the tubes were immersed, arrangements being made so that a streak or streaks of highly-coloured water entered the tubes with the clear water.

The general results were as follows:—

(1.) When the velocities were sufficiently low, the streak of colour extended in a beautiful straight line through the tube (Fig. 3).

(2.) If the water in the tank had not quite settled to rest, at sufficiently low velocities the streak would shift about the tube, but there was no appearance of sinuosity.

(3.) As the velocity was increased by small stages at some point in the tube always at a considerable distance from the trumpet or intake, the colour band would all at once mix up with the surrounding water, and fill the rest of the tube with a mass of coloured water, as in Fig. 4.

Any increase in the velocity caused the point of breakdown to approach the trumpet, but with no velocities that were tried did it reach this.

On viewing the tube by the light of an electric spark, the mass of colours resolved itself into a mass of more or less distinct curls showing eddies, as in Fig. 5.

The experiments thus seemed to settle questions 3 and 4 in the affirmative—the existence of eddies and a critical velocity.

They also settled in the negative question 6 as to the eddies coming in gradually after the critical velocity was reached.

In order to obtain an answer to question 5 as to the law of the critical velocity, the diameters of the tubes were carefully measured, also the temperature of the water and the rate of discharge.

(4.) It was then found that with water at a constant temperature and the tank as still as could by any means be brought

about, the critical velocities at which the eddies showed themselves were exactly in the inverse ratios of the diameters of the tubes.

(5.) That in all the tubes the critical velocity diminished as the temperature increased, the range being from 5° C. to 22° C., and the law of this diminution, so far as could be determined, was in accordance with Poiseuille's experiments.

Taking T to express degrees Centigrade, then by Poiseuille's experiments—

$$\frac{\mu}{\rho} \propto P = 1 + 0{\cdot}0336\,T + 0{\cdot}00221\,T^2,$$

Taking a metre as the unit, U_s the critical velocity, and D the diameter of the tube, the law of the critical point is completely expressed by the formula $U_s = \frac{1}{B_c}\frac{P}{D}$, where $B_s = 43{\cdot}7$. This is a complete answer to question 5.

During the experiments many things were noticed which cannot be mentioned here, but two circumstances should be mentioned as emphasising the negative answer to question 6. In the first place, the critical velocity was much higher than had been expected in pipes of such magnitude, resistance varying as the square of the velocity had been found at very much smaller velocities than those at which the eddies appeared when the water in the tank was steady. And in the second place it was observed that the critical velocity was very sensitive to disturbance in the water before entering the tubes, and it was only by the greatest care as to the uniformity of the temperature of the tank and the stillness of the water that consistent results were obtained. This showed that the steady motion was unstable for large disturbances long before the critical velocity was reached, a fact which agreed with the full blown manner in which the eddies appeared.

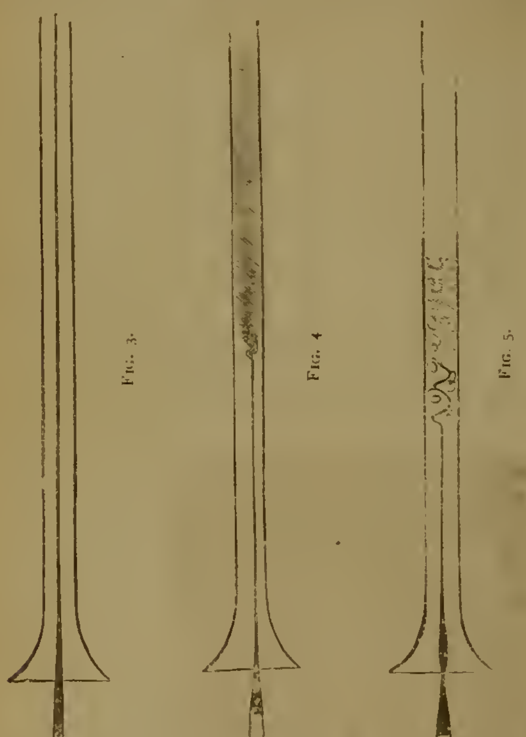


FIG. 3. FIG. 4. FIG. 5.

12. *Experiments with Two Streams in Opposite Directions in the Same Tube.*—A glass tube 5 feet long and 1·2 inch in diameter, having its ends slightly bent up, as shown in Fig. 6, was half filled with bisulphide of carbon, and then filled up with water and both ends corked. The bisulphide was chosen as being a limpid liquid, but little heavier than water and completely insoluble, the surface between the two liquids being clearly distinguishable. When the tube was placed in a horizontal direction, the weight of the bisulphide caused it to spread along the lower half of the tube, and the surface of separation of the two liquids extended along the axis of the tube.

On one end of the tube being slightly raised, the water would flow to the upper end, and the bisulphide fall to the lower, causing opposite currents along the upper and lower halves of the tube, while in the middle of the tube the level of the surface of separation remained unaltered.

The particular purpose of this investigation was to ascertain whether there was a critical velocity at which waves or sinuosities would show themselves in the surface of separation. It proved a very pretty experiment and completely answered its purpose.

When one end was raised quickly by a definite amount, the opposite velocities of the two liquids, which were greatest in the middle of the tube, attained a certain maximum value depending on the inclination given to the tube. When this was small no signs of eddies or sinuosities showed themselves, but at a certain definite inclination waves (nearly stationary) showed themselves, presenting all the appearance of wind waves.

These waves first made their appearance as very small waves of equal lengths, the length being comparable to the diameter of the tube.

When by increasing the rise the velocities of flow were increased, the waves kept the same length but became higher, and when the rise was sufficient the waves would curl and break, the one fluid winding itself into the other in regular eddies.

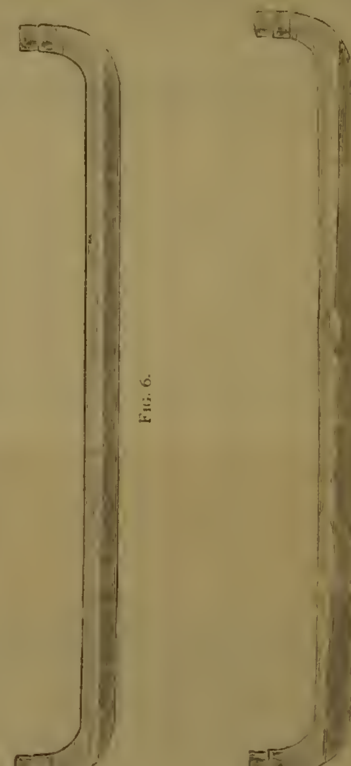


FIG. 6. FIG. 7.

Whatever might be the cause, a skin formed slowly between the bisulphide and the water, and this skin produced similar effects to that of oil on water; the results mentioned are those which were obtained before the skin showed itself. When the skin first came on, regular waves ceased to form, and in their place the surface was disturbed as if by irregular eddies above and below, just as in the case of the oiled surface of water.

The experiment was not adapted to afford a definite measure of the velocities at which the various phenomena occurred, but it was obvious that the critical velocity at which the waves first appeared was many times smaller than the critical velocity in a tube of the same size when the motion was in one direction only. It was also clear that the critical velocity was nearly if not quite independent of any existing disturbance in the liquids. So that this experiment shows—

(1.) That there is a critical velocity, in the case of opposite flow, at which direct motion becomes unstable.

(2.) That the instability came on gradually and did not depend on the magnitude of the disturbances, or, in other words, that

for this class of motion question 6 must be answered in the affirmative.

It thus appeared that there was some difference in the cause of instability in the two motions.

13. *Further Study of the Equations of Motion.*—Here the author explains that he had so far succeeded in integrating the equations of motion as to find that there must be two critical values of the velocity—the one that at which steady motion would break down into eddying motion, the other that at which, as the velocity diminished, previously existing eddies would die out; both these velocities depending on the relation $U \propto \frac{\mu}{\rho c}$.

14. *Results of Experiments on the Law of Resistance in Tubes.*—The existence of the critical velocity described in the previous article could only be tested by allowing water in a high state of disturbance to enter a tube, and after flowing a sufficient distance for the eddies to die out, if they were going to die out, to test the motion. As it seemed impossible to apply the method of colour bands, the test applied was that of the law of resistance as indicated in questions (1) and (2) in § 8. The result was very happy. Two straight lead pipes, No. 4 and No. 5, each 16 feet long, and having diameters of a quarter and half inch respectively, were used.

The water was allowed to flow through rather more than 10 feet before coming to the first gauge-hole, the second gauge-hole being 5 feet further along the pipe.

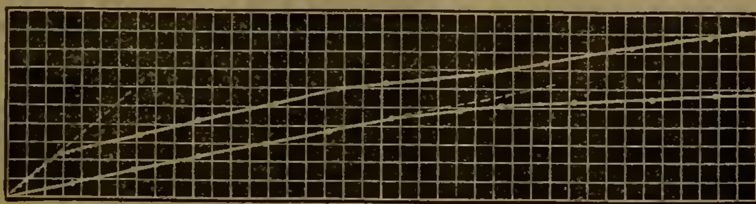


FIG. 8.

The results were very definite, and are partly shown in Fig. 8.

(1.) At the lower velocities the pressure was proportional to the velocity, and the velocities at which a deviation from this law first occurred were in the exact inverse ratio of the diameters of the pipes.

(2.) Up to these critical velocities the discharges from the pipes agreed exactly with those given by Poiseuille's formula for capillary tubes.

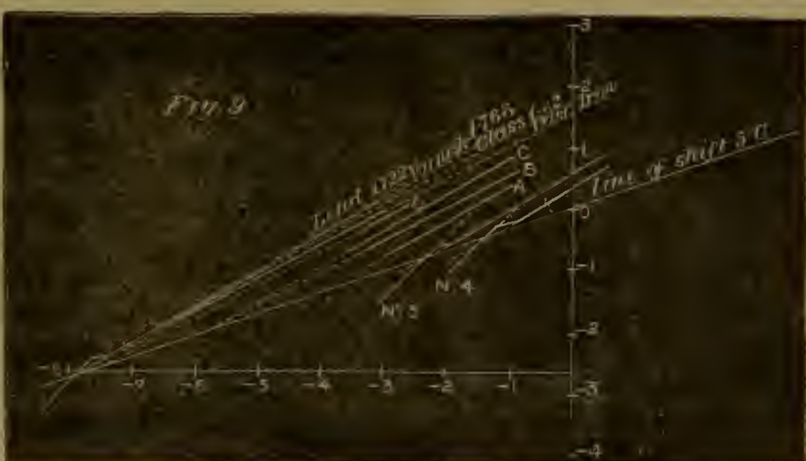
(3.) For some little distance after passing the critical velocity no very simple relations appeared to hold between the pressures and velocities; but by the time the velocity reached 1·3 (critical velocity) the relation became again simple. The pressure did not vary as the square of the velocity, but as 1·722 power of the velocity; this law held in both tubes, and through velocities ranging from 1 to 50, where it showed no signs of breaking down.

(4.) The most striking result was that not only at the critical velocity but throughout the entire motion the laws of resistance exactly corresponded for velocities in the ratio of $\frac{\mu}{\rho c}$. This last result was brought out in the most striking manner on reducing the results by the graphic method of logarithmic homologues as described in my paper on thermal transpiration.

Calling the resistance per unit of length as measured in the weight of cubic units of water i, and the velocity v, log i is taken for abscissa, and log v for ordinate, and the curve plotted.

In this way the experimental results for each tube are represented as a curve; these curves, which are shown as far as the small scale will admit in Fig. 9, present exactly the same shape, and only differ in position.

Either of the curves may be brought into exact coincidence with the other by a rectangular shift, and the horizontal shifts are



Pipe No. 4, Lead ... ... 0·00615 m. diameter
,, ,, 5, ,, ... 0·0127 ,,
,, A, Glass ... ... 0·0496 ,,
Pipe B, Cast Iron ... ... 0·188 m. diameter
,, D, ,, ... 0·5 ,,
,, C, Varnish ... ... 0·196 ,,

given by the difference of the logarithm of $\frac{D^3}{\mu^2}$ for the two tubes, the vertical shifts by the difference of the logarithm of $\frac{D}{\mu}$.

The temperatures at which the experiments had been made were nearly the same, but not quite, so that the effect of the variations of μ showed themselves.

15. *Comparison with Darcy's Experiments.*—The definiteness of these results, their agreement with Poiseuille's law, and the new form which they more than indicated for the law of resistance above the critical velocity, led me to compare them with the well-known experiments of Darcy on pipes ranging from 0·014 to 0·5 metre. Taking no notice of the empirical laws by which Darcy had endeavoured to represent his results, I had the logarithmic homologues plotted from his published experiments. If my law was general, then these log. curves, together with mine, should all shift into coincidence if each were shifted horizontally through $\frac{D^3}{P^2}$, and vertically through $\frac{D}{P}$.

In calculating these shifts there were some doubtful points.

Darcy's pipes were not uniform between the gauge points, the sections varying as much as 20 per cent., and the temperature was only casually given. These matters rendered a close agreement unlikely; it was rather a question of seeing if there was any systematic disagreement. When the curves came to be shifted, the agreement was remarkable; in only one respect was there any systematic disagreement, and this only raised another point; it was only in the slopes of the higher portions of the curves. In both my tubes the slopes were as 1·722 to 1; in Darcy's they varied according to the nature of the material, from the lead pipes, which were the same as mine, to 1·92 to 1 with the cast iron. This seems to show that the nature of the surface of the pipe has an effect on the law of resistance above the critical velocity.

16. *The Critical Velocities.*—All the experiments agreed in giving $v_c = \frac{1}{278}\frac{P}{D}$ as the critical velocity, to which correspond as the critical pressure $i_c = \frac{1}{47700000}\frac{P^2}{D^3}$, the units being metres and degrees Centigrade. It will be observed that this value is much less than the critical velocity at which steady motion broke down.

17. *General Law of Resistance.*—The log. homologues all consist of two straight branches, the lower branch inclined at 45°, and the upper one at n horizontal to 1 vertical, except for the small distance beyond the critical velocity these branches constitute the curves. These two branches meet in a point, O, on the curve at a definite distance below the critical pressure, so that, ignoring the small portion of the curve above the point before it again coincides with the upper branch, the logarithmic homologues give for the law of resistance for all pipes and all velocities $A\frac{D^3}{P^2}i = \left(B\frac{D}{P}v\right)^n$, where n has the value unity as long as either member is below unity, and then takes the value of the slope n to 1 for the particular surface of the pipe.

If the units are metres and degrees Centigrade—

$$A = 67{,}700{,}000$$
$$B = 398$$
$$P = 1 + 0{\cdot}0336\,T + 0{\cdot}000221\,T^2.$$

This equation then, excluding the region immediately about the critical velocity, gives the law of resistance in Poiseuille's tubes, those of the present investigation, and Darcy's, the range of diameters being from 0·000013 metres (Poiseuille, 1843), to 0·5 metres (Darcy, 1857); and the range of velocities from 0·0026 to 7 metres per sec., 1883.

This algebraical formula shews that the experiments entirely accord with the theoretical conclusions. The empirical constants are A, B, P, and n; the first three relate solely to the dimensional properties of the fluid which enter into the viscosity, and it seems probable that the last relates to the properties of the surface of the pipe.

Much of the success of the experiments is due to the care and skill of Mr. Foster of Owens College, who has constructed the apparatus and assisted me in making the experiments.

SOCIETIES AND ACADEMIES

Paris

Academy of Sciences, October 15.—M. Blanchard, president, in the chair.—Note on a formula of Hansen in connection with the mechanism of the heavens, by M. F. Tisserand.—On the measurement of the forces brought into play in the various actions of human locomotion (continued), three illustrations, by M. Marey. By combining the indications obtained from the dynamometer with those yielded by instantaneous photography, a continuous comparison may be made of the forces brought into action with the movements resulting from them. The various applications of these two methods will form the subject of future experiments.—On a memoir by M. Raoult, entitled: "Loi générale de Congélation des Dissolvants,"—report by MM. Cahours, Berthelot, and Debray. Water holding saline bodies in solution freezes at a lower temperature than pure water, and the English physicist Blagden had shown in 1788 that the lowering of the freezing-point due to this cause is in many cases in proportion to the quantity of matter held in solution. This principle is now generalised by M. Raoult, who arrives at the conclusion that the freezing-point of any liquid compounds capable of solidification is lowered by all solid, fluid, or gaseous bodies dissolved in them. The reporters agree with the author that his methods will be found useful in supplying new means for ascertaining by a simple process the degree of purity of given substances.—Trial trip of an electric screw balloon made by MM. A. and G. Tissandier, note by M. G. Tissandier. This preliminary experiment took place at Auteuil on October 8, and was attended by a certain measure of success, although the apparatus proved powerless to prevent the spinning motion of the balloon when heading against aërial currents. The trip will be renewed as soon as certain improvements have been made in the electromotor suggested by this experiment.—Studies made on the summit of the Pic du Midi, with a view to the establishment of a permanent astronomic station, note by MM. Thollon and Trépied.—On the transformation of certain equations of the second degree to two independent variables, and on some integrations thence deducible, by M. R. Liouville.—On a method of isolating the calorific from the luminous and chemical rays, by M. F. van Assche.—On the form and characters of the reflex muscular contraction, by M. H. Beaunis.—On the resisting power of a ring whose outer surface supports a normal pressure constant as to unity of length of its mean axis, by M. J. Boussinesq.—On surfaces whose total curve is constant, by M. G. Darboux.—Indices of refraction of fluate of lime for the rays of different wave-lengths as far as the extreme ultra-violet, by M. Ed. Sarasin.—Note on a new method of insulating the metallic wires used in telegraphy and telephony, by M. C. Widemann.—Note on the determination of the equivalents of metals by means of their sulphates, by M. H. Baubigny.—On the process at present employed to determine the glucose in cane-sugar, by M. P. Lagrange. The object of this paper is to show that the quantitative analysis of glucose, made on a liquor whether treated or not with subacetate of lead, is liable to serious errors.—Analysis of a specimen of guano from the Cape Verde Islands, by M. A. Andouard.—Zoological dredgings and thermometric soundings in the lakes of Savoy, by M. F. A. Forel.—On the organisation of the *Spadella Marioni*, a new species from the Gulf of Marseilles, by M. P. Gourret.—On some peculiarities in the structure of Tunicata, by M. L. Roule.—Fresh studies on the fossil ruminants of Auvergne, by M. Depéret.—On the treatment of strabismus by means of the capsular "advancement," by M. L. de Wecker.—On the part played by the ligneous vessels in the upward movement of the sap, by M. J. Vesque.—Note on a lunar mirage observed on the night of October 11, by M. Virlet d'Aoust.

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